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SYMPOSIUM ON
**SOCIAL RESEARCH
FOR DEVELOPMENT**

PROCEEDINGS

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THE AMERICAN UNIVERSITY IN CAIRO**

SOCIAL RESEARCH FOR DEVELOPMENT

by

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CORE PAPER

Presented at

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When I was invited to present the core paper of this symposium on "The role of research in development", I assumed that I was being asked not to collate what all kinds of people - both scientists and amateurs - have said and written about development research, but rather to express a personal point of view - the result of over a quarter of a century of social science research experience within developing countries. I shall therefore try to encapsulate and present within the next half hour or so what I think I have learned in the process of slowly and laboriously trying to build a social research institution at this university, with the aid of all the colleagues who are still working or who at one time or another have worked at the Social Research Center - a number of whom I am delighted to find as participants in this conference - and with the help of all those who funded us, praised us, challenged us, or even fought us. As many of you know, the main effort during the years in which I was administratively responsible for the Center was directed towards developing an institution with an indigenous research capability that could independently, and in collaboration with imported talent, help answer some of the cru-

cial questions that arise as a society like ours tries to chart its course and plan its future within the turbulence of rapid technological and social change.

Undoubtedly colouring what I shall be saying concerning the "what" and "how" of social research for development will be my international experience of the last five years. Before joining the United Nations Research Institute for Social Development to direct a research project that kept me commuting between Europe and three other continents, I had been quite familiar with the state of academic social research in developed countries, especially the United States and was, I believe, well-informed about the state of the art in Egypt. Undertaking research under the umbrella of the United Nations gave me the added opportunity of learning about the variety of activities the non-academic world labels as "research" and what people in other walks of life understand of and expect of the behavioural scientist. It also made it possible for me to check within the context of other countries such as India, Mexico and Niger, some of my thoughts and conclusions about the manner in which social research should be undertaken so as to serve the needs of a developing country and to learn further about the challenges and serious constraints that stand in its path.

I would like to start my discussion of development research with a few simple and obvious statements. Social science is the study of human behaviour; hence, any process involving human thought, sentiment and action is a legitimate area of social inquiry. Development, no matter how defined, is undertaken by and for human beings. Therefore, there should be no great difficulty figuring out how social research relates to development; it can do so in innumerable ways. The topics of this symposium represent one possible list of priority areas deserving the attention of social researchers. I expect that the rest of the speakers, all of whom are highly knowledgeable about their respective subjects, will point out which among the countless research questions that can be pursued are, in their opinion, the most urgent and most relevant for developing countries and, perhaps specifically, for Egypt today and tomorrow.

For my part, I shall try to raise what I believe are fundamental issues that, no matter what topic is chosen for investigation, need to be addressed

and resolved if the significance and validity of social research are to be assured. Perhaps some of my fellow speakers will be referring in their presentations to some of the same issues. That, from my point of view, would not be redundant but would, on the contrary, be most salutary; for good research obviously does not take place just because we have a list of good topics and significant questions. It has other requisites. It needs good researchers, clear conceptualization, appropriate methodology and adequate institutional framework. It also needs, which I have not forgotten but which I deliberately put at the end of the list, funding. Why at the end? I shall try to answer this question later on.

There are many constraints and problems facing social research today, especially research focussing on the complexities of social change and development. To them I plan to devote much of my talk, but let me begin by complying with the advice of an old song refrain and "accentuate the positive". The most positive contemporary development in favour of the social sciences is the widespread realization that the alignment of technological advances and efforts to change society with existing general political and economic theories of development do not necessarily produce the desired results. They often butt against human feelings, values, decisions and responses that had not been given enough attention in the neat logic of the theoreticians or in the carefully drawn plans of the practitioners of development. Insofar as the poorer nations of the world are concerned, which I assume are the main objects of concern of our conference, years of so-called development effort seem to have left them with their basic social and economic problems unresolved. The majority are still faced with a formidable challenge: how to improve standards of living and meet people's rising expectations - often strongly influenced by consumption patterns of the more affluent societies - and satisfy demands of national pride and security under conditions of limited and poorly distributed resources; in adequate social, managerial and political institutions; lack of skilled manpower, inadequate technology and overwhelming competition from the more advanced nations, and the rich ones for that matter, find themselves grappling with another knotty problem: how to make the appropriate choices and to produce harmonious development in the middle of the cacophony of clashing values, ideologies, interests, influences and pressures - coming both from within and from without - that seems to plague most contemporary societies.

