The social survey has come to be a familiar type of sociological research, one immediately invoked for solving new and unfamiliar research problems. We find that Makerere's UNICEF - FAO Rural Extension Project has listed as one of its specific objectives, "to organise surveys" which will ascertain "the needs and interests of farmers" throughout East Africa. The information gained thereby can tell the government departments concerned what is the general state of rural farm family welfare, and what are the factors which underlie the emergence of progressive farming. I have recently been planning the first of such "Farm Innovation" surveys, and this paper explores the methodological considerations which have arisen. Particularly, we will examine whether or not the traditional social survey rationale, that of hypothesis testing, is suited to a situation where one must operate without preliminary knowledge. I shall argue that the procedures for finding ideas are, in fact, rather different from those necessary for testing ideas. This distinction has important implications for the design of social survey research.

Traditionally, the exponents of social survey research have stressed the unity of rationale linking social science with natural science: "The formal features of survey analysis are no different at an abstract level from the procedures of more traditional scientific work." The survey is thought of as an analog to the natural scientist's laboratory experiment. It is (to quote but two of many like explications), "a way of organising the collection of evidence that a hypothesis may be tested," or, "the proof of an hypothesis which seeks to hook up two factors into a causal relationship through the study of contrasting situations which have been controlled on all factors except the one of interest." From this standpoint, the function of a survey is the same as that of the experiment: by its outcome, each survey either confirms or disproves the one or more hypotheses which would have predicted alternative outcomes. The purpose of a social survey is, then, to prove inferences.

In its most simple form, the received "research design" paradigm is as follows. The scientist identifies a problem, about which he has ideas. He translates these into analytical language as hypotheses, relating in some way to previous knowledge labelled theory. He devises methods for putting his hypotheses to operational test within an experimental situation. The record of an experiment or a survey becomes raw data which are then processed according to the set operations to yield results. These either confirm or reject the original hypotheses, and must be interpreted to confirm or revise the general body of theory from which they were ultimately derived. The test stage is midway in the process, and it is here that both the experimental (in natural science) and the social survey (in social science) occur. They are alternative avenues for validating predictable relationships; they differ only in the methods they employ.

Consequently—following upon these premises—the discussion of research methodology has usually been confined to examining its role in testing already developed theoretical inferences. Our working procedures in many branches of science are based upon the supposition that theory grows through the accumulation of trial results. Methods are important in conducting the trial itself, but the theory antedates the trial and so is independent of research methodology. Accordingly, you have a theory, but you choose a methodology.
Interestingly enough, one finds that the natural scientists are growing rather disenchanted with the foregoing model of the research process. To be sure, it can still be found in most introductory texts. But at the plane of more sophisticated discussion, a number of fundamental criticisms have been raised:

1. The model ignores developmental aspects in a research tradition. Is there a phase structure to the development of a theoretical tradition? And if so, are the same procedures necessarily relevant at different stages, or do they change as a tradition matures? It would seem that the elaborate precautions which are characteristic of a developed experimental tradition may be prematurely confining to one already in the act of "getting off ground," so to speak. In its "infancy" a tradition goes through a phase when important discoveries and ramifications are a daily occurrence, and when there is a rapid grandissation of new insights. To demand experimental tests at the outset of a new line of inquiry may actually inhibit its acquiring the necessary coherence to achieve any sort of explanatory power.

2. The model makes no provision for continuing feedback between the test and the observation. According to the model, feedback occurs after the test has been completed and the results analyzed. Then, and only then, is the theory modified to accommodate to the operational test of hypotheses. In practice, however, two aspects are even more important: tests are generally expensive to conduct, and in many procedures negative results may become manifest in an early stage. New insight often accompanies the administration of experimental procedures, so that the relationship between test and concepts is cyclical, not linear. The most rapid progress occurs when the observer is free to modify tests even as they are being run—perhaps through assigning others to carry out new tests as they come to be suggested out of current work. The research design ought to contain provision for sequential tests to accommodate to one's expanding ideas as research progresses.

3. A one-to-one relationship between concept and test is inefficient. Because a test is cumbersome and difficult (in many cases), it is not practicable to furnish experimental proof for each set of hypotheses used in developing a theory. In place of the simplified sequence of theory-hypothesis-test-refined theory one needs a chain of interlinked hypotheses which can be tested as a totality in a crucial experiment. Even better, one would like to have "strong inference" to design tests which will discriminate between several mutually exclusive chains of testable hypotheses. In this model, thinking takes higher priority: one postpones "tests" until one has worked out a number of alternative launch pads to operate from, and, once begun, each successive test in the sequence must discriminate between several alternatives.

