INTRODUCTION

An enthusiastic young technician from the U.S. Department of Agriculture went to Nigeria in 1966 to help direct a study of factors affecting agricultural production. He selected a ‘national’ sample of some 30 villages, hired a corps of Nigerian interviewers, and - after a lofty academic briefing by the chief of the project - sent them off to gather data on lengthy interview schedules from a sample of about 900 farmers.

Problems immediately began to occur. The interview questions, which asked for details on farm acreage, number of livestock, and crop yields per acre, simply did not make sense to the village farmers. They could not or would not recall such facts as how many bushels of yams they sold in the past year, and at what price. Further, the interviewers’ work was neither personally supervised nor verified. Since the postal services was as unreliable in transmitting paychecks as field instructions, several of the interviewers quit having never seen either their pay or their paymaster.

Nine months and many thousands of dollars later, a now unenthusiastic researcher returned home with a total of only 43 completed interview schedules.

His experience is not unique:

A U.S. sociologist in a Latin American country attempted to interview lower socio-economic class respondents using five-point agree-disagree attitude scales. His respondents either tended to ‘strongly agree’ with every scale item or else to claim they had ‘no opinion’. They would not or could not answer in a continuous scale of opinion.
graduation as intended by the researcher. He was disappointed to find that the restricted bi-modal distribution of his attitude scores made measurement and analysis impossible.

An Asian researcher assumed that he could determine the manner of adoption of agricultural innovations by gathering data from the heads of all farm households in one village. His research design was implicitly based on studies in Western countries, where most innovation decisions are made by individual farmers. He eventually found, however, that one powerful landlord in the village decided which fertilizers, crop varieties and other new ideas were to be adopted by everyone. Thus, the research was designed for culturally inappropriate units of response.

In Colombia, interviewers explained to their rural respondents that their survey was sponsored by a North American University. Soon the rumour spread that the interviewers were really spies, looking for small children to be ground into sausage in the United States. The interviewers were puzzled by an almost 100 per cent refusal rate. They did not realize that the villagers having had no previous conception of survey research had to learn how to be respondents, including the notion of trusting complete strangers who ask peculiar questions.

In a Punjab village in North India, a social scientist asked socio-metric questions of his respondents, such as: 'Can you tell me the name of villagers you talk with about new farm ideas?' The survey was partially completed before the researcher learned that analyses of the answers were meaningless. All of the villagers had the same name.

What went wrong in each of these cases? There are, of course, a variety of reasons for each of the data-gathering debacles, but a common theme underlying all is the limited appropriateness of 'Western' survey research methods. In each case, implicit assumptions associated with a research technique became explicitly erroneous when it was utilized in a different socio-cultural setting than that in which it was developed.

Through a great number of errors such as these, as well as through many methodological successes, we have learned much in recent years about how properly to go about conducting survey research in developing nations. And the biggest single lesson is that it is much different than in the more homogeneous, industrialized nations such as Sweden, England or the United States. This important point, along with its many ramifications, is what the present volume is all about.
I. DEFINITIONS

A. SURVEY RESEARCH

Survey research is a cross-disciplinary approach of the social sciences used to further the objective study of human social behaviour. The major social sciences were intellectually born in Europe, grew in methodological sophistication particularly in the United States, and in turn have been exported to most of the rest of the world. But their transplantation to developing countries causes certain difficulties. Among others, the social science concepts and methods of the industrial nations are not always appropriate to developing nations.

This book focuses on overcoming cultural biases in survey research. We consider survey research methods as all those techniques of scientific investigation that are utilized to gather data from the field in order to generalize results from a sample to a larger population. Our central concern in surveys is with the ability to generalize. From the sample, we infer or generalize from the data obtained to the entire population.

As will be seen in Chapter II, certain other characteristics usually typify a survey. For example, the number of respondents interviewed generally is numerically and culturally adequate to allow generalization to the study population. Seldom does a survey sample consist of only one or a very few individuals. The unit of response in the survey typically is a person, although this is not a hard and fast restriction. Presumably any population — whether people or objects — that can be fully counted, sampled and measured can be the survey unit of response or analysis.

Further, most survey data-gathering instruments are relatively structured, or standardized, so as to facilitate quantification of responses into categories and numbers. This is usually desirable to optimize the capacity for summarization and generalization of the results.

Most surveys entail only a relatively brief amount of time for gathering information from each respondent. We are usually more interested in gathering data from many respondents than in obtaining intensive, detailed information from a very few individuals. This limited time duration of survey data gathering necessarily limits the number and types of variables that can be measured adequately or
even accurately. The disadvantage is offset by the primary purpose and advantage of surveys: generalization of results.

Many surveys are designed for analytic or diagnostic purposes to test hypotheses about relationships between variables, but some surveys are conducted simply to describe the nature of a population. Historically, most surveys were undertaken for descriptive purposes, but today the hypothesis-testing potential of the survey is being more fully realized. And diagnostic research is more important for problem-solving in developing nations than simply for testing hypotheses for theoretical interests.

Reliance on field data gathering distinguishes survey research from laboratory investigations. All of the social sciences use survey methods, although the disciplines of sociology and political science perhaps depend most heavily on survey methodology. For practical problem-solving, social science laboratory research is rarely conducted in developing nations, while field research is very prevalent. This is principally a result of the growing need for more reliable field information by governmental and other developmental agencies sponsoring programmes for the social and economic development of the more disadvantaged segments, usually rural villages and urban slums, of the developing nations.

The inherent advantage of experimental research techniques is the high degree to which the investigator can control or remove the effect of unwanted variables from those he is studying. In contrast, the ability to generalize from survey research results is of great importance to development planners, administrators and other practitioners in developing nations. For example, each year the Indian national sample survey, designed by the Indian Statistical Institute, conducts at least one survey of perhaps 4,000 villages and 26,000 households and describes 115,000 people throughout the country. The NSS is a basic source of all varieties of data on population characteristics, movement, employment, income and so forth.

But the difference between field surveys and laboratory testing does not mean that survey research methods are equivalent to non-experimental techniques. There is a useful hybrid of survey and experimental methods, called the controlled field experiment, in which an experiment is conducted in the field rather than in the laboratory. This approach provides many of the advantages of the survey, such as the ability to generalize, with several of the benefits of the experiment, such as the ability to control the effects of extraneous influences (see
Chapter IV). And, in the past decade, improvements in computerized data-analysis techniques have greatly increased the survey researcher’s control of the influence of unwanted variables, although the degree of control does not approach that of the experiment.

B. RESEARCH IN DEVELOPING NATIONS

This book concerns survey research in countries of the so-called ‘Third World’, those countries with predominantly agrarian economies that are increasingly on the march toward higher levels of industrialization. Descriptive conventions usually range from ‘developing nations’ to such synonyms as traditional, industrializing or emerging nations. We will typically talk in terms of developing nations. In all cases, we think of these countries as arrayed toward one end of a dynamic continuum of industrialized development.

The continuum of development is often arbitrarily divided into two classes on the basis of socio-economic criteria: the ‘developing’ and the ‘developed’. The United Nations’ classification of developing nations includes all those of Latin America, Africa and Asia, with the exception of Japan, South Africa, Australia and New Zealand. Like all dichotomous categorizations, this one leaves much to be desired. For instance, it lumps both Haiti and Argentina into the developing category, even though Argentina’s per capita income is about seven times that of Haiti.

In spite of the tremendous diversity among developing nations, they have certain common characteristics. They tend to be agricultural economies typified by relatively low per capita (1) income, (2) productivity, (3) nutritional status, (4) literacy, (5) transportation and communication facilities and (6) health, water and social service facilities. They also tend to have comparatively high birth and death rates, high rates of illness, malnutrition and disease, and short life expectancy.

National development projects and international technical assistance to developing nations burgeoned after World War II. Although countless development projects were launched by local and national governments and by international agencies, relatively few have been analyzed by techniques adequate for measuring their actual effectiveness or for guiding them towards increased effectiveness (Hayes, 1966). Perhaps many development programmes failed because either (1) they were not guided by scientific (non-arbitrary) study aimed at their im-
Improvement, or (2) in the comparatively rare cases when such investigation was conducted, the research methods involved were improperly utilized or were so inappropriately adapted to the socio-cultural setting that the obtained data could not appreciably improve the development programme.

One of the important difficulties for development administrators, planners and researchers is that the data on basic population parameters and development indicators, e.g., agricultural production and infant mortality, are often unreliable, if not incorrect. Social science research methods offer development agencies an effective and efficient approximation of critical development indicators. Without the contribution of the social sciences the process of development is very much slower, more painful, more arbitrary and more unequal than it has to be (Pool, 1966).