Many fields of science and technology obviously need to join forces to help meet the fundamental challenges confronting developing countries. Within such a concerted effort, the social sciences have an important, if not always clearly defined, role to play. There is today a growing recognition of the need for social science knowledge to understand the determining influence of human motivation and behaviour in all phases of the development process, from choice of strategy, to programme execution, to goal attainment. With the introduction of social or technological innovations - innovations, that is, insofar as the specific society is concerned and not necessarily the world at large - one frequently finds responsible persons expressing their need for some factual knowledge and insights as to how people are affected by and are affecting programmes of change and development. There is also an insistent demand for recommendations as to what needs to be done so that the task of the planner and implementer of development programmes is not complicated or made impossible by human unpredictability. This I have found to be true in relation to every field of development with which I have personally come into contact, be it industrial development, the introduction of new agricultural technology, the development of new lands, urban development, the planning and building of new human settlements, environmental protection, population planning and control, improvement of basic community services, integration of women or other less-favoured groups in development, etc..

In effect, there is a multitude of questions relating to development that are looking for adequate answers from the social researcher. Before suggesting a few, let me just state in very simple terms the general perspective from which, I believe, the human factor should be viewed in development research:¹

1. In all developmental processes, the human being stands at the center, both as the subject or decision-maker and as the object or recipient of all the direct and indirect benefits and costs. I define decision-makers as all people whose values, options and actions affect what happens to any specific development situation. They may be the planners, administrators, experts, politicians, legislators, who have a formal responsibility for local, region-

¹ The following paragraphs are from a monograph by the author on "Land Development in Egypt's Western Deserts" to be published by U.N. Research Institute for Social Development, Geneva.

al, national or international development. They may also be private groups or individuals from various levels of society who influence the development process by the responses they make to what is happening around them - responses that are largely determined by what they perceive as new opportunities or as new dangers, and what they evaluate as beneficial or intolerable. These would include the private enterpriser, the opinion maker, the ideologue as well as the ordinary citizen.

2. Any human society, no matter how homogeneous or simply organized, must be looked at as a complex whole made up of functionally interrelated parts and encompassing groups differentiated along a number of parameters - such as, to mention but the basic ones, age, sex, level of knowledge, division of labour, relative wealth, social status, geographic location, ethnic or cultural affiliation. Any major change introduced into any aspect of the life of a society may have important repercussions on other aspects, and may affect differentially various groups and individuals within it.
3. Economic development programmes and the technologies that underpin them inevitably induce modifications in the natural as well as in the social environment. Some of the impacts may be intended and constitute part of the stated or implicit goals of development; others may be unplanned side-effects. Regardless of whether intended or not, major development activities will set in motion or accelerate certain social processes. They will necessarily have demographic consequences; they may induce population movements and changes in population composition, growth and possibly quality. They will unquestionably have an impact on the size, the structure and the functioning of human settlements. They may also have serious implications for the socio-economic structure and for the distribution of power within the society, and may effect negatively or positively the ability of various social groups to meet their respective basic needs.

Although none of the ideas I just expressed seems to me to contain anything controversial but is rather a statement of the obvious, I make a point of emphasizing these ideas because in practice the obvious is frequently overlooked. It is often forgotten by the development strategists and executors. It is, likewise, at times ignored by the researcher, either because of inabil-

ity to deal with the complexities of reality or because of the temptation to sacrifice the obvious-but-true for the sake of the original but not-so-very-true.

Following this very brief presentation of the general premises from which I personally approach development research, I now turn to the list, by no means complete, of questions I feel social researchers should attempt to answer in order that the introduction of technological and social innovations for the amelioration of the human condition not produce disappointing or even reverse effects. These, as may be noted, are closely interrelated and not always mutually exclusive:

1. What and whose perceptions and values influence the choice of development strategy, programmes, and means of implementation?
2. How do development processes and accompanying changes - political, social, economic, technological or environmental - affect or are affected by pre-existing social structures, institutions, distribution of power, and patterns of thought and behaviour - both initially and in the long term? What socio-cultural tensions and conflicts result and how are these resolved?
3. What are the impacts of planned development programmes and activities - intended or unanticipated, positive or negative - on people from different strata, social groups, sexes or age categories? Who is reaping what benefits and who is sustaining what social, economic, political or psychological costs?
4. What perceptions and values influence people's responses to new ideas and their assessment of the costs and benefits of new life options, new institutions, new ways of doing things? Why do some people accept certain innovations, others accept them only partially or in a modified form, while others still reject them totally?
5. What are the side effects of development activities on aspects of society and of the physical environment other than those directly concerned?
6. Are planned programmes of development achieving their stated goals, and, if the latter happen to be incompatible with the researcher's value system, are they achieving what from the researcher's point of view are the real goals of development? If they are, what are the facilitating factors; if they are only partially or not at all, what are the obstacles and constraints -

socio-cultural, political, economic, managerial, technical, environmental - whether or local, regional, national or international origin?