4. The model restricts observation to the test phase. It assumes that the place and time for observation coincides with the test itself. But this ignores the observations which lead to the refinement and operationalizing of the initial hypotheses: it presupposes that the investigator has considerable prior information available to him, either from his own experience or from the storehouse of an already developed tradition. In the opening of a new field on which there is no preliminary information, one's techniques for arriving at hypotheses become more important, and one requires initial observation before framing operational tests. The paradigm then becomes one of general test—operationalized hypothesis—crucial test, in which case methods for deriving applicable hypotheses become of equal importance to those for testing hypotheses.

Points on this order—and there are many more—have turned natural scientists to the question of efficiency in the research process. Which combination of techniques are optimal for which purposes under what circumstances? The scientist is also a person. And, as such, there are definite constraints to what he can give to a research situation. Furthermore, these constraints are operative throughout the research process. Consequently, the "climate" of research at each stage may be just as important as its "design" in influencing the outcome. Methodology becomes relevant to all stages in the process, and different situations dictate different research paradigms. We do not abandon the need for proofs, but we recognize that they may come at different junctures within the sequence of events which evolve into a research tradition. Especially at those stages in research which require major inputs of creativity (which depends most directly upon a scientist's personality), one would expect to find a divergence between the methods useful for generating hypotheses and those suited to proving them.

Let us return to the problem of social survey design. Standard texts contain no hint of the kinds of complications referred to above, which are of concern currently in the natural sciences. The stages in survey design seem to be clear cut; they stem directly from the aforementioned ideal for natural science experiments. We will examine two examples briefly. The first comes from Hyman's Survey Design and Analysis:

<table>
<thead>
<tr>
<th>1</th>
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<tbody>
<tr>
<td>Has a Problem</td>
</tr>
<tr>
<td>Communicates Problem</td>
</tr>
<tr>
<td>Communicates Treated Data &amp; Answer</td>
</tr>
<tr>
<td>Selects Course of Action to Solve Problem</td>
</tr>
</tbody>
</table>

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<tr>
<th>2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Operational Translation</td>
</tr>
<tr>
<td>Designs Research</td>
</tr>
<tr>
<td>Formulates Problem</td>
</tr>
<tr>
<td>Selects an Answer Treatment of Data</td>
</tr>
<tr>
<td>Transmits Data</td>
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</tbody>
</table>

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<tr>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Moves into Environment With Respondent</td>
</tr>
<tr>
<td>Records Response</td>
</tr>
<tr>
<td>Interviews Stimulant</td>
</tr>
<tr>
<td>Responds</td>
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</tbody>
</table>

Or consider the ten stages in small-scale surveys (given by Morris):

1. The general problem to be studied,
2. A review of previous work on the problem, which permits it to be stated more definitely,
3. Questions or hypotheses guiding the survey,
4. The selection of suitable techniques of investigation,
5. The selection of the people to be surveyed,
6. Testing of the survey techniques in a 'pilot survey' or trial run,
7. The collection of information,
8. The preparation of this information so that the initial questions may be answered or the hypotheses tested,
9. The interpretation of the survey findings,
10. The writing of the survey report.

1Slightly modified from horizontal to vertical format, p. 45.
2In, Humphrey & Argyle, eds., 1962, pp. 181 - 203.
At first glance either outline appears to be an admirable guide for the ideal research project. Certainly, this kind of model has come into vogue with the research foundations and overseas agencies in their consideration of research applications. But is it a realistic model for East African conditions, however relevant elsewhere?

To my knowledge there have been two large-scale rural surveys on social life within East Africa in recent years. The first, a "Culture and Ecology in East Africa Project," was carried out under the direction of the anthropologist Walter Colichard from the University of California at Los Angeles. The project had very substantial backing from the National Science Foundation in the USA; its goals were to trace the impact upon religious beliefs and structure of the change over from pastoral to agricultural ways of life in a select sample of East African societies. The project was distinctive—at its initial stages, in any case—by its ambitious attempt to set out a series of working hypotheses relating subsistence patterns to religion. The final results of the project are, however, puzzling. The papers so far published contain little reference to the original hypotheses; and, in fact, one gains the distinct impression that it is hard to demonstrate any significant relationships between the specifics of subsistence and the minutiae of religious organization. To date the project seems to be a classic case of a grand research design based upon theory which turned out to be of low explanatory power in the very region to which it was supposed to apply. This happens often enough in survey research, but it is embarrassing in a project that costs nearly a quarter of a million dollars and involves so many senior researchers.

The second project, an AID "Baseline Survey" carried out by E.I. Hayes and Marcus Surveys in Kenya, is just nearing completion. Preliminary survey results are available in a recent EMIHR Conference Paper, and in separate publication. The survey was carried out in three locations of Kenya, on a total sample of 676 farm household heads. The goal of the survey was to establish a "baseline" of information on characteristics of rural life, covering as many parameters (physical, economic, educational, sociological) as possible. In many respects the survey is a model case study in the application of standard social survey design; it closely approximates the stages we have already presented.

