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## P R E F A C E

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The purposes of the Conference on Social Science Research on Development were to promote an informal exchange of views about priorities in social science research, to develop a better understanding of the financial support presently available for such research, to review the policies governing such financing and to explore the relationships between researchers and policy makers and between researchers in various countries. The Conference was sponsored by the Ford Foundation, the International Development Research Centre, the Rockefeller Foundation, the U.S. Agency for International Development and the World Bank.

The papers in this volume were prepared as background material for the participants in the Conference. Because the papers were an important stimulus to the discussion and because the subjects treated are of broad interest to many people, it was decided to publish them. In addition, a summary of the discussions is included.

The problems treated at the Conference are neither simple nor new, and the objective of the Conference was not to come to any definitive judgements or to reach any operational conclusions but to contribute to a better understanding of the problems in doing research and in disseminating the results of research to decision makers. The distribution of the Conference proceedings should be of service to others engaged in research, research administration or the planning and implementation of development programs.

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Some Problems in the Use and Transfer  
of an Intellectual Technology

by

Paul Streeten  
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## INTRODUCTION AND SUMMARY

In this paper<sup>1/</sup> I examine some of the problems that arise when research in the social sciences on social and economic development is carried out by scholars from rich countries with established and comparatively well endowed centres of learning. Research is itself a social activity, though social scientists tend to neglect the analysis of their own activities.<sup>2/</sup> In this paper the social sciences will be treated as a form of (intellectual) technology.

Technology has been defined as the "skills, knowledge and procedures for making, using and doing useful things".<sup>3/</sup> In spite of this broad definition, which covers knowledge of organization, administration and management, the concept is not entirely appropriate for applied social science. While there are some similarities with commercial technologies, there are also important differences. I shall ask what are the

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1/ I have benefitted from comments and criticisms of an earlier draft by Irma Adelman, Peter Balacs, Ronald Dore, Edgar Edwards, Unni Eradi, Michael Faber, Anne Gordon, Keith Griffin, Seev Hirsch, Jill Robery, Ernest Stern, Frances Stewart, Hugh Streeton, B.R. Virmani, Gordon Winston and Howard Wriggins. To these, and to a research seminar at Queen Elizabeth House, I am very grateful. I am also grateful to the Economic Development Institute of the World Bank and its Director, Mr. Andrew Kamarck, for having provided the facilities and stimulating atmosphere for the early stages of this work.

2/ While it may appear odd of social scientists not to examine the social origins and implications of their own activities, it is quite consistent with what might be called the blind spot of auto-professionalism (or, by Gunnar Myrdal, "The Beam in our Eyes"): psychologists' children are crazy mixed-up kids, and the suicide rate among psychiatrists is the highest; dentists have bad teeth; experts on management cannot manage their affairs nor marriage counsellors their marriages; planners are quite incapable of planning their own lives; the London School of Economics, that power house of social science, is in difficulty about its own structure. So it should not surprise us that social scientists have somewhat neglected the social determination and implications of their own professional activities.

3/ Frances Stewart, "Technology and Employment in LDCs", Paper for Ford Foundation Conference on Technology and Employment in New Delhi, 1973, where she quotes R. S. Merrill "The Study of Technology" in the Encyclopedia of Social Sciences.

scope and the limits of the use and transfer of development research and what social, political, philosophical and moral problems arise when scholars from one set of countries carry out social research on and in substantially poorer foreign countries. The paper is an exercise in the sociology of research and of international relations. As an economist, I have no particular advantage in writing about these issues. I am aware of the philosophical shortcomings of my argument and of the lack of thorough empirical evidence to support some of my suggestions.

In the first section I contrast the linear, stages-of-growth, missing-component, view of development with the view that underdevelopment is partly the result of the system of international relations, whether through malign exploitation or benign neglect, on the part of the developed countries. Research itself can then be regarded either as providing a missing component or as being part of the oppressive or neglectful system. The role of rich country support is radically different according to which view is accepted.

In the second section I examine critically the charges that have been made by developing countries against research on their problems and in their territory by scholars from rich countries. The five main charges are (1) academic imperialism, (2) irrelevance, inappropriateness and bias of concepts, models and theories, (3) research in the service of exploitation, (4) domination through a superior and self-reinforcing research infrastructure and (5) illegitimacy. The additional charge of illegitimacy that is occasionally made is not a serious one.