7. What are the means utilized for the implementation of planned programmes - the administrative and managerial structures, the decision-making procedures, the relationship and modes of communication between the administrator and the administered - and how do these affect the ability of the programmes to meet their immediate targets and attain their long range objectives?

To provide answers to these questions, multi-disciplinary research utilizing a variety of research methods is indispensable. There is a place for macro-level analyses on large aggregates, using quantitative methods and statistical indicators for diagnosing the overall development situation at national and sub-national levels. There is an even greater urgency for micro-level empirical research that investigating in depth and longitudinally in time the manner in which the social processes, set in motion by development activities, manifest themselves at the field level, where the action takes place, and modify the lives of real people. Insofar as action research is concerned the research teams should include not only academics from various relevant disciplines, but also technicians, planners, administrators and representatives of groups directly affected by the development. For the members of such teams to be able to communicate intelligently they need to work jointly over a long period of time to begin to talk in the same idiom.

The importance of the questions that social research can theoretically help answer and the heightened recognition of the need to understand the social factors influencing processes of planned and unplanned change should the behavioural sciences with a great opportunity for contributing to the development of human societies. Unfortunately, however, we appear to be somewhat handicapped and not always able to deliver the expected goods. We even find a certain amount of disillusionment and skepticism emerging as to the real value of the contribution we as social scientists are in effect able to make. Many accuse us of being vague and incomprehensible, under a thick layer of jargon, or of being verbose without enough focus or concreteness to provide a basis for decisive action. I have also heard people from other fields say, "The social

scientists like to tell us what is wrong with what we are doing but hardly have anything to say as to how to do it right," or, "We had a social scientist in the team, and we took his recommendations. We still ended up with serious social problems."

These criticisms are partly justified because some of the fault, in my opinion, is not in our stars but in ourselves, and partly unfair because some of the difficulties do originate outside our field. As a matter of fact, the internally and externally induced difficulties are closely interconnected, for it is often by bowing to external pressures that we undermine the viability of our field. However, since our survival concerns us more than it does anyone else, it is up to us to try and keep our house in order and to resist any detrimental external influences that tend to impinge on us.

At this point I shall review what I consider the major constraints and obstacles - whether self-inflicted or caused by others - facing the social sciences today. If my talk henceforth will seem to over-emphasize the shortcomings of our field, it is because I believe that only through a sincere analysis of these can we possibly hope to begin "to eliminate the negative", as the old song refrain also advises.

Insofar as development research is concerned, one of the first obstacles preventing social scientists from making their full contribution is the lack of clear definition and conceptualization of what is development and what is scientific social research. Since the definition of "development" presents the more serious problem and since the lack of clarity surrounding the concept can and does affect the quality and usefulness of related research, I shall treat it first.

There are reams of publications trying to theorize about and define development. Besides all the economic writings on the subject there is an extensive sociological literature full of controversies on what is "real development", "balanced development", "integrated development", "unified approach to development", "social development", etc.. At the base of some of the debates are superficial disagreements about the meaning of the word "development", which, in my opinion, would vanish if greater clarity of thought prevailed. Behind some

of the controversies, on the other hand, are also genuine philosophical and ideological issues that need to be addressed. There is no doubt that a researcher cannot undertake significant research on development without first defining what he means by the term and without elucidating to himself and to others the philosophical basis from which he is viewing and evaluating the phenomenon. I am, however, rather weary of some of the long-winded and muddled discussions that abound in the literature about value-laden definitions of development that confuse rather than guide the researcher and that divert the attention from more urgent research work.

I, myself, prefer to use, for heuristic purposes, the following simple definition, with which few people would have any quarrel: development is any activity purporting, or process involving, the improvement of existing conditions of life whether at a local, regional, national level. This definition, I am quite aware, skirts around but does not resolve the value questions that arise as soon as the researcher attempts to determine, as he must, what specific changes should be considered "improvements", how these should be brought about, who would benefit from them and when - now or in the long future - and what or whom should be sacrificed in the process. A researcher who does not try to answer for himself these questions cannot, in my opinion, undertake any meaningful evaluation of general development trends or specific development programmes.