Yet the results seem to be peculiarly unifying, considering the exemplary design used in their derivation. Results tended to be of the order of "more family heads with large cash incomes were more firm in their belief that they could borrow money from their county councils..." "More agricultural officers... visited the farms of the more affluent farmers," etc. The statements about relationships uncovered strike one as being valid—yet, at the same time, they are unproducible. They do not point to the underlying dynamics of social behavior; rather, they present an impenetrable facade of descriptive attributes, somehow both true and yet misleading.

After reading the "Baseline Survey" results, I turned back to several standard descriptive surveys from the inter-war period in the USA and after. If one reads these with care, a similar phenomenon emerges. The descriptive generalizations are themselves the most bland part of the research report. In contrast, interwoven through the average report is a running commentary justifying certain questions and explaining unusual features in the final results. Stripped of its conjecture, the conventional descriptive survey is conceptually impoverished. Because both investigator and reader share the culture being investigated, the reader does not object to being led

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1Not including several large-scale nutritional and economic surveys.
down the primrose path so long as the departure is legitimized with the suitable protestations of "life" and "truth." To rationalize this intersection of conceptual complexity at the interfaces of the report, the writer generally trumpets "further studies" on this or that point at some time in the vague future. At the extreme one finds some sociologists—who had best go un-named at this juncture—whose facts are "all wrong" but who nevertheless contribute enormously productive ideas to the general dialogue.

I am, in fact, implying two things; and, by extrapolation, a third: (1) the best of one's operational hypotheses about an East African problem, derived from the literature alone, are apt to depart sufficiently from the realities "on the ground" to be useless for experimental design from afar; yes (2) valid! Descriptive generalizations derived locally, if divorced from their cultural context and from more powerful analytical theories, lose their research value no matter how soundly they are based. Thus, (3) either the East African situation contains special dangers which make the usual techniques less efficient, or the standard survey design is itself not self-sufficient in the stages it sets forth. In the two sections following I will suggest that both possibilities are true in part: field research in Africa does present special difficulties for survey design; but the design itself—as traditionally given—has certain inadequacies.

II

What are, then, the special characteristics of research in East Africa which might affect the administration and success of a social survey? (I speak here principally of research carried out by ex-patriates, whose stay in the country is determined by their research.)

1 The research time of the senior investigator is very expensive. It is expensive personally, in that the investigator in the field forgets his normal professional advancement and contacts, his access to documentation and the recent literature in his field, and sometimes even at some of his personal life with his family. It is expensive financially in overseas air tickets and the necessity of maintaining three bases of operations: one at "home," one at the foreign metropolis, and one "in the field." The investigator may arrive already committed to a larger topic than he would have dared to adopt overseas—"The Economic Development of Uganda," for instance—whereas his research time is usually fixed in advance of his preliminary reconnaissance.

2 The investigator spends more time on non-research tasks. Paradoxically to the above, the organization of research in a strange country where logistics are difficult and secretarial help rare commits one to doing a great deal of the routine, non-research administration personally. There are weeks wasted in getting visas, letters of introduction, housing, transport, etc. which are seldom anticipated in the research design schedule. Hiring assistants only exacerbates the difficulties, since one must then provide them with the amenities as well.

3 The investigator cannot communicate directly with the objects of his study. The "language problem" applies to all short-term social surveys and research projects. Inevitably, the investigator must become dependent upon intermediaries for all his communication at the local level, save those with the educated elite. Most social situations become small group interactions between himself, his assistant(s), and "the people." He cannot rely upon chance communication to tell him when his assistants have misunderstood him, or to warn when someone is staging a reply. His role is continuously being affected by that of his intermediaries; where they err, he errs.
The investigator's role is always a dominant one in dealing with rural residents. The previous colonial and bureaucratic history of East Africa makes it unavoidable that the expatriate will be given a dominant role in most social interactions. The status is ascribed by race and tradition; there is nothing he can do to achieve a servient or egalitarian position. Consequently, many of his initial social contacts outside of the elite will partake of the aura of patron-client relationships. The expatriate's role automatically connotes colonial norms, the demands which were so long pressed by missionaries, settlers, and administrations for "standards," "proper behaviour," "service," etc. On a rational grounds, therefore, one can expect continuous distortion in all statements made by local people directly to the senior investigator.

The activity of integrative questioning is automatically suspect. People do not allow the stranger the role of asking personal questions. Most East African peoples (if not all) have traditional proverbs which warn against telling private family affairs to outsiders. Yet etiquette demands fitness in one's style of response to strangers, so that the one questioned would rather answer obliquely than withdraw into taciturn silence. In many areas questioning in the past has always been the prelude to some sort of governmental activity, often seemingly prejudicial to local interests.