The great need for accurate data on cross-cultural human behaviour and acceptance of change has been increasingly realized by development planners in emerging nations. Among the social sciences, mainly anthropologists and economists were first to respond to the need for research pertinent to international development. Then, on a large scale, sociologists and political scientists and some social psychologists became involved in survey research in the 'Third World'. Since World War II, a major commitment of each of the social sciences has been to international research on development problems.

To date, the problem of the social scientists generally has not been a lack of eagerness to enter the arena of need, but rather that their tool bags are filled with culture-bound tools. And, while we know that the theories and methodologies developed in industrialized societies require selective adaptation and modification in different cultural settings, we do not know - until after an expensive process of trial and error - just how much and what kind of adaptation is required.

Some research projects are cross-national as well as cross-cultural in nature, but these two types of research are not necessarily identical. Cross-cultural studies inasmuch as possible attempt to develop comparative - and hopefully equivalent - concepts and operations. The purpose usually is to test the validity of a proposition in more than one culture. The different cultures may be within the same state, within the same nation, or in different nations. An example of a cross-cultural, mono-national project is the study of family planning practices in Haryana and Tamil Nadu states in India reported by Mukherjee (1973) for the Council for Social Development.
Cross-national studies are comparative studies in one or more national settings. An illustration of cross-cultural, cross-national research is the Diffusion Project, 1965-69, in Brazil, Nigeria and India. Each of these countries had different languages and cultures (in fact, three different major cultural-linguistic groupings were included in the study in India and two in Nigeria), but the objective was to develop cross-cultural generalizations about the process of the diffusion of innovations in each country.

Some surveys in developing nations are carried out in order to compare their results with those obtained in other cultures or other nations; but of course most are not. Nonetheless, surveys restricted to one sub-culture within a single nation, such as small area studies carried out for graduate theses, are also the concern of this book. Wherever possible, however, we shall discuss the special, additional problems and considerations that are involved when the objective of a survey research project is cross-cultural comparison.

II. THEMES OF THE PRESENT BOOK

Early attempts in the 1940s and early 1950s to create an international social science largely amounted to the migration of thousands of foreign students from the developing countries to universities and technical schools in the United States and Europe. They received a stock ‘Western’ education with almost no application to the special needs of their home countries. It was somehow assumed that, upon their return, these students would find appropriate intellectual and technological outlet for their newly acquired knowledge. But many did not. Writers became clerks, economists became bank tellers and scientists became salesmen (Thompson, 1966). And of course many did not return, causing an intellectual drain of serious proportions from the developing nations.

Gradual realization of the problems in the late 1950s and early 1960s led to a different approach called institution-building. Instead of mass migrations of students, relatively small numbers of professors

1Throughout this volume we refer to this study as the ‘Diffusion Project’, although its full and official title was Diffusion of Innovations in Rural Societies. The project was sponsored by the United States Agency for International Development and administered by the Department of Communication, Michigan State University (Rogers et al., 1969).
from industrialized nations journeyed in reverse to foreign institutions around the world. Their mission was to educate students in developing nations within their own cultural settings.

The visiting scientists soon realized the appalling degree to which their social science specialties were culture-bound. Heretofore adequate theories and concepts suddenly had to be reconsidered in light of the abrasively dissimilar social structures of the now-pertinent host countries. Slowly the perspective of social science began to broaden to see the need for empirical research to test the applicability of ‘Western’ theories in unfamiliar socio-cultural circumstances. But when such investigation was attempted, the sojourning professors realized that their research methods were just as culture-bound as their theories. Still needed were ‘culture-free’ survey methods appropriate to world-wide research settings.

The typical research project in a developing nation in the 1950-60 period consisted of ‘data-mining’ by the sojourning social scientist. He came to the host country, gathered his data, and went home to complete his analysis. This ‘safari’ research expedition left no lasting imprint on the host country’s research capability. If anything, the visitor left disappointed hosts who expected useful research results, correction of social problems, or even joint publications. Some of these sojourners have been:

The ‘data-exporter’ or ‘safari’ scholar who takes all of the data and leaves nothing of value to the country. Sometimes he is called the ‘hit and run’ researcher, with more ‘runs’ than ‘hits’.

The ‘theory-builder’ who has some notion about how development works and tries to get as many cultures in the sample as possible in order to derive universal generalization.

The ‘penny-collaborator’, who has access to some money and seeks ‘cooperative’ projects by providing the money while someone else does the study.

The ‘professional overseas researcher’ who lives from research grant to research grant as a way of life.

The ‘instant expert’ who may be doing his or her first study, but because it is in an unknown area acquires the status of an expert as soon as something is written (Tagumpay-Castillo, 1968).

It should be presumed now that the days of the narrow-minded
sojourn scholar are over. Technical assistance usually from 'outside' agencies or universities seeks to improve the research capability of the host institution, so that it can competently continue social research after the period of assistance is ended. In fact, the effectiveness of institution-building research efforts will largely be determined in the years after direct assistance. Institution-building requires long-term planning; a high degree of interest in the development of institutional competence rather than personal reputations; and often institution-to-institution collaborative relationships through which an institution in the developing nation is paired with a counterpart organization in the industrialized world.

Institution-building is one important theme of this book. In addition, there are a great number of specific differences in the conduct of survey research when the setting is the Punjab, Nairobi or Chile, rather than Texas, London or Japan. One salient characteristic of developing nations is the great variability within them. The vast heterogeneity of languages, styles of life, levels of living, attitudes, ethnic backgrounds, religions and other human characteristics of most developing countries is virtually impossible to categorize. None of the developing nations, even the smallest, appears to have the characteristics of a 'mass culture' such as that of the United States and many Western European countries.

Cultural variation occurs with regionalism much more in developing countries than in industrial Western countries. For example, in Thailand the people depend primarily on rice and fish for subsistence, the predominant religion is Theravada Buddhism along with an active spirit cult, houses are typically built on stilts, most of the languages are tonal, and age is an important status factor. However, we must remember we are speaking only of the dominant valley people, the Thai. In the North there are many tribes, in the South there are Moslems, and in the cities and towns there are Chinese and Indians, each with distinct cultural complexes. The same kind of variability based on regional differences is found in many Latin American and African countries.

There are different sub-cultural groups in industrial countries, of course, although the degree of heterogeneity is considerably less. For example, some of the people of the poorer rural and urban ethnic areas in the United States have different customs of speech, food and dress than are typical of the broad middle-class 'master culture'. But these differences are much smaller than between the Meo in Northern
Laos and the valley Lao, or between the Hausa and Yoruba tribes of Nigeria.

The major consequence of these greater differences for comparative ‘development research’ is that, unless there is adequate sub-group representation, one cannot make valid inferences about nations or even smaller sub-divisions with a separate cultural identity, however great the temptation. Data collected from the Yoruba do not provide generalizations applicable to the Hausa, just as data on the Meo do not explain behaviour of the Lao.

The concept of ‘cultural type’ is probably more useful for cross-cultural generalization than is the concept of nation. Thus, there are in the developing nations horticultural societies, agricultural societies, nomadic herdsmen, the urban working class, the urban elite. The way of life of people of a cultural type, such as peasant agriculturists, should have common features, regardless of other cultural differences.

Another distinction within developing countries is the social distance separating urban elites from rural peasants. The elite, who control the power and politics of these countries, are members of a cultural complex which has many close connections with the ‘other’ industrial world. They usually speak a European language as well as their own. They often wear Western-style clothes and they consume many types of Western food and drink. Their participation in political affairs is based somewhat on a Western model. On the other hand, the rural people usually speak only the local language(s), consume local food, wear traditional clothes, and take little part in the national and international affairs which concern the elite.

A particular characteristic of the developing country research settings with which this book is concerned is their degree of unfamiliarity to ‘outsiders’ – that is, non-indigenous researchers who may be both foreign and host-country social scientists. Only a small share of social science research has been conducted in developing nations, and too few of these social scientists are indigenous. Consequently, the survey researcher often starts an investigation without the helpful legacy of a large, reliable body of accumulated past research. Further, many developing nations are huge in land area and in number of people. These are initial conditions under which familiarity, and hence generalization, is difficult.