A student of societal change, even when undertaking a purely descriptive piece of work, needs to have a conceptual framework that reflects his assumptions, his values and his special definition of "the good life". It will guide his selection of research problems - the principal questions as well as the situation and people he would wish to investigate. Should the researcher want to go beyond description and begin to evaluate and make recommendations about development strategy, he decidedly must have philosophical premises on which to base his judgements. To remain a scientist, though, he needs to be explicit about his value orientations and be careful not to present his theories, assumptions and hypotheses as facts and as scientifically substantiated conclusions. In his search for understanding he needs to exercise as much self-questioning and objectivity as is humanly possible, for it is very easy for bias to creep into a social scientist's interpretation of human behaviour. No one can look

at his own or other people's actions and thoughts with pristine eyes uncoloured by the convictions and sentiments that cultural and social experience engrain in him over the years and that tend to remain subliminal. The objectivity I am pleading for is not that of a demi-God, but that of a scientifically trained person who is deeply concerned with understanding and communicating reality as honestly as possible.

Scientific objectivity, however, does not seem to be currently a la mode in some sociological quarters. This brings us to one of the main weaknesses we find today in development studies. A great deal of the writings on the subject, encouraged by the type of world we live in, are unfortunately, highly politicised, jargonized, sloganized and - if I may be allowed to add a new verb to the English language "banalized" out of much of their significance.

Responsible for these ailments are two types of guilty parties. There are, first of all, those opportunists, among whom are some professional but also many amateur social scientists, who deliberately use social research as a facade to serve political ends or other narrow interests at the expense of scientific integrity.

Discourse on development tends to be highly politicized because the ends and means of development are at the very heart of political concern and decision. All the major questions researchers raise about "how" and "for what" are the very same ones over which people have been fighting in the world's political arenas - not only with words but at times even with guns: What should be the goals of development? Is it just to achieve economic growth - that is, a rise in per capita income and GNP and an increase in available goods and services - and let the fittest survive? Or is it also to meet people's minimum needs as well as achieve greater equality in the distribution of goods, services and power? If equality is desired, how much equality - total equality or should some remain more equal than others? If partial, what is the ceiling and the floor beyond which inequality would be intolerable? Is development more than economic growth and the better distribution of "the good things of life"? Is it also freedom of expression and choice, protection against arbitrariness, and perhaps spiritual growth? Should the objectives be clear, which of them are the first priority and which ones can be laid aside for a while or totally sa-

crificed if necessary? And, most important of all, how are the objectives to be attained: by revolution and destruction of existing social structures or by gradual evolution and change; by collectivization and/or nationalization or by free enterprise; by more bureaucracy or by less bureaucracy; by greater central planning and control or by more decentralization and more local initiative; and so on.

It is around the choice of development priorities and of development means that some of the more violent ideological controversies have been raging. Since development research involves, among other things, the evaluation of objectives, priorities and means, it is not surprising to find that more polemics than enlightenment characterize some of the discussion and that many publications on the subject exhibit strong political bias. Some writers are probably unwittingly biased; but there are others whose works clearly do not aim at the scientific world but masquerade as scientific papers so as to be effective media for propagating specific political ideologies and slogans among a more gullible pre-professional and non-professional public.

Ideological bias and sloganeering not only mar our image as scientists, they also invite or are the pretext for more political control and pressures against social researchers - sometimes subtle but often direct and even brutal. In many countries we find strong restrictions placed on social research, particularly in the area of development, because of political implications of research results and fear on the part of the people in power that the knowledge may be exploited by their rivals or by real or potential external enemies, that is, other nations that may be tempted to impose themselves economically, politically or militarily. Because of the tendency of society to want to control the social researchers, it is imperative that the latter stick to scientific rigor. There is no better protection for the researcher, in my opinion, than the respect that scientific integrity inspires.