The investigator is more prone to think in polar categories. In his own society, the investigator intuitively corrects his perceptions to accord with his prior knowledge of the parameters of roles and groups. At home, for instance, he would never take the opinion of a rural, retired "farmer" aged seventy-five, sitting in the back porch of a daughter's house, as representing "the American viewpoint." Overseas, however, he easily slips into thinking about "The Balanda" or "The Kimuyu." He is likely to generalize particular incidents and opinions to the wrong social categories—either to a polar type "The People," or else to the social categories and roles of his own home society. He easily slips into a "we" - "they" categorization of all his experience which exaggerates the absolute differences in the phenomena observed.

The objective characteristics of the population to be studied may be misleading. Many of the traditional techniques of research require knowledge in advance of the size of the universe in various social categories, and some general descriptive information on group characteristics. For many East African peoples, much information is simply not available. The traditional survey design becomes an all-or-nothing proposition, either it furnishes all the required data internal to its own compass, or it is not worth conducting in the first instance. Any research design which depends upon the matching of individuals or groups as its means of "control" will involve enormous field difficulty.

The investigator discovers the relevance of theory to interesting local features in the terminal phases of his research. The published literature is sufficiently "out of tune" with local realities that few of the problems which seemed so provocative from overseas materialize as valid research topics. Instead, one is likely to uncover his best "leads" in the final months after he has learned the language and people have come to trust him enough to reveal their more intimate lives. The investigator commonly finds he learns more of consequence in the final three months of a research period than he learned in the preliminary fifteen. The marked threshold in gaining access to confidence will often cut short much of what one has thought to be true previously; it will render "dated" most preliminary survey questioning.
Supervision of assistants in any number becomes a full-time job. The various logistic and training problems connected with the use of untrained assistants require full-time supervision if what they are doing is of any magnitude. Such supervision has high "opportunity costs," in that it is given at expense of additional time spent in learning the language, etc.

Lack of opportunity for secondary analysis.
By convention, individual social science researchers have usually taken their "data" with them at the close of their field research. No further record in detail exist locally as to what they have done, to facilitate contacts by other researchers working on similar topics in the future. Nor are they obligated to send back the final reporting on their research, often in the form of a doctoral dissertation. In practice, this means that one cannot carry out secondary analysis of any type--each research project must begin de novo with its own field data. In sharp contrast to research in the natural sciences, the social science field research cannot become cumulative, because so little of the necessary material is published in final form.

Field Research is personally exhausting.
Going "on safari" can easily become a full-time activity in of itself. The various components of field work--frequent travel, constant social interaction with strangers in an unfamiliar cultural context, the work of keeping adequate records and complete accounts, the administration of assistants--all require major inputs of energy. The methods themselves tend to preclude time spent in refining one's concepts and insights while in the field. The investigator may discover that he is "flying rusty" in the grasp of overall concepts while in the field, that he requires additional time taken in a different environment to let him think about his materials.

The foregoing remarks, of course, apply with equal force to any type of field research. They are not confined merely to conducting social surveys. The administering of social surveys, however, has in addition its own special problems. These arise out of the initial stages in survey organization for which we do not have an explicit methodology comparable to what available for the actual "running" of the survey "in the field." The difficulties are:

The researcher must define the problem for the Agency, "Consumer," and "Public." It is then up to the researcher to convince him that this is a particularly good place of research. In this respect, the researcher is under a great deal of pressure to show that Government rarely wastes outside advice and investigation of areas in which it has a large commitment and uncertain chances for success. Contrary to the model, the "Consumer"--in countries where most developmental activity is centrally initiated--rarely comes begging with research projects to be done. The researcher, to the contrary, must evolve his own estimate of what problems are actually crucial to Government. Then he must spend a great deal of time and energy trying to convince his "consumer" that the project is vital enough to be worth granting the permission and assistance required. The researcher is faced with a continuing public relations job with his potential client, the Government, as well as with the public.

Almost any type of research requires a major "information retrieval:" research effort merely to assemble the necessary preliminary descriptive information. The literature search phase of survey research--necessary before one can frame "working hypotheses"--is itself likely to require an entire research project. The researcher finds that he cannot rely upon the published material; he must become a contemporary historian, and synthesis an over-view from the files of the various government departments whose activities impinge upon his topic. This
'Preliminary stage' may actually take up several months of work in the archives and in government offices. The amount of time required poses a major dilemma: if it is done initially, the investigator will have to sacrifice much of his efficiency in the language; if postponed, his trial hypothesis formulations will be impractical and his assumptions uncertain. Yet this 'research' is itself very pedantic and unexciting, even though 'gaining access' to the files is one of the more difficult feats in gaining governmental cooperation.