Another reason for the relative unfamiliarity of most socio-cultural settings in developing countries is that the investigator is markedly
different from his respondents. Such differences make effective communication between the two very difficult. They do not share the same frame of references, meanings and (often) language. Survey data-gathering is essentially a communication process, and in developing nations these critical source-receiver differences pose important obstacles to understanding. Typically, there is a vast social chasm between the researcher and the respondent in terms of education, income and perspective. Compared with the urban, white-collar or upper-middle class researcher, most respondents are either rural villagers or urban poor. And, social distance is widened further when the researcher is from another country.

To bridge this hiatus of unfamiliarity, the researcher must learn to take the role of the respondent, as the opposite is unlikely to occur. To gain empathy with one's respondents, the researcher should seek well in advance of actual interviewing to know as much as possible about them through a careful programme of reading, discussion and exploratory data-gathering. In short, the scientist seeks to make the unfamiliar socio-cultural setting more familiar by involving himself in the culture before presuming to study it. Social researchers should spend much effort in first using relatively unstructured data-gathering methods — case studies, unstructured interviews, etc. — before moving to such highly structured techniques as survey methods (see Chapter VI).

The themes of this book concern how survey research must be designed and conducted as an institution-building strategy in developing nations, with the related objectives of, first, accounting for the great variability within them, and, second, becoming more relevant to the socio-cultural environment within which measurement occurs.

Subordinate to these key issues, other themes developed in the book are (1) the need for using complementary research methods to support and enrich surveys; (2) the need for adequate pre-testing and other pre-study exploration prior to conducting the full study; (3) the significance of human errors — particularly those of the interviewer — that act systematically (non-randomly) to bias and thereby weaken the survey methodology; (4) the need to study non-random sources and types of biases and to report these methodological findings along with the findings for the study population; all as a basis for (5) improving the ability of other researchers to replicate the study and of practitioners to know the socio-economic feasibility of implementing results.
We see culture as a system of interrelated beliefs and behaviours shared by members of a society, and transmitted with some changes from one generation to the next by means of education, formal and informal. Thus, a given cultural pattern is distinct from all others, at least in its total configuration. Some specific behavioural patterns such as languages are so different as to be incomprehensible to members of another culture. This does not mean that two languages are not comparable, but the comparability is not obvious to the ordinary speaker, even if the languages are related. The fact that languages are comparable below the surface level is indicated clearly by the fact that one language can be translated into another.

Other behavioural patterns are related across cultures, as is obvious even to the superficial observer. Thus, all known peoples have some type of dancing, which appears to fill the function of providing aesthetic satisfaction. However, dancing can have a wide variety of other functions in different cultures such as contributing to rain-making rituals, celebrating fertility rites or helping to cure the sick. But at least one aspect of the behaviour, aesthetic satisfaction, seems to be comparable cross-culturally.

Such examples indicate that culture can influence the degree of comparability of research designs and theories. But in planning a research project, it is necessary to know more than this. The investigator needs to know how much cultural differences influence concepts and techniques.

A. Universal Human Behaviour

How similar is the behaviour of people across cultures? The answer largely depends on the level of generality. The more general the type of behaviour considered, the more likely it will be universal. All people have many common characteristics and problems, whatever their culture. This situation is quite similar to that of biological classification. Members of a biological unit such as mammals have by definition some universal characteristics such as hair, feeding the young by means of milk glands, and a long period of dependency for the young. If one moves to a broader classification, such as vertebrates, there are even more universals such as eyes, ears, locomotor appendages, a backbone, etc. However, if one moves to a more
specific level, from mammals to ungulates, for example, there are fewer universals such as hoof types and teeth patterns.

Behavioural patterns are no different insofar as classification is concerned. At a very general level, there are many cross-cultural similarities. Examples are the necessities of communication, food, shelter, procreation, defence, etc. There are also common psychological characteristics of humans, to a large extent derived from their biological nature. Thus, the long period of dependency of the human child creates a special relationship between child, parents and more distant relatives. Consequently, there is a universal behavioural pattern called kinship, based to a large extent on attitudes of adults toward their dependent children. If children matured in a year or less, probably neither the family nor kinship would exist. There are also social needs which do not satisfy biological drives directly, but assist in the maintenance of society. Examples are education and social control, found in every known society.

Murdock (1945) was centrally concerned with the problem of cross-cultural classification. In his survey of world ethnographies, he found 72 behavioural items which he claims occur in every known culture. The range is illustrated by the following examples: age-grading, cleanliness training, cosmology, divination, faith healing, folklore, gestures, hair styles, incest taboos, kinship nomenclature, magic, modesty concerning natural functions, numerals, population policy, sexual restrictions, tool making, and weather control.

Thus, cultural universals exists, but they are of a general nature and depend upon the definitions of the classifier. Within each broad classification, however, there are great variations; social scientists have emphasized these, rather than cultural commonalities. For example, hand and facial gestures vary to an extent that a person in one culture can make a serious error in using the gestures of another. Several American Indian tribes point with pursed lips, a gesture that invites kissing among Westerners. North Americans indicate assent by tipping their heads up and down in a forward motion, while the same attitude is expressed in some areas of India by rocking the head sideways. Similar contrasts can be made in regard to weather control. Westerners depend on measurements of meteorological events, and try to control rainfall by cloud seeding with chemicals. Some Laotians, on the other hand, attempt to produce rain through means classified as magical by Westerners; they pour water over statues of the Buddha. Thus, we recognize cultural
universals in general, but cultural variability in specific content.

What cultural universals exists in development and modernization? Unfortunately, cross-cultural research on development is in its infancy, and much work has not been truly comparative. In a sense, the social sciences of development are in somewhat the same position today as was anthropology before large-scale comparative efforts were attempted, such as Murdock’s Human Relations Area Files (see Chapter VI). There were then many adequate ethnographies from cultures throughout the world, although little attempt had been made to extract universal phenomena (Murdock, 1953). Today, we possess a number of investigations of development in the industrializing nations, but there has been little intensive effort to synthesize these into more general theories of development and modernization. Thus, the search for universals in the process of social change and development continues as a primary goal and should be a major challenge to development researchers.

B. MODERNIZATION AND DEVELOPMENT

All definitions imply value judgements. For convenience, we define modernization as the process by which individuals change from a more traditional way of life to a more complex, technologically advanced, rapidly changing style of life. Modernization is an individual process. The parallel process at the level of the social system is development; defined as a type of social change in which new ideas are introduced into a social system in order to produce, e.g., higher per capita incomes and levels of living through ‘more modern’ production methods and/or ‘more efficient’ forms of social organization.

The nations of the world often are divided into two classes on the basis of socio-economic development criteria; the ‘developing’ and the ‘developed’. While development and modernization are similar processes, they occur at different levels. There are five common misconceptions about modernization:

1. Modernization often has been equated with ‘Europeanization’ and/or ‘Westernization’. Our concept of modernization agrees with neither of these notions. They are too limiting and inaccurate, especially considering the innovations that have come from the technological prowess of countries such as China, Israel or India and from the industrialization might of the U.S.S.R., West Germany or Japan. Modernization is a synthesis of ‘old’ and ‘new’ ways, and as such
varies in different environments. Most imported innovations require considerable adaptation to meet the differing conditions of the receiving countries. As such, new ideas diffusing to developing nations will not make them into replicas of the industrial exporting nations. However, the main factor that distinguishes developing nations from the more-developed countries is labour-intensive industrial production and a heavy dependence on human or animal power. If so-called Westernization is thought to be somewhat synonymous with industrialization, then modernization will be like Westernization. But the labels of description are as weak as is the logic of direct application.

2. Modernization is a sometimes incorrectly thought to be a unilinear rather than a multilinear process. The notion that developing nations move in only one direction was stated by Lerner (1958) and carried further by Moore (1963), to the effect that modernization is seen as a total transformation of a traditional or ‘pre-modern’ society into the mould of technology and associated social and political organization that characterize the advanced, economically prosperous nations of the West. We sharply disagree.

The main weakness of the straight-line principle of modernization is that it is an ill-disguised evolutionary theory in which all developing societies are heading invariably towards one goal: contemporary Western society. Not only is no one sure which are the Western nations, but we scarcely know what they will be like by the time the traditional systems reach the ‘goal’.

Modernization and development is a multilinear evolutionary process. There is deliberate choosing in this process of change based on the wish to adopt new science technology but to maintain the stability and cohesiveness of the indigenous culture. At any point in time, modernization in a particular country may thus follow one of numerous possibilities conditioned by time, priorities, resources, locale, history and its unique culture. Viewed in this light, modernization and development in developing countries is a technological-cum-cultural synthesis, and will be different and multilinear in each case.