The other type of opportunists, who have been as detrimental to social research as the political manipulators, are persons with social science degrees who happen to lack the intellectual equipment, the methodological skills, the genuine curiosity, the energy, or perhaps the right circumstances and facilities to undertake scientific, empirical research on the complexities of devel-

opment, but who feel the desperate need to get on the bandwagon and publish on trendy subject or perish. Instead of undertaking real research as it is defined by the dictionary - that is "a critical and exhaustive investigation or experimentation having for its aim the discovery of new facts and their correct interpretation, the revision of accepted conclusions, theories, or laws in the light of newly discovered facts, or the practical application of such new or revised conclusions, theories, or laws ..." - they find it more comfortable and quicker to produce publications out of secondary sources or by superficially picking the brains of those who happen to be close to the primary data. They tend to collate what others have said or written on a subject and produce what they like to label as "syntheses", some of which I would hardly qualify as anything more than glorified undergraduate term papers. Sometimes, because of the absence of a teacher to set standards, these works are not even as good as the better students' papers. Many of these authors do not worry much about data verification or the niceties of scientific logic, and deliberately neglect to cite their sources so as to give the reader the illusion of originality. Borrowers are also very quick to pick up and propagate the latest slogans and academic jargon, for they need to remain fashionable, and thus help fill the literature with uninspiring banalities that confuse rather than elucidate development issues.

Since there are more imitative than inspired writers, some of the same second hand notions tend to appear again and again in the literature; and, as we all know, the mere repetition of an idea in print tends, unfortunately, to bestow upon it the aura of scientific respectability. The trouble is that synthesizers and borrowers are sometimes seen and heard more often than original thinkers. They are able to get themselves to every conference and grind out papers at an incredible speed. This is because they do not have to go through the time-consuming and painful work involved in producing and analyzing data based on direct observation of human behaviour or on communication with real people in real development situations; nor, as is often necessary in developing countries, do they have to help in the creation of the research capability and infrastructures that make development research possible.

I have not been astonished to find that the authors of "quickies" and of ideologically slanted works, whom I have personally had occasion to know, are

not interested in scientific verification. They are sometimes even strongly opposed to and deliberately discourage the use of field methodologies that are likely to help check the validity of the assumptions and beliefs they prefer to call truths. They mount aggressive campaigns against these and some strongly discourage their students from using them. The tactics and types of arguments they use to discredit certain methodologies are very similar to those used by politicians against their opponents. They highlight well-known limitations of specific research methods while carefully avoiding to mention their strengths or offering alternatives devoid of weakness. They will advise categorically against quantitative methods, for example, by exploiting the fact that surveys may give misleading results unless carried out on the basis of a great deal of direct knowledge of the specific study situation and under conditions in which interviewer-interviewee relationship is one of trust and respect; or that statistics can lie if not used very carefully and honestly; or that the results of research can be used in ways that do not fit the code of ethics that some social scientists have worked out for themselves; and so on. They will not point out, of course, any of the following: that research methods are merely tools to be used at the discretion of the researcher; that they have neither ethics, political colour nor analytic ability; that if the investigator has intellectual acuity and uses his methods discriminately and artfully he can produce great works, but if a researcher uses a method clumsily and inappropriately nothing in the method itself will make his production worthwhile. In other words, the method is only as good as its user.

It is disturbing to find that the demands of the market tend greatly to encourage the production of "quickies" and syntheses. There is a voracious demand from various types of international organizations, conference organizers, editors of journals, and even some funding agencies for papers containing the latest wisdom on how to proceed with development, and what is wrong with what everybody has been doing so far for rapid world-wide propagation of this information, particularly to developing countries. And there are many who are willing to oblige. Requests for sociological knowledge also come from people from other fields who are aware of the difficulty of handling the human component within the development areas for which they are responsible as technicians, planners or bureaucrats, and who want a few easy and precise formulas for deal-

ing with it swiftly and painlessly so that they can get on with their main business. We, as social scientists, are obviously unable to do that, but some fear that by admitting our limitations we risk being thought inadequate. In reality, by accepting to do the impossible, many have run the worse danger of proving that they are indeed inadequate.

Behind this problem of over-expectation is a genuine misunderstanding - for which both we, the social scientists, and the consumers of our knowledge are responsible - as to the nature of the phenomena with which we deal, the requisites of good social research, particularly applied research, and what the social sciences are or are not able to deliver. As latecomers who have been reluctantly accepted as legitimate scientists, the social scientists have, understandably, had to make a special effort to convince the skeptics about the value of the knowledge at their disposal and of the insights their research can produce. We had to insist that human beings are natural phenomena and can be studied as such; and, if given a chance, we could help avoid some of the major problems caused by human behaviour which complicate the tasks of the engineer, the physical planner, the agronomist, the bureaucrat, the industrialist, the economist, etc.. While trying hard to convince the rest of the world that we do indeed have knowledge of importance to offer, I am afraid we have sometimes oversold ourselves as scientists - a term usually understood to mean people with exact knowledge, and as a result other scientists, technicians and practitioners have come to expect from us the precision that the word "science" connotes.