14 The Derivation of hypotheses requires its own research methodology. The methods which serve to validate hypotheses are almost directly anti-logical to those which are most efficient for deriving them in the first place. The derivation is itself a much more difficult task where one lacks background information and where the cultural context is different from one's own. The traditional model for survey design may tell how to "operationalise" concepts, but it does not tell how to derive them in the first place. It is assumed that they "flow" from one's acquaintance with local circumstances and with general theory. In strange circumstances, to the contrary, one finds that it is hard to go beyond "commonsense" description: the leap from a livestock ranching scheme or a coffee co-operative to "theory" is a difficult one indeed. One soon comes to appreciate that techniques for deriving relevant concepts rapidly are a most important part of one's research methodology.

15 Most survey measures will be entirely undetectable to interviewers and respondents alike. In Western countries, one usually worries about "test sophistication" affecting survey results: here, one must worry about the other actors. Many types of measures whose overall configuration is familiar overseas may give difficulties when administered locally. This is especially true of tests which require instruction concerning the registering of a response: e.g., through ranking (indicate 1st, 2nd, 3rd choices), indices (place yourself on the following scale), or other such response devices. Even the simple choice of "check" versus "ticks" to indicate a positive choice may cause difficulties. Consequently, each measure to be used in a study requires pre-testing and revision, sometimes for each area in which it is to be given. The effort is to further lengthen the time required in setting up and in scoring a survey: operations which would be routine overseas require skilled attention at all phases here.

The foregoing difficulties are not unique to Africa, but they do make social survey research a very difficult proposition in any non-Western nation. They underline the social anthropologist's traditional reliance upon participant observation techniques. Contrast, for example, the usual stages in an anthropologist's research with those already given as model for surveys:

<table>
<thead>
<tr>
<th>Study</th>
<th>Read</th>
<th>Make</th>
<th>Choose</th>
<th>Observe</th>
<th>Learn</th>
<th>Type</th>
<th>Record</th>
</tr>
</thead>
<tbody>
<tr>
<td>Home University</td>
<td>Metzana</td>
<td>In the Field</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6 Refines Concepts</td>
<td>7 Sends back copies of his book</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4 Unites</td>
<td>From the standpoint of proving ideas, the anthropologist's path is little better than a raker's progress. From the standpoint of finding ideas, however, it would be hard to improve upon.</td>
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<td></td>
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In summary, I would suggest that the social survey researcher seeks his own anthropologist when he is studying his own culture. The incompleteness of social survey methods does not fully appear until one exports them from out of their accustomed institutional and academic context into a foreign environment. To use them efficiently overseas, one must combine them with other methods which are presupposed in their home context.
Furthermore, I would suggest that the survey techniques needed to find ideas are often irreconcilable with those needed to test ideas. The tension between these two conflicting goals lies behind many of the hardest choices in research design which tries to accomplish both simultaneously.

For example, what use should be made of general, "world-wide" hypotheses in an investigation into a new region? Let us use for our exercise the problem with which this paper is concerned, the factors leading to the emergence of progressive farming in East Africa. There is no shortage of generalisations derived from other areas which might apply to the topic here. Biers has published a list of thirty-eight "principles of technical assistance action" which might be responsible for the success of governmental programs. \(^1\) Rogers has evolved fifty-two similar hypotheses about the diffusion of innovations in rural society; Batten gives twenty-seven "sumary conclusions" about what makes an extension worker effective, and so the list grows. \(^2\) All of these ideas might turn out to have significance in the East African situation, but it is hard to say \textit{ex post facto} which ones.

The traditional survey design would indicate that proper procedure would be to choose one or two of the most promising, and then to proceed to put them to operational test. The paradigm would be:

\begin{itemize}
  \item [1] Emphasis in \ldots  
  \item [2] Choice of \ldots  
  \item [3] Field test \ldots  
  \item [4] Revision \ldots  
  \item [5] Ideas of hypothesis
\end{itemize}

One moves from the limited, working hypotheses, to the more general ideas which grow out of the research experience, and as theory advances. Because surveys are expensive to organise and controls difficult to attain, one must always begin with the limited objectives of working with only a few initial ideas.

For our situation, this design carries with it grave risk that the few hypotheses chosen for testing will turn out to have a low order of significance. Certainly, the traditional procedure offers no systematic ways of narrowing one's risks of studying irrelevant phenomena. Relevance grows out of the crucial test itself.