I conclude that basic knowledge is a common good in use but not in production. Its pursuit unites scholars across national frontiers. Truth cannot be nationalised but there exists bias, distortion and intellectual imperialism in a quite different sense from the one often decried, the correction of which is demanded by true scholarship. Scholarship rejects diplomacy and tact, though sensitivity is essential in social studies and tactics are in order if implementation is desired. The infrastructure of research is subject to increasing returns (both physical and intellectual), so that polarisation, dominance and dependence will tend to be established. There is an infant industry argument for encouraging research in developing countries even, initially, at lower standards. This argument should, however, be clearly distinguished from arguments for parcelling up what is the unity of scholarship. All ideas should be exposed to world-wide scrutiny and criticism.

In the third section I distinguish between different arguments for collaboration in research between rich and poor countries and try to separate different reasons for such collaboration. These motives range from using the local institute in a subservient capacity to participatory theory construction. Collaboration between rich and poor country scholars, like joint business ventures, may be merely a facade for domination but this can be avoided by first building up research capacity and then entering into genuine joint ventures. But the choice

between capacity and quality raises difficult problems of objectives, time discount rates and risk. As there are grounds for collaboration, there are also grounds for specialisation. But the economically appealing rule that countries should specialise according to their relative intellectual and physical factor endowments cannot be applied to research.

The fourth section deals with interdisciplinary research. (Readers in a hurry may skip this section.) There are three distinct reasons for interdisciplinary work. First, specialists in different disciplines may work together on a specific practical planning problem. Second, assumptions, concepts or methods evolved in one discipline may yield fruitful results when applied to the problems previously treated by another. Third, the concepts, models, paradigms may have to be recast so as to encompass variables previously separated in distinct disciplines because the social reality of a different culture demands this. This is illustrated with the concept "capital". There is a conservative and a radical version. In the former, new wine can be poured into old bottles; in the latter, wholly new concepts and models must be constructed.

The fifth section illustrates some of the issues with the aid of an example: research on poverty eradication and strategies for greater equality. Readers only interested in the research issues may skip section three, which concludes that strategies must constitute a tripod, standing on the three legs of price policies, institutional reform and technological innovation.

The sixth section discusses the question whether research in rich countries should confine itself to the interface of rich-poor relations or whether development research is an indivisible whole. The arguments for confining it to the interface are (1) that this is an area in which rich countries can act; (2) that this escapes the charge of academic imperialism; (3) that it avoids the paradox of rich country institutions propagating indigenous capacity building and (4) that it avoids the impropriety and counter-productivity of advocating radical solutions for others. Objections to confinement to the interface are (1) since not everyone can be prevented from doing research on the domestic issues of the poor countries, balance demands that anyone should be free to correct a possible imbalance; (2) attention by foreigners to domestic issues of poor countries may be a correction to internal brain drain and encourage domestic work on relevant issues; (3) international and internal variables interact and a division is methodologically impossible; (4) free research should not be limited by national boundaries. In spite of these objections, interface issues are in the present climate particularly suited for rich country research. The section also discusses, as part of the interface, research on questions of international cooperation, confrontation (conflict) and LDC self-reliance.

The seventh section deals with problems arising from the origin and organization of research funds, the questions whether and when money is tainted, whether there should be concentration or dispersal of sources of funds, and how to bridge the gap between the requirements of policy makers and the freedom of academic research. It warns against sacrificing the important to the urgent.

The eighth section contains a brief warning against over-simple quantification in an attempt to emulate the "hard" sciences. It ends on the sceptical note that we still do not know what are the springs of development, and a warning that we should not sacrifice the important to the manageable.

An appendix reproduces a review of a book on Project Camelot, a research project that sparked off some of the fire of the debate and that contributed to the difficulties scholars nowadays face in their work in developing countries.