3. Modernization is not unidimensional, and therefore cannot be measured by a single criterion. Modernization is a process involving the interaction of many factors, so we need to measure more than one aspect of an individual’s behaviour in order to determine his status on a modernization continuum. Variables such as literacy
and education, political participation, mass media exposure, achievement motivation, empathy, innovativeness, and a lack of fatalism are all factors related to modernization. Therefore we take a multivariable approach to operationalizing modernization, although emphasizing technological change.

4. We should not assume that modernization is a one-way process, even though there is only a trickle of neo-traditionalization in comparison with a world-wide tide of modernization. Neo-traditionalization is the process by which individuals change from a modern way of life to a more traditional style of life. It is modernization 'in reverse'. Traditions which are dead or on the decline are deliberately revived as an after-effect of exposure to modern ideas. Illustrations of neo-traditionalization are the ritualistic return to native dress and pre-colonial languages by political leaders following their nations' independence.

In some Indian villages, customs of alcoholic abstinence, vegetarianism and seclusion of women are being revived. Srinivas (1966) calls this particular variety of neo-traditionalization 'sanskritization', after the ancient Sanskrit culture of India. He argues that such neo-traditionalization often is motivated by the desire of lower caste individuals to appear more like the Brahmins and other upper castes.

Neo-traditionalization may be motivated by a desire to synthesize modern with traditional ways, or as a result of disenchantment with modernization. An illustration might be the use of Hebrew as the official national language in Israel, and the Black Muslim movement in the United States. These illustrations suggest that neo-traditionalization occurs in social customs, rather than in technology. Thus, neo-traditionals may give up Western clothes, but not radios and railroads. The process of neo-traditionalization has not received much empirical inquiry to date, but we do know that it exists.

5. It has often been implied that all modernization is 'good'. We intend no such value judgement by our definition. Modernization means change, and as such may cause conflict, pain and relative disadvantage. The adoption of new ways of life does not necessarily lead to a 'better' life for all individuals involved, although so far as the society is concerned, technological change may be a necessity.

Thus as we see it, modernization is a multilinear and multidimensional process, tempered by neo-traditionalization, and not to be equated with Europeanization or Westernization, that implies no
value judgement as to its desirability, except perhaps for technological change.

C. SELECTION OF CONCEPTS AND THEORIES

Science aims to be value-free, to be concerned with 'what is' rather than 'what should be', to be objective rather than subjective. Yet the scientist's cultural viewpoint and professional values enter and bias the research process at many points. Two early points are in his selection of concepts and theories and in his choice of research methods (see Chapter II).

Man's environment affects his language which in turn structures his perceptions. This notion is involved in the Sapir-Whorf illustration that a certain Eskimo language contains twelve words for different kinds of snow, while English only has one word (Whorf, 1956). The Eskimo is thus able to perceive twelve kinds of snow, while English-speakers can only distinguish one kind without qualifying adjectives. In a similar way, a social scientist's perception of social problems is equally structured by the concepts and theories that he has been taught. He 'sees' social status, alienation, fatalism and achievement motivation because he has been taught these concepts. And of course he does not perceive phenomena for which he lacks concepts. So his scientific language structures or limits his perceptions of the world, and it affects his choice of concepts, theories and methods for investigation.

This cultural and linguistic effect is not necessarily undesirable. But in many cases 'social science' really means 'Western social science'. So there is a potential danger in the unquestioning utilization of social science concepts and methods in inquiries conducted in quite different socio-cultural settings.

Our elusive goal is an international, cross-cultural social science, whose concepts, theories and methods are as culture-free as possible. To help reach this objective, we need carefully conducted survey research in many developing nations, so as to form firm empirical bases for comparative generalizations and principles. These would not be just academic principles, but ideally are intended to be culture-free generalizations of direct implications and guidance to development agencies in meeting the needs of disadvantage populations around the globe. The goal is a distant one, to be sure.

If a determined attempt is not made to avoid cultural limitations in concept selection, the researcher in an unfamiliar socio-cultural
setting is likely to find (1) that a high proportion of variables studied will be irrelevant, since they may be characteristic only of industrial societies; (2) that his measurements may be insensitive to the cultural expression of those variables in non-industrialized societies; (3) that important variables may be ignored in the inquiry.

A highly industrialized culture provides an atypical example of the total range of world cultures. One wonders how the concepts considered significant to the analysis of communication and change would be different if the most influential social scientists were Pakistani or Nigerian. For instance, there is much concern today in social science research with the locus of decision-making. This emphasis is perhaps derived from egalitarian ideals that decisions are supposedly a matter of individual free choice. If historically the most influential social scientists were members of the land-holding classes of many Asian or African cultures, they possibly would not have conceived of individual choice among tenant farmers as having relevance to agricultural change, since the landless traditionally have had only limited choice among economic alternatives. Thus in countries in which the capacity for decision-making largely depends on one’s socio-economic position in a hierarchy, it may be sufficient merely to describe the hierarchy.

There are inevitable cultural biases in concepts and theories derived from the rigorous methodologies of Western-oriented social sciences, simply because these concepts were not selected with cross-cultural application in mind. And, so, the national social scientist using imported tools and his foreign counterpart bringing them in must learn to ‘know the territory’, to understand the conditions he proposes to study, to take the respondent’s frame of reference, and to anticipate socio-cultural modifications. This is a first step toward avoiding inappropriate decisions about the choice of concepts and methods.

D. CROSS-CULTURAL EQUIVALENCE

Just as cultural differences can be a prohibitive problem in the selection of concepts, they can also act to impede the cross-cultural equivalence of concepts. In a pure sense, cross-cultural equivalence of concepts never occurs, although approximation is possible (see Chapters II and VI). However, there are recognized techniques that can be utilized to maximize the degree of equivalence.

The problem of conceptual equivalence, the comparable study of
the same concept or concepts in two or more cultures, should be distinguished from the problem of operational equivalence, the comparable study of the same measure or measures in two or more cultures. One may, of course, investigate the same concept in different cultures, but measure the concept differently in each.

For instance, the concept of social status (defined as one's position relative to others in a social system) could, among other ways, be operationalized in some South American countries as the number of afiliados (godchildren), in some African countries as the number of wives, and in some Islamic countries as whether one has made a pilgrimage to Mecca. Thus, the operations of social status are culturally variable, and must be different in each culture in order to measure the same concept. Generally, social scientists feel that maximizing conceptual equivalence is a meaningful goal while operational equivalence is largely impossible. This is often true even in selecting the 'same' unit for sampling in different cultures, as Chapter V indicates.

Because the bulk of survey data are verbal responses, the problem of equivalence becomes especially apparent in attempts to translate from one language to another. The meanings of words are seldom identical across cultures, and sometimes even across sub-cultures. Words, as language symbols, are dependent upon the culture in which they are used. Meanings of specific words vary even when there seem to be directly corresponding terms. Examples are found especially in the lack of comparable meanings of value-laden words such as 'peace', 'democracy' and 'authoritarianism', which possess markedly different connotations growing out of people's dissimilar experiences with the implementation of these words in such nations as Mexico, Italy, West Germany, England and the U.S. (Buchanan and Cantril, 1953).

Anyone who has attempted cross-cultural testing of language is well aware of the deep, bridgeless chasms which separate the linguistically ordered thought-patterns of the people of different cultures (Kluckhohn and Strodtbeck, 1961). For instance, one really can never be certain that the concept measured by a functional literacy test in Brazil (in Portuguese) is identical to the one measured with a seemingly similar test in the state of Andhra Pradesh (in Telugu) or in Uttar Pradesh (in Hindi) in India.

An attempt to maximize cross-cultural operational equivalence is illustrated in the five-nation study by Almond and Verba (1963). They developed one personal interview schedule in English which included
measures of all of their variables. Then, the common instrument was translated into Spanish, Italian, etc., equivalents. Almond and Verba encountered many equivalence problems of a linguistic and cultural nature with this procedure. For example, they could not obtain a satisfactory Spanish word in Mexico that was equivalent to 'local government' in English. Other than Mexico, the countries studied were rather highly developed, and all were predominantly Western European in ethnic background. This cross-cultural similarity undoubtedly aided Almond and Verba's attempt at operational equivalence.