In actuality, most researchers follow a reverse pattern. They know that their own time and funds are in scarce supply, and that a definitive experimental test is expensive of both. Significant results are more likely to flow from a wise choice of the variables tested than they are from the manipulation of data after a test has been run. \(^3\) The rational priority in the field situation is to maximise one's feedback of information on all points which are possibly relevant to the final problem. The general hypotheses become points of departure for examining the society, rather than vice-versa. One uses them as tools to discover areas of potential significance; positive feedback serves as its own test of their relevance. The research paradigm then becomes:

\begin{itemize}
  \item [1] Isolate \ldots  
  \item [2] Choose \ldots  
  \item [3] Institute \ldots  
  \item [4] Narrow one's \ldots  
  \item [5] Devise a
\end{itemize}

\begin{itemize}
  \item [A] wide range of possible working hypotheses
  \item [B] conditions informal choice
  \item [C] tests of maximum relevance
  \item [D] a few chains of inter-related hypotheses
  \item [E] test between
\end{itemize}

\(^1\) Howard V. Beers, 1964, pp. 74 - 83.
\(^2\) Rogers, 1962, pp. 311 - 315; Batten, 1965, pp. 102 - 104.
There are many other ways in which research oriented towards conceptual enrolment differs from that which tries to assess conceptual validity. Below is a partial list of differences which appear in one's field behaviour:

<table>
<thead>
<tr>
<th>A. Research to Find Ideas</th>
<th>B. Research to Test Ideas</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Use friends heavily for getting as much information as possible in a limited amount of time.</td>
<td>1. Avoid relying upon friends, who will bias the representativeness of one's sample.</td>
</tr>
<tr>
<td>2. Take assistant's ideas seriously as valuable indicators on topics close to one's own perception.</td>
<td>2. Test assistant's views carefully before acceptance to control for special interest.</td>
</tr>
<tr>
<td>3. Rely upon known and accepted roles in local community to increase one's immediate contacts.</td>
<td>3. Cultivate a detached, neutral role which will not bias reception of research data.</td>
</tr>
<tr>
<td>4. Provide definite services to those who co-operate in order to increase social interaction.</td>
<td>4. Remain outside the social system being studied.</td>
</tr>
<tr>
<td>5. Concentrate upon learning the language to increase one's access into people's symbolic culture.</td>
<td>5. Concentrate upon one's observation of social behaviour to check upon the respondent's claims.</td>
</tr>
<tr>
<td>6. Compare widely contrasting areas to reveal to oneself his own underlying assumptions and &quot;blind spots&quot;.</td>
<td>6. Compare areas which are alike in all essential points except for the one independent variable in the test.</td>
</tr>
</tbody>
</table>

In summary, then, the research plan given in the pages following rests upon the premise that different methodological techniques are appropriate to the different stages in the development of a sustained inquiry. Social survey methods are useful primarily at the beginning and end stages of an inquiry. At the beginning, they will indicate the general characteristics of the population being studied. At this juncture the survey supplies "background data" with sufficient quantification to be useful for later correlation tests. The preliminary social survey should not attempt to give a definitive test of a few variables; it should encompass as many different and diverging lines of information as one can practically include. Along with the preliminary survey one should employ historical techniques for "information retrieval"—the analysis of files and past records in the locality to be studied. The researcher should also assemble a battery of possible generalisations on as many relevant topics as he can.

In the intervening stages of research, the concentration should be upon access to information and the elaboration of conceptual richness in one's working hypotheses. Here the social anthropologist's techniques of field work come into their heyday: the important constraint is now time. Every contact should be used to the fullest, and one responds to opportunities rather than to design. The researcher finds the method of "strong inference"—based upon informal tests between alternative hypotheses—his most valuable aid to narrowing down the field of inquiry. When he has been able to construct two or three interlocked chains of hypotheses relevant to the solution of his particular research problem, he is then ready to design a more formal test.

The last phase in field research can again be comprised of a social survey, perhaps combined with a re-analysis of one's preliminary survey results. Here all of the formal requirements of test sampling come into play, and one is again concerned primarily with providing experimental controls for all generalisations made. The survey test gives the researcher his final "data" to take home with him and process in the interval until he receives his next research grant.
Procedural Modifications Suggested for Social Survey Design

1. Offer explicit services to farmers who co-operate with the study: farm plans, photographs, beer, technical advice, etc.

2. Define study locally in such a way that it is clearly understood in relationship to some previous activities. Test various “tags” for naming the study with local informants. Derive the stated goals in consultation with local officials.

3. Work in circumstances where one’s own role is already understood, at least where one intends to get data without preliminary preparation through local residence.

4. Search out assistants who are willing to give negative feedback, who are ruthless in exposing false generalisations.

5. Make maximum use of students who already know the language and yet whose general level of education is sufficient for them to be entrusted with considerable responsibility. Use such individuals as both informants and assistants.

6. Do not try to carry on one’s own field research and supervise an enumeration team simultaneously; while carrying on enumeration, concentrate upon recording the enumerator’s impressions and providing him with as wide an experience as possible.

7. Where possible use students in their home locations, but rotate them with outsiders in the enumeration to give checks upon the quality of their data.

8. Work intensively with enumerators in the period after the survey data collection is complete, to record all of their various observations. Hold joint seminars of enumerators to compare opinions and gain group validation for individual impressions.

9. Rotate one’s own areas of intensive field work to provide controlled contrasts on different items, and remain sensitive to looking for differences between areas.