We are not a science, however, like any other science, and we should not be reticent to say so. We have not perhaps been explicit enough about the fact that human beings, although differing from all others in that they do not behave in terms of immutable laws like the physical world nor largely by instinct like the rest of the animal world, are natural phenomena. What we learn about the behaviour of a specific group can give us general indications but it does not tell us exactly how another group will behave under the same conditions nor how that group will behave in the future if conditions do change. There are no short-cut and easy answers at our disposal. We must be willing to admit that we are unable to recommend action in relation to development situations unless

we have a great deal of specific knowledge of the situation and the people involved and of general knowledge of the broader socioeconomic, political and cultural context as well as a thorough grounding in relevant areas of the social sciences. For action research, in particular, we need long-term involvement because with time the elements of a situation change and people's thinking, feeling and behaviour may also change.

Let me give an illustration from the field of architecture and physical planning of the type of demands that are sometimes made on the social scientist and the difficulty of responding to them. Architects feel they know how to build good and beautiful houses, but many have come to realize that things sometimes go wrong once the people for whom they are intended begin to look them over or actually live in them. They may not like them aesthetically; they may not find them suitable to their needs; they may change them and waste some of the money and the effort already spent; they may use the available space and facilities in the wrong way - that is in ways not intended by the architects; they may let them deteriorate; they may even abandon them and refuse to live in them. An architect appeals to a social scientist to help him work out plans for new settlements he has been asked to construct in some developing nation so that the problems mentioned above may be avoided, and to advise him which social scientists would be able to give him all the necessary answers if the future occupants are as yet unknown and if no parallel situation already exists from which to draw some preliminary insights.

I am not here referring to a hypothetical situation. I was personally approached by a world-famous European architect/physical planner to advise him about an important problem for which he felt he needed the social scientist's knowledge. Since there were very few social scientists in the country in question, he wanted me to suggest someone from outside to help him. He had apparently been asked by an oil-rich neighbouring country to build ten thousand urban dwellings and was given only two specifications: they should be "middle class" houses and cars should be able to drive up to every house. He was not told who was expected to live in them and where they would come from. The first advice I thought I ought to give him was that he should insist on additional information as to the type of families likely to inhabit these houses and

then have a quick social study on the type of houses similar families inhabit, on how different members of the family use the internal and external space, what improvements they would like to make on the existing facilities, and so on. The second advice was that he should not build all ten thousand units in one go, but should have a few pilot settlements, then study the characteristics of the people brought to live in them and their reactions to the housing and other facilities, and to adjust his plans for the rest of the settlements in the light of this information. The architect's response was that my advice made perfect sense but that, unfortunately, the government concerned had set a deadline for the completion of the entire project and that gave the physical planners hardly any leeway for the social research activities I was proposing. My response was that under the circumstances no outside social scientist could really help him, and that perhaps the advice of a few native families would be more useful.

Before I wind up, let me briefly mention two additional tendencies that do not augur well for the future of the social sciences. Whereas the world is asking more and more of the social scientist, the academic institutions are training students to do less and less. Training in methodology and in field techniques are totally lacking in many institutions and where there is still a tradition of requiring a thesis based on primary sources, the students are often left to muddle through without any guidance from more experienced researchers. Most students need the extra emphasis on scientific methodology because many of those who go into the social sciences do so precisely because they had never quite grasped their natural sciences or had been unable to handle their mathematics. Furthermore, in the developing countries, as is true of Egypt, the social sciences do not always attract the best students. There is such prestige attached to technical and professional fields, such as engineering and medicine, that many students who have no genuine interest in these fields and who might have turned out to be brilliant in some social science, or literary or artistic pursuit, enter them just because they had received ninety percent or more in the examination for the Secondary Certificate.

Let me conclude by saying how pleased I was to hear that the Social Research Center intends to train new generations of social scientists because

research needs above all trained minds with the capacity to generate ideas. Of course, it needs funds and facilities, but these do not undertake research. People undertake research, but not any people. An academic degree does not automatically qualify a person to be a researcher. It takes people with the intellectual flexibility, the humility and the ego strength to be able to regard their own pet theories and methodologies with a certain amount of skepticism. Research is discovery and the research process must, therefore, remain dynamic. People who plan every detail of a study and never deviate in the process in response to new perceptions and insights are not, in reality, researchers but are robots. In a word, research needs people with curious and creative minds, people with sensitivity and intuition, as well as honesty and scientific rigour.