IV. PRAGMATIC INFLUENCES ON DEVELOPMENT RESEARCH

If research has no implication for theory, it lacks even a minimum of scientific method and explanatory power and should not be undertaken. If, on the other hand, research in the context of national social and economic development cannot provide a clear, direct and unambiguous line between results and decision alternatives, it lacks pragmatic validity and similarly should not be undertaken. Within the longer-term perspective of the carefully planned research project, an early research phase may only provide clear information for research-related decisions about future project foci or methodologies. But, nonetheless, the overall perspective of the research is to provide actionable (reliable) and relevant bases for policy administrators and programme practitioners as guidelines for decisions regarding the allocation of scarce resources.

Although we seek to enrich and embolden the theories of human behaviour, the ultimate test of the value of development research is a pragmatic one: its usefulness in practice; in alleviating conditions of poverty, disease, malnutrition, illiteracy and high mortality. By this test, much of the research done in the 'name' of development has been a waste.

Hence, in this book, we define development research as systematic, representative and non-arbitrary information gathered from the 'field reality' of developing nations and aimed at the solution of priority problems or amelioration of unwanted conditions. In this respect, it is 'advocacy' (not arm chair) research which accepts as a professional obligation the role of advocating the implications of data for decision-making.
It is theory-based, but action-oriented. It is research applied to specific problems and not to the general study of basic processes for the sake of theoretical purity. The major objective is, therefore, the reduction of risk-taking in making decisions about the nature, quantum and location of resources allocated to development programming.

A. DIFFERENCES BETWEEN PURE AND APPLIED RESEARCH

In Taichung, Taiwan, the number of annually reported pregnancies decreased one-fifth during 1963. This was considered to be the result of an intensive family planning campaign, based on and combined with careful research (Berelson and Freedman, 1964). The study showed that while most married women preferred no more than four children, they did not know how to use birth control techniques. Twelve different methods for introducing family planning were compared in an experimental design. Within two years after the study started the results were extended to other parts of Taiwan, and before the final report was published some of its findings were known to and applied – appropriately or not – by family planning agencies in all parts of the world.

The Taiwan study is an example of applied (or ‘action’) behavioural research. An example of pure research is the well-known book, The Passing of Traditional Society (Lerner, 1958). Both studies have significant implications for devising strategies for introducing change in developing countries, but the Taiwan study was planned with problem-solving as its immediate and testable goal.

There is a difference between the behavioural sciences and the natural sciences in their applied and pure branches. In the natural sciences, the applied branches are separate but respectable sciences, as is illustrated by medical engineering and agricultural research. In the behavioural sciences, unfortunately, the applied branches such as social work or extension education research are much less developed and are often considered to have a lower status than pure sociology or psychology. The distinctions of pure and applied science are shown as ideal types (Berelson, 1966):
### Pure Behavioural Research

- Aims to contribute to theory and techniques of the discipline.
- Studies any problem.
- Studies a problem usually from the focus of one discipline.
- Restricts to research.
- Requires mainly technical judgement.
- Seeks generalizations.
- Accepts the hypothesis that variables not measured remain constant.
- Looks for the 'basic processes'.
- Content with small differences if statistically significant.
- Tries to say why things happen.
- Attempts to get all the facts.
- Aims at publishing a research report (on the average this can be done about five years after the field work is done).
- Reports in technical language of the discipline.
- Sees sociology, psychology, etc., as academic disciplines.
- Gives rewards for theoretical and technical virtuosity.
- Has as its reference group other behavioural scientists.

### Applied Behavioural Research

- Aims to solve or ameliorate a problem.
- Studies problems with important social consequences.
- Often several disciplines collaborate for solving the problem.
- Often is involved in action or administration, not only in research.
- Requires also a sense of what the situation and personalities can bear.
- Often studies individual cases without the objective to generalize.
- Recognizes that other variables are constantly changing.
- Looks for any variable which makes the desired difference.
- Interested in important differences.
- Tries to say how things can be changed.
- Should attempt only to collect facts which speak reasonably directly to practitioners' decisions, to actionable alternatives recognized in advance.
- Aims at bringing out actionable points quickly in a series of memoranda and discussions with practitioners.
- Reports in common language.
- Sees applied behavioural science as a profession with first allegiance to treatment and making decisions despite uncertainty.
- Gives rewards for solving practical problems.
- Has as its reference group practitioners and their clients.
Pure and applied research are both necessary, naturally, and most social research is somewhere in between both ideal types sketched above. Pure research has a less direct influence on the actions of practitioners, but it can greatly improve the efficiency of applied research by developing new scientific theories and research techniques. In the past, applied research has contributed little to scientific theory. Rather, the principal achievement of applied research seems to have been the provision of facts without trying to explain these facts.

Undoubtedly in applied research, where we study many factors at the same time, explanation is more difficult than in pure research, because the situations and processes studied are more complicated in 'real life'. However, if we can provide a testable explanation for the phenomena observed, practitioners have a much more powerful tool for problem-solving than they gain merely by having a description of phenomena. Therefore, applied research is specifically concerned with real-life explanations. These explanations often make a very valuable contribution to scientific theory, because they show pragmatic limitations of existing theories. The fact that the applied behavioural scientist does not accept the hypothesis that the variables not measured remain constant, and sometimes works with experimental designs, means that he needs more powerful theories than the pure scientist.

In development research we tend to operate at the middle range of testing propositions that lie somewhere between the specifics of raw empirical data and the generalistics of grand theory. In other words, our theoretical basis must be specific enough to be empirically testable, or else it is useless for problem-solving. But our data must test theoretic hypotheses, or else they are irrelevant for knowledge.

B. DEPENDENT AND INDEPENDENT VARIABLES

One problem with our methodology of theory construction is the rather arbitrary specification of variables as 'dependent' and 'independent' in our generalizations. A dependent variable is a dimension of human behaviour which we are trying to predict or explain through its relationship with an independent variable(s). Conversely, an independent variable is a dimension of human behaviour used to explain variation in the dependent variable. Independent variables are sometimes called 'predictor' or 'experimental' variables. Generally in a time-order sequence, we tend to think of independent variables, e.g., age, as 'coming before' dependent variables, e.g., literacy.
Let us define farmers' adoption of innovations, or innovativeness, as the dependent variable and their socio-economic status as the independent variable. Such definitions derive from the purposes of the research and usually, but not necessarily, correspond to the expected time-order in which the variables occur in the real world. In our example, it is assumed that higher socio-economic status may lead to higher rates of adoption of innovations. Although the labelling of variables as independent and dependent implies that they are 'causes' and 'effects' in a time sequence, we seldom are so confident that we have accurately conceptualized reality.

The time-ordering of concepts is especially difficult to establish with survey research methods. Many conceptual relationships probably are interdependent—or intervening. Such interrelationships occur when a small increment in one variable results in a small increment in a second variable; then, the increment in the second variable makes possible a further increment in the first variable, which in turn affects the second one, and so on (Zetterberg, 1965). Suppose, for example, that a little increase in socio-economic status leads to a small increment in innovativeness, which leads to a little higher socio-economic status and so forth. Who is to say which is the antecedent variable in this relationship?

Most development research surveys are correlational analyses of data gathered at one point in time, rather than before-after analyses of field experimental data. This means that the time-order nature of our concepts seldom can be definitely determined. Neither can the 'forcing quality' of one variable on another be easily determined, which is the other important aspect of causal relationships in addition to time-order.

Forcing quality, the degree to which one variable is a necessary and sufficient explanation of the other, can seldom be adequately determined by experiments either, but must rest ultimately on the researcher's knowledge of the concepts he is studying. So causality is not entirely an empirical question, especially in surveys. Thus it is often preferable to speak of pairs of concepts in theoretic hypotheses as independent and dependent, rather than antecedent and consequent. But even the independent-dependent classification is often arbitrary, and changing the labels does not change the problem. Much of social science theory and methodology is relatively weak in terms of our ability to understand social phenomena, predict behaviour and, thus, cope with problems.
Our hypotheses often deal with *pairs* of concepts, while the nature of modernization and development is certainly a mosaic cobweb of interrelationships among many variables. So, we should not forget that we are artificially chopping up reality into conceptual bite-size pieces.

This invites us to look at three levels of complexity in the objectives of surveys. Our illustration is an example of a two-variable relationship. The objective is to determine the nature of the association between the dependent variable, innovativeness, and the independent variable, socio-economic status. Either a positive or negative relationship is hypothesized between the pairs of concepts in two-variable investigations. But in either case the purpose is to establish directional *relationships* between variables (see Chapter IX).