10. Use the technique of “strong inference” to force oneself to think constantly of alternative explanations for social phenomena, and thus to improve one’s own field observations.

11. Choose areas for intensive surveying where preliminary ethnographies and community studies of high quality already exist.

12. Use working hypotheses as tools to guide one’s preliminary interviews, but let opportunity determine the sequence.

13. Commission specialised studies after the completion of a large-scale social survey, to investigate systematically the variables which are indicated as being important.

14. Set aside regular “re-think” periods, immediately following separate data-recording periods, so that one moves regularly from observation to re-conceptualisation. Do not try to mix observation-al periods indiscriminately with those set aside for thinking.

15. Make maximum use of one’s own short-term memory retention by working across topical fields while on field research, to enable one to discover functional associations between different classes of phenomena.
Batten, T.R.

Beard, Howard W.

Bigar, Robert E.

Fyffe, Herbert.

Lazarsfeld, Paul & Morris Rosenberg, eds.

Morris, J.F.

Noyes, G.H.

Naylor, W.L. & Joseph Ascroft.

Platt, J.R.

See also rejoiner, Weber, P.W. & Susan Presswood.


Rogers, Everett M.

UNICEF
The Kenya Farm Innovation Study

Purpose

The survey here proposed would have as its practical purpose the gathering of background information on all aspects of rural life which might have a bearing upon farmers’ nutritional welfare and their acceptance of technical advice. Within these two large areas of farm activity, the emphasis will be upon the isolation and analysis of factors which lead to farm innovation. A subsidiary purpose will be to train agricultural students and departmental staff in the techniques of applied nutritional and social research.

Sponsorship

The survey is being organised as the first major research project of the UHECF sponsored Rural Extension Unit within the Faculty of Agriculture, Makerere University College, University of East Africa. In addition to the initial support of UHECF, FAO, and the University of East Africa, affiliation will be sought for the project with the East African Academy and with the respective departments of Government with which work will be carried out in the field. Funds will be sought to be able to affiliate a number of related research projects to the larger survey; these projects would be sponsored through the respective national research Institutes of the University of East Africa.

Research Personnel

The survey will be carried out under the direction of the Lecturers in Rural Extension and Rural Sociology of the Faculty of Agriculture, Makerere University College. They will work under the advice of the relevant departmental heads in sociology and agricultural economics, and also in communication with the research directors of the relevant institutes. It is hoped that field training can be given to eight second year Makerere College students, from the Faculty of agriculture, and also to a larger number of Agricultural Training Centre students at Nebu. Negotiations are underway to bring into association with the survey two medical doctors (one a specialist in child nutrition), a lecturer in education, a Professor of Geography and a Professor of Economics. These individuals would have some student help in addition to the personnel hired under the Project itself.

At different times the project will entail keeping between ten and forty individuals in the field, but large numbers of these would be students already in residence locally.

Research Schedule

The period of initial planning and organisation of the survey commenced in December of 1965. It is anticipated that field work will begin in mid-February, survey training and pre-tests in March, preliminary surveys in April, extended surveys in May and June, and intensive community studies in July through September. The period of large-scale activity will last for six months, with data processing continuing through the subsequent year.