The least complex research objective is to describe; to count the frequency of some event or to assess the distribution of some variable. Descriptive research is particularly valuable in the definition of the existence and magnitude of problems. It is a desirable first step in advance of research inquiry that seeks to understand the reasons for social behaviour and attempts to provide alternative solutions to problems. Although useful for defining problems for further study, too much development research of the past has produced too many data that are only descriptive, not explanatory, as the basis for decision-making.

Research objectives may involve three or more variables. In addition to the dependent variable and the independent variable involved in determining two-variable relationships, one usually has third or fourth independent variables. The objective of three (or more) variable research is to control the independent variables, in order to study their variously combined effect on the relationship between the dependent and all independent variables.

An illustration of three-variable analysis is provided by the possibly intervening nature of land-holding in the relationship of innovativeness and socio-economic status. We expect that size of land-holdings among farmers is positively related to both innovativeness and socio-economic status and that part of the relationship of these two variables may be due to their mutual relationship with land-holdings. A variety of research designs and statistical means are available for controlling the possible effects of intervening variables. For instance, we can try to hold constant the effect of land-holdings on innovativeness and socio-economic status by:
1. Limiting the variability of land-holdings among our respondents by our "sampling procedures." We might sample only farmers who lease or who have single-family plots of land, and exclude all those who are landlords. This results in greater homogeneity in land-holdings and tend to reduce this variable's effect on our hypothesis.

2. Measuring the land-holdings of each respondent in a survey and controlling its effect through such mathematical and statistical means of control as partial correlation, cross-tabulation or analysis of co-variance. Because there usually is so little advance information for sample stratification in developing countries to improve sampling homogeneity (see Chapter V), control of third variables typically is achieved through analysis.

3. Studying the relationship of innovativeness and socio-economic status by a field experiment, in which the dependent variable is measured at least at two points in time and a change in the independent variable is manipulated between these two times (see Chapter IV). For instance, the socio-economic status of villagers might be increased directly by crop-subsidy schemes or even indirectly by improving their accessibility to markets—e.g., by constructing an improved road to the village, by providing regular bus service, or by providing bicycles to certain farmers. As one means, control over intervening variables in a field experiment can be attained by randomization: the farmers who receive and who do not receive subsidies or bicycles are randomly selected. Such randomization theoretically assures us that the two groups are equivalent in land-holdings (and all other possibly intervening variables); hence its effect is removed, even though it need not be measured.

One of the main advantages of survey research (and an explanation for its increasing use in developing nations) is the capability to generalize from samples to population. Both practitioners and researchers seek to apply findings to large groups, to know the variations within these groups, and to explain a wide range of human behaviour. Thus, the generalization of survey results is of great importance for testing theories, yielding problem-solving insights and planning programmes. But there are several roles that surveys cannot perform in testing hypothesis.

First, because survey methods are highly structured, they are often inappropriate for searching for new concepts to explain certain types of behaviour. Early problem formulations are most readily constructed from the researcher's insightful use of less-structured methods
such as case studies, observation and unstructured interviews (see Chapters II and VI).

Also, survey methods are generally inappropriate if one needs to establish the *time-order* of variables. When survey data are gathered only at one point in time, we may not be able to assess the chronological sequence of variables. The field experiment, essentially a treatment applied to the respondents between surveys, offers some of the advantages of the laboratory experiment for determining time-order of variables and for controlling the effects of unwanted variables.

Surveys excel at describing variables and showing *relationships* between pairs of them (two-variable analysis), although we can learn little about their time-order. Survey data can also tell us something about the relationships of independent and dependent variables, but fall short of the experimental approach because randomization can seldom be utilized as a means of control.

The common procedure with survey data is to use statistical tests of significance for accepting or rejecting hypotheses. A basic notion in such statistical tests is that if the empirical relationship between two variables is at a certain level, we may conclude that the chances are, say, 1 or 5 in 100 of incorrectly rejecting the relationship, when in fact it is correct. We also assume that in, say, 95 or 99 of 100 similarly drawn samples from the same population, a similar conclusion would be obtained.

Statistical significance refers only to whether a specified level of probability has been met by the empirical data, or not. The social significance of the relationship between two variables depends on practical utility.

For instance, we might find that a hypothesized relationship between adoption of innovations and the farmer's access to markets is not strong enough to be statistically significant at the 5 per cent level. So we reject the hypothesis of a positive relationship. Yet the fact that we find a positive relationship, even though at less than a 95 per cent probability level, might be useful for policy-makers or development planners in deciding—depending on time, resources, need and policy priorities—whether highway construction and the improvement of public transportation would contribute to the adoption of new ideas.

On the other hand, even if the relationship of innovativeness and accessibility were statistically significant at the 1 per cent level but no funds were available to improve roads, the research results would
have little social significance. Hence, social significance varies depending on the situation in which application is to be made, while statistical significance is a standard basis for accepting hypotheses.

C. INTERDISCIPLINARY RESEARCH

One of the significant, functional characteristics of village life is the high degree of interrelated human behaviour. ‘Functionalism’ is the viewpoint that all parts of a system are functionally interdependent, and hence must be studied in totality rather than separately. Thus, economic activity in a rural village usually is linked closely with religious, political and familial activity. It is difficult to bring about a desired change in one social institution without the consequences affecting all of the other social institutions.

An illustration is provided by Sharp (1952), who traced the multiple effects on an Australian aborigine tribe of the introduction of the steel axe. The apparent consequences included the breakdown of certain status symbols, the rise of prostitution and increasing laziness. Well-meaning missionaries who introduced the axe did not anticipate the many direct and indirect, positive and negative consequences that ensued. Because of the highly integrated nature of village life, the effect of a single innovation introduced in these settings is multidimensional.

While it would be incorrect to think that similar interrelationships do not exist in industrially advanced societies, the social institutions have become much more specialized. Thus, a rather narrow disciplinary focus in social science research in highly developed countries may be more appropriate than in developing nations, where an interdisciplinary approach is more meaningful for a complete understanding of human behaviour (Mahalanobis, 1967).

As we said before, the need in developing countries is for applied and practical research. Typical questions asked by government leaders and programme practitioners are:

(1) How do we increase our agricultural and industrial production?
(2) What factors inhibit villagers from adopting agricultural and health innovations?
(3) How do we change subsistence farming to commercial farming?
(4) How can we reduce migration to the cities and the formation of urban slums?

These questions are asked impatiently, and practical answers are needed. It is obvious that complete answers cannot be given by any single social science discipline. Further, complete answers cannot be supplied on theoretical grounds alone. We need data. So, in comparison with its counterparts in more-developed countries, social science research in less-developed countries should be (1) more interdisciplinary in nature, and (2) more problem-oriented.

The state of academic social science disciplines in most developing countries is generally poor. Exceptions are economics, statistics, demography and, in a few countries, anthropology. This is explained in part by the influence of former colonial rulers. Economics has long been a strong discipline in the colonial European countries. Anthropology was also emphasized in Europe, perhaps to satisfy curiosity about the ways of ‘primitive’ peoples. This intellectual legacy has been transferred to the academic institutions of former colonial countries. Quite naturally, economists and, in some cases, anthropologists have dominated the field of applied social science research in developing countries. Yet, the multidimensionality of the development problems in these countries – whether Ghana or Nepal – limits the success of a single-discipline research project.

Over the past several years, there has been a steady flow of interdisciplinary expertise from industrialized countries to the developing nations. In spite of the many built-in inhibitions that the sojourning scholar has, such as cultural bias, disciplinary loyalty and adjustment strains, this flow of knowledge is generally encouraging. Too often, unfortunately, the outside ‘expert’ is himself a study in mediocrity and neo-colonialism. For whatever reasons, most of the real experts seem to stay at home, with the result that development research tools often are imported in the hands of multidisciplinary incompetents. Interestingly enough, however, the tradition of interdisciplinary research in the United States and Europe is weak. For various reasons, social science disciplines in these countries have grown in virtual intellectual isolation, and in the few cases of serious interdisciplinary efforts the attempts have been made mainly on a theoretical level.

Applied social science research in developing countries is mostly carried on outside of university departments, often by government-
constituted, interdisciplinary research institutes. Sen (1969) details the historical reasons why applied, empirical research in sociology grew in such institutes in India rather than in university departments of sociology, which are oriented mainly to non-empirical, social philosophies. Unfortunately, there is often an appreciable lag between the vigorous activities of the government institutes and the slow pace of interdisciplinary integration at the theoretical level.

From the point of view of interdisciplinary research, the relatively slow emergence of academic social science disciplines in developing countries may prove to be a blessing in disguise. Interdisciplinary research departments may grow easily in situations in which disciplinary chauvinism does not yet exist. The demand for interdisciplinary problem-solving and research, assisted by the pace-setting activities of the government-sponsored research institutes, may produce similarly oriented departments in the universities. One illustration is the case of the government-sponsored research institutes in India. Several have been accredited to grant graduate degrees. In essence they have thus begun to function like university departments with an interdisciplinary character. Also, the staff of these institutes have begun to serve as thesis advisers for graduate students in university departments, thus creating an interdisciplinary thrust to the student's research. As advanced knowledge in each discipline is already available elsewhere, there is less need to repeat the same process of introverted disciplinary growth as happened in Europe and North America.

One of the major objectives of all sciences is to formulate generalizations on the basis of empirical data. This is necessary for consolidation of knowledge and for increasing the predictive power of science. Problem-oriented, interdisciplinary research must, therefore, produce concepts which can have meaning for all the disciplines involved. Without such conceptual sharing, interdisciplinary research will remain only at the fact-gathering stage. So while interdisciplinary research begins with a problem-orientation, there eventually must be a conceptual convergence based on empirical data.

V. TENSIONS BETWEEN RESEARCHERS AND PRACTITIONERS

Too often what is called a multidisciplinary study is, in fact, a set of
independent studies by soil scientists, hydrogeologists, agricultural economists, sociologists, etc., because no team member is able to integrate them. With the increasing specialization in our universities, it becomes more and more difficult to find scientists who can successfully integrate the work of a number of specialists. If the researchers cannot achieve integration, it is hard to believe that the policy maker will be able to do so. Development research is, obviously, interpersonal as well as interdisciplinary. Therein lie many problems.

A. THE PRACTITIONER’S POINT OF VIEW

Programme planning is changing at least to the extent that administrative and programming practitioners are asking for more reliable data for decision-making. The increasing use of applied research represents the desire to improve the reliability of information for decision-making and thereby the certainty with which we plan and recommend programmes.

But, the word ‘research’ carries connotations of academic esoteria. There is nothing esoteric about wanting information for programming decisions. Nonetheless, some practitioners think of scientists as people proving the obvious, as impractical, as bothersome and nosy; as always speculating and hypothesizing, but never having an answer to the practitioner’s questions; and as people who cannot bridge the gap between their ivory tower and real problems. Sometimes these perceptions are valid.

Tensions exist between researchers and practitioners because they frequently neither understand nor value the work of the other. The problems of institutionalizing research in action agencies is more than educating staff to research concepts and techniques. Demonstrable effectiveness in programme improvement is clearly the most important means by which systematic research can weld into the programming process. But demonstration of effectiveness in improving the impact and continuity of a national development programme is a long-term process.

Of course, research is not needed for all programme, and all research does not have to be long-term, large-scale or formalized. But where research is a part of programming, it must be built in as a planned, integral component. Research goes with action; sometimes as evaluation information on programme effects and sometimes as management information for programme technology and logistics.
Practitioners sometimes have the misconception that programming must be halted, disrupted or delayed while waiting for the results of applied research. This is unreal. The urgent reality of development programming is that it must go on, with or without good research. There is both a priori logic and good in programming, e.g., people need medical treatment and the provision of health services reduces mortality and morbidity. How most effectively to accomplish these goals is, however, another matter.

Some programmers also feel that the researcher works only on long-term projects, exclusive of immediate problems and needs. That, too, is a misconception. An applied research objective may require three weeks and $250 or three years and $250,000 to achieve, depending on the nature of objective. But in either case, applied research is undertaken with a long-term programme and research perspective. Immediate, provisional answers can be related to immediate problems, but the purpose of research is to continue incrementally to add to the storehouse of reliable information, steadily and comprehensively improving programming over time.

Programming cannot wait for belated research ‘truth’, no doubt. Since practitioners feel they cannot wait for long-term projects, research often fails to guide programming objectives, because short-term impact is often difficult to evaluate. At best, sometimes all that can be accomplished in the short run is to stop ill-conceived research and build a minimum level of competence and comparability into project designs that are undertaken. However, while we distinguish applied and basic research, if a research project has no implication for theory, it should not be undertaken. Programming by experience alone is better than programming the implications of bad research.

Time duration does not distinguish applied and basic research. Time, as methodology, is a by-product of objectives. The real question is whether a project meets its specific action objectives. Thus, the built-in, planned immediacy and certainty of feasible ‘action’ following research is the criterion of distinction.

Practitioners often tend to view research as a one-time expenditure, rather than as an investment in future programming. So long as ‘research’ is an expenditure, it is marginal to programming. When research becomes an investment, it is guidance to programming. Although financial resources obviously are critical constraints on research, money per se is a consideration after the definition of attainable objectives within the framework of an organization’s policy,
its overall programme priorities and commitments. Budget allocations, whether for programming or research, derive from objectives, not from how money can, should or must be spent. Financing becomes, of course, the eventual reality constraining objectives. But, there is no single methodology nor an inflexible strategy for applied research. Projects are of many types, costs and time durations depending on the particular question and objectives at hand.

Regardless of the variations of research, the administrator's view is that money for research should be a subordinate and small part of the budget in general. 'Subordinate and small' assistance implies a more or less constant proportion of allocations across programmes or a known ceiling. These erroneously assume that problems are proportionate to allocations, that needs and demands are proportionately constant across problems, and that problem dimensions and significance are recognized in advance.

Administrators complain that research should be more 'practical' and show direct benefit to 'significant' numbers of programme recipients in a 'reasonable' period of time. Assuming that all conditions of good research are fulfilled, this is a difficult principle to apply without a systematic and reliable basis for evaluating time, material and financial cost-per-unit of benefit.

Comparative cost-effectiveness can be determined by testing, say, different modes of transportation for the distribution of equipment, but cost-benefit is more complex because benefits are social, psychological, physical and even futuristic. Judging cost-benefit remains speculative until there is a workable evaluation system and a gradual rationalization of benefits by type, levels, numbers, cost and time (see Chapter IX).

Project design is administrative, programmatic and technical. Administrators and programmers design and evaluate research projects in terms of commitments and priorities. Researchers try to help ensure that projects are executed with realistic and manageable objectives. As such, research must have a direct and effective line to programming. Programmer-administrator-researcher communication is most effective within an acceptable and workable policy system. As a sub-system, applied research is most productive when, as appropriate and possible, it is built into projects at the design stage.

B. THE RESEARCHER'S POINT OF VIEW

Researchers often seem to practitioners to lack the ability to under-
stand practical problems. The researcher's empathic ability and willingness to take the role of the practitioner depends, among other factors, on the situation in which the researcher works and the status given to that work.

Invariably, pure behavioural science research carries higher prestige than applied research. This is an especially important factor for academic researchers who take short-term assignments to developing countries. After they return home, few colleagues will know whether they were successful in solving practical problems, and few will care. Their promotion is much more influenced by the number of scientific articles and books they publish about their research abroad. No doubt, working with practitioners takes much time and produces few publications.

Sometimes the lesser prestige of applied research leads the researcher to dissipate time and energies tackling extraneous basic research questions—answers to which are seldom immediately germane to practical difficulties of development programming. Perhaps because of the lower prestige of applied research, technical assistance programmes in developing countries attract researchers and consultants with relatively little academic status or research experience. Worse yet, the preference for pure research by many foreign consultants influences the research done by the host-country nationals.

Part of the problem for the applied researcher is, whether he sees it consciously or not, to develop a satisfactory reference group for his work. The researcher will probably do better research as the frequency of his communication increases with practitioners and their clients. As he increasingly appreciates the relationship and the compelling significance of the problems under study, he will place higher value on his contribution to the solutions of those problems. In time, he might give somewhat less weight to the opinions of outsiders—heretofore his pure science colleagues—regarding his work.

Good research in itself is no guarantee that practitioners will use the findings. Stimulating the use of results can be done in an indirect way by improving scientific theory, of course, but we are interested here in the more direct influences of applied research on the work of the practitioners. Some people believe that this is rather simple:

Utilization (of research findings) is often thought of as a process of telling people, writing better pamphlets, drawing better charts, making more and better teaching films, cranking up transmitters
in the mass media. This is clearly an inadequate picture (Schramm, 1965, p. 18).