Theoretical Relevance

The results of this inquiry should allow us to compare the effects of "proximate factors" versus "conditioning factors" in leading to increased levels of farm family welfare. We will also be able to compare the differential strategies best suited to the development of both high potential and low potential areas. The information should be of great use to all three East African governments in their choice of development policies. The material gathered on the dynamics of farm innovation will be able to test a large number of generalisations derived from experience elsewhere in the tropics. This will be the first such test undertaken in East Africa, and it should yield results closely comparable to a similar study being initiated under the Economic Development Institute of the University of Nigeria.
<table>
<thead>
<tr>
<th>Date</th>
<th>Operation</th>
<th>Personnel</th>
<th>Type of Data</th>
</tr>
</thead>
<tbody>
<tr>
<td>JANUARY</td>
<td>Preliminary interviews in Embu</td>
<td>Watts &amp; Morris</td>
<td>Vertical File begun</td>
</tr>
<tr>
<td></td>
<td>Obtain legitimacy from Nairobi</td>
<td>Morris, through Faculty of Agriculture</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Collect background data from documents, files, etc.</td>
<td>Morris &amp; Watts</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Set up filing headings</td>
<td>Morris</td>
<td></td>
</tr>
<tr>
<td>FEBRUARY</td>
<td>Design of protocols, recruit student enumerators</td>
<td>Morris &amp; Watts</td>
<td></td>
</tr>
<tr>
<td>MARCH</td>
<td>Get local introductions in Embu Chief's, D.C., Heads Dept., etc.</td>
<td>Morris &amp; Watts</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Pre-test preliminary interviews</td>
<td>Morris &amp; Watts</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Choose broad sampling areas</td>
<td>Morris &amp; Watts</td>
<td></td>
</tr>
<tr>
<td>APRIL</td>
<td>One week's training for jr. enumerators, position sr. enumerators</td>
<td>Watts</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Initiate Preliminary Survey</td>
<td>Watts, 20 students</td>
<td>(1) Farm Profile</td>
</tr>
<tr>
<td></td>
<td>Initiate parallel studies of water usage, school leavers, marketing, etc.</td>
<td>Morris, 5 students</td>
<td>(2) Index of Farm Change</td>
</tr>
<tr>
<td></td>
<td>Conference of enumerators</td>
<td>Morris</td>
<td>(3) Enumerator ratings of each locality</td>
</tr>
<tr>
<td></td>
<td>Placement of Sr. enumerators at new locations for special projects</td>
<td>Morris</td>
<td>(4) Enumerator ratings of farm variation</td>
</tr>
<tr>
<td></td>
<td>Rotate visits between areas, rotate days on farm visits, record impressions</td>
<td>Morris</td>
<td>(5) Special Project Protocols</td>
</tr>
<tr>
<td>JUNE</td>
<td>Same as above, completion of special projects</td>
<td>Morris &amp; Watts</td>
<td>(6) Farm Visit Contrasts</td>
</tr>
<tr>
<td></td>
<td>Recording of Enumerators overall impressions, reliability checks between</td>
<td>Morris &amp; Watts</td>
<td>(7) Sr. Enumerator ratings, special projects</td>
</tr>
<tr>
<td></td>
<td>enumerators at new locations for different enumerators</td>
<td></td>
<td>(8) Descriptive Outlines</td>
</tr>
<tr>
<td></td>
<td>Study of Institutional Factors in Watts Extension</td>
<td></td>
<td>(9) Health Records</td>
</tr>
<tr>
<td></td>
<td>Initiate Farm Family Nutrition visits</td>
<td>Dr. Watts</td>
<td>(10) Field Data on three areas</td>
</tr>
<tr>
<td>JULY</td>
<td>Beginning of intensive community study, background interviews</td>
<td>Morris</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Intensive Investigation of Local Extension Organisation</td>
<td>Watts</td>
<td></td>
</tr>
<tr>
<td>AUGUST</td>
<td>Continuation of the above</td>
<td>Watts, Dr. Watts, Morris</td>
<td></td>
</tr>
<tr>
<td>SEPTEMBER</td>
<td>Comparative interviews with Ten-Outgrowers &amp; Enumerators, Kambu</td>
<td>Morris</td>
<td>(11) Comparative farm profiles</td>
</tr>
<tr>
<td></td>
<td>Perhaps also Kisii &amp; Korioho.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>OCTOBER</td>
<td>Begin Processing of Data</td>
<td>Morris &amp; Watts</td>
<td></td>
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</tbody>
</table>
VARIABLES

The project will concern itself with two dependent variables, which are probably related to one another but which must be kept separate in the design of research. These variables will be studied in the context of the behavior of farms as units. The first is the attribute of innovativeness, and the second the attribute of nutritional well-being.

We are considering a number of alternative ways of dealing with farm innovation. Most of these involve a categorization of farmers at the two extremes into separate samples of "innovators" and "rejectors." The innovators might be designated on *a priori* grounds as those who are taking up new crops which are currently being introduced into the area—tea in the upper ecological zones, and cotton in the lower. Alternatively, we have considered drawing a sample from those farms which have been offered to the department for farm planning. The "rejectors" are more difficult to isolate. Perhaps the "chain" method can be used, whereby the least progressive neighbor of each "innovator" will be entered in the sample—assuming that proximity has furnished occasion for communication about innovations which have been rejected by a given family.

There is no advance information by which we can identify farm families which have good and bad nutrition, respectively; we will have to draw our samples from areas which are sure to furnish some of each. So far we intend to stratify our sample according to ecological zones, access to transportation, nearness to political centers, and subject to land consolidation. The sample design is still under consideration, and obviously involves issues of great complexity.

The independent variables which we are particularly anxious to control for are the following:

1. Previous access to education.
2. Ecological potential of farm environment.
3. Different types of farming systems within one culture.
5. Income levels.
6. Administrative history of past development efforts.
7. Access to markets and transportation.
8. Family structure.
9. Command of resources.
10. Institutions of production organization.
11. Intensity of agricultural technical services.
12. Ideology re: aspirations, expectations, etc.
15. Membership in elite groups vs. traditional groups.

Almost certainly many of these variables are inter-related, and one of the main tasks of data analysis will be to discover and elucidate these relationships.

We would welcome additional assistance in drawing up a sampling design to control for as many of these variables as possible. The location of the survey area, in Hubei, is a great advantage of control available.
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