There are many reasons research reports lie buried in confidential files years after completion. Sometimes action agencies are afraid of programme critique and try to suppress reports. An example is Arensberg and Niehoff's well-known book, *Introducing Social Change* (1964). After 5,000 copies had been printed for an agency's internal use, high-level staff members noticed that various examples were cited in which programmes had not achieved the desired results. Efforts were made to prevent distribution of the book for fear of harmful publicity. A year later, influenced by outside pressure, the book was distributed and permission was given to publish it commercially. Now, thousands of copies have been printed without harmful effects to the agency; but many technicians working in developing countries both inside and outside the organization have profited from reading this book, just as have many students.

There are also many reasons why projects fail. In one Asian country, an experimental project was halted short of its goal of developing a prototype micro-level planning methodology. Several field teams were created for extensive data collection in different parts of the country. Some teams started work in 1969 and some more in 1970. Finally, in 1971, authorization was given for establishing a central technical office for designing the field work, helping to train field teams, coordinating data processing and devising planning strategies. More teams started work in 1971 and a few more in 1972, while staff were still being added to the central office. The central technicians never could catch up with the work undertaken by different methodologies in the field nor develop the capability to process and interpret many thousands of data cards. In 1973 the project was ordered to be terminated.

In those relatively few instances in which it is found that first-rate applied research is being done in developing nations, invariably the researchers are in very close contact with the staffs of action programmes and with the people for whom the programmes are intended. A researcher who serves as a staff member of an action organization tends to be highly concerned about the problems of the organization. It is both easy and in his self-interest to communicate his research findings to his practitioner associates. One problem for him, however, is that some of the decisions of top-level policy makers may
have undesirable effects on the work of the organization. For an outside researcher, in such cases, it is often easier to bring the problems to the attention of top officials, than it is for one of their own subordinates (v. d. Ban, 1963).

With which level of practitioners the researcher communicates depends on the importance of his study and the prestige and relations he has. A well-known social scientist working on a large agricultural study is perhaps in a position to discuss his research with the Minister himself. A student working on a thesis will have to be satisfied with discussions at a much lower level. Nonetheless, both types of researchers must try to cooperate with practitioners whom their research concerns in the organization, if they want to see their findings utilized.

There are difficulties involved in working with top-level policy makers. They often lack the time to give serious attention to the problems of the researchers. This is especially true in developing countries where there are comparatively few experienced and well-trained administrators and often a resistance to delegation of decision-making. Another difficulty is that unwary top-level administrators believe that they know the problems of their field staff, whereas in fact they often do not because of improper feedback. Inadequate feedback is an extremely serious problem when decisions regarding budget allocations, geographic programme distribution, projected targets, distribution of supplies, transportation needs, staff assignments and the like are made at the upper levels of the bureaucracy on the basis of reports from the field workers.

Staff members often do not dare pass unpleasant information back to their superiors. Such information, even if it is true, may be perceived as a reflection of the administrator's incompetence and thus jeopardize the position of the staff member. If staff are afraid of recriminations and see that the researcher has very close contacts with top-level policy makers, they are reluctant to talk freely. It is important for the researcher to develop close contact with people at all levels of the hierarchy. Rapport at all levels is necessary for the execution of decisions based on the research. It is much easier to gain cooperation if the staff does not fear the researcher on whose data the decisions are based.

Researchers can give countless examples of staff misunderstandings and misperceptions that prevented attainment of the objectives of programme evaluation. Placing obstacles in the path of the researcher is not, however, a behaviour peculiar to indigenous agency
staff or government officials. It also occurs among academicians, officials and technicians working with foreign technical assistance programmes.

In one African country, the administrators of an agricultural development programme, contracted by an American university, succeeded in preventing an American representative of an inter-university consortium from coming to the country to evaluate their work. They persuaded the American director of the local economic research institute, representing still a third university group, to deny institutional affiliation to the would-be researcher. After two years of operation, the administrators claimed that a preliminary evaluation of programme effectiveness would be premature. This was a bogus argument. The purpose of the evaluation was not to measure programme effects, but to assess progress in the development of a 'counterpart' system with staff members of the Ministry of Agriculture.

In another country, American Peace Corps administrators thwarted efforts of a team of evaluators sent from Washington to test the language proficiency of the Peace Corps volunteers. The evaluation team left the country with less than one-half of their testing completed. The administrators perceived the language tests to be an evaluation of their own administrative proficiency, and consequently reflect poorly on them should they or several volunteers fail. Not a single administrator participated in the evaluation, the purpose of which was to provide guidance for the development of language-training courses for future Peace Corps volunteers. Naturally, such evaluation research is often threatening to the practitioner who sees himself and his work under scrutiny. Building good working relationships requires a large investment of the researcher's time as well as adroit interpersonal skills.

Implementation of research findings frequently involves additional work for the practitioner. It is unfortunate that the use of new and more efficient programming methods is not always compensated with rewards, despite the favourable implications of research findings for national development. Such is illustrated by a study made by a commercial research firm intended to increase the efficiency of a large farmers' cooperative in one African country.

The study was made at the request of the cooperative's administrators. By the time government officials in the Ministry of Cooperative Development learned of the study, there already was strong
evidence that a major factor undercutting efficiency was the unethical collaboration of various government and cooperative officials in distributing scarce resources in a way beneficial to their personal interests. Implementation of the research findings would have required revision of the distribution system, thus involving much difficult work for the Ministry staff. Probably most important was the feeling that the publicity created by the research report would have unfavourable repercussions for the Ministry's administration. Consequently, rather than taking corrective actions, Ministry officials erected a variety of bureaucratic barriers questioning the legality of the researcher's entry into the country, and succeeded in halting the research project when it was half-way finished.

Hopefully more important than political problems in the use of research is the appropriateness of the research itself. The latter can be more easily corrected by the researcher in the design and execution of the study. Specifically, if one conducts a study involving an experimental action programme, it is difficult for the sponsoring agency to utilize the findings if the programme cannot be replicated more or less in the same way on a larger scale.

It is a particular weakness of the research design if an experiment employs an extremely capable and motivated staff, whose special skills are not available at the conclusion of the project; if the research staff gives unusual technical assistance to the action agency, which is withdrawn at the conclusion of the project; or if the study is based on such a specialized segment of the population, that the results cannot be generalized for the relationships being studied. The last problem is especially serious in developing nations, which are characterized by many heterogeneous sub-cultures and where the presence and influence of many variables are unknown for the purposes of control.

As cited later in Chapter IV, an example of an experimental programme that would be hard to replicate on a large scale is provided by a study in Peru. This study was concerned with the effectiveness of radio broadcasts in changing the behaviour of rural villagers. Every day during a nine-week campaign, four hours of radio time were devoted to information about four new community-development practices. The content of the radio programmes was based, to a great extent, on results of interviews and discussions with people in only two villages. Certainly, a national programme would be much different: less time could be devoted to such educational broadcasts; aspects of rural development other than just four practices normally
would be included; the broadcast presentations would have to be adapted for the comprehension and interest of people of different life styles than that typified only by the two villages; and the people interviewed for the programme usually would be unknown to the national audience.

Of course, one of the basic problems in conducting experimental projects in developing nations is that the costs and levels of control required are inordinately high, with the result that most experimental studies are done on a small scale with limited generalizability. In all cases, however, applicability of findings is a significant, often determining factor in the formulation of the research problem.

C. A COMMON POINT OF VIEW

The manner in which the research problem is formulated, and whether conclusions of research reports are drawn and implemented, depends partly on the advantages and disadvantages perceived by the practitioner. Naturally, the practitioner's use of research depends on whether he has more confidence in the research findings than in his own judgement. To achieve such confidence, the researcher and practitioner must at least share a common frame of reference.

For research generally, but for survey research specifically, this book is written to help promote understanding between applied researchers and development programmers and administrators. It is intended that the book be useful to researchers whether involved in long-term, costly research programmes or simply conducting a one-village survey for a graduate thesis. We aim at both national and foreign researchers, feeling that for the most part researchers in developing nations are 'outsiders'—not indigenous to the sub-culture under study. In this respect, a national researcher is an outsider when conducting research in a part of the nation that is not his or her home.

This book will not take up all issues nor even try to answer all of the most important questions. In common-sense terms and without statistical complication, we are trying to explain salient aspects of the process and limitations of survey research in developing nations. We are speaking, therefore, non-technically to researchers and to practitioners, would-be researchers and would-be practitioners.

Our principal concern is not with the successes of surveys in the cause of national development, but with the weaknesses and biases of the approach that restrict its capability as a problem-solving tool.
We feel that as researchers acknowledge and practitioners understand the limitations of the methodology, they have the initial basis for sharing a common frame of reference both in terms of what surveys can and cannot accomplish.

REFERENCES