#### I. WHENCE DO WE COME? WHERE ARE WE? WHERE DO WE GO?

Our perception of development has undergone a radical change in recent years. The thinking of the Fifties and Sixties, codified in the Pearson Report, was dominated by W.W. Rostow's doctrine of the stages of growth. According to this doctrine, development is a linear path along which all countries travel. The advanced countries had, at various times, passed the stage of "take-off", and the developing countries are now following them. Development "was seen primarily as a matter of 'economic growth', and secondarily as a problem of securing social changes necessarily associated with economic growth. It was taken for granted that organizing the march along the development path was the prime concern of governments....."<sup>1/</sup>

The linear view begged a host of questions about the nature, causes and objectives of development. It tended to focus on constraints or obstacles, the removal of which would set free the "natural" forces making for the steady move towards ever higher incomes. This view is reflected, for instance, in the "General Principles Governing the Award of O.D.A. Grants for Economic and Social Research in Developing Countries". The first sentence states: "The fundamental criterion is that the research proposed must be related to the problems that impede

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<sup>1/</sup> Colin Leys "The Role of the University in an Underdeveloped Country". Education News, April 1971, Department of Education and Science, Canberra. For a critique of the linear theory, see chapter 2 of Development in a Divided World, edited by Dudley Seers and Leonard Joy (Penguin 1971).

the social or economic progress of developing countries" (underlining mine). According to this view, obstacles can be removed by supplying various missing components -- capital, foreign exchange, skills, management. The doctrine provides a rationale for international capital aid, technical assistance, trade, private foreign investment. By breaking bottlenecks, rich countries can speed up the development process in underdeveloped countries.

This linear or stages-of-growth view has come under heavy fire. It was criticised on logical, moral, political and economic grounds. Logically, it should have been clear that the coexistence of more and less advanced countries is bound to make a difference (for better or worse) to the development efforts and prospects of the less advanced compared with a situation where no other country was ahead or the distance was not very large. The larger the gap and the more integrated the international system, the less relevant are the lessons to be learned from the early starters. Morally and politically, the linear view ruled out options of different styles of development. Inexorably, we were all bound to pass through the Rostovian stages, like a tram, not a bus.<sup>1/</sup>

There is another view which has gained adherents with the spreading disenchantment about development and about the international contribution to it. According to this view, the international system of rich-poor relationships produces and maintains the underdevelopment of the poor countries. In various ways, malignly exploitative or unintentionally neglectful, the coexistence of rich and poor societies renders the efforts of the poor societies to choose their style of development more difficult or impossible. Certain groups in the developing countries-- entrepreneurs, salaried officials, employees -- enjoy high incomes, wealth and status and, constituting the ruling class, they perpetuate the international system of inequality and conformity. Not only Marxists but also a growing number of non-Marxists have come to attribute a large part of under-development to the existence and the policies of the industrial countries of the West, including Japan and the Soviet Union.

This shift from a linear theory of missing components to some version of a theory of neo-colonialism was accompanied by a change in emphasis of what constitutes the meaning and measure of development. Economic growth by itself was found to be largely irrelevant and the eradication of poverty, reduced inequality, larger, securer and more diversified jobs and livelihoods, took the place of growth.

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<sup>1/</sup>There once was a man who said, "Damn!  
It is borne in upon me I am  
An engine that moves  
In predestinate grooves  
I'm not even a bus, I'm a tram."

Maurice Evan Hare.

This shift affected the view of the role of research. On the linear, missing components view, research in rich countries or by rich country scholars can contribute bits of knowledge and thereby remove a particular constraint. On the neo-Maxian view, research may itself be part and parcel of the international oppressive or at least impeding system, depriving the developing countries of brains, or diverting the attention of their brains to irrelevant problems or inducing them to produce apologies for their ruling class and the unjustifiable world system. The new view has been reflected in growing tensions and difficulties encountered in research relationships and resistance to the admission of social science researchers to developing countries.

In several places in this paper, and particularly in the next section, I shall try to analyse how this shift of perception affects the role of the research donor. What should be noted here is that, irrespective of the scientific status of the new view, the mere fact that influential people in developing countries hold it is bound to make a difference to the research relationship.

But in addition to this fundamental shift, there have been shifts in fads and fashions. Just as fashion setters emphasize, display and conceal at different times different parts of the female anatomy (though presumably all are there all the time), so economic and social research tends to be preoccupied with different aspects of the variables in the social system, to the neglect of others. Certain subjects or views, at any given time, have "sex appeal". The explosive interest in equality at the expense of economic growth can be regarded as such a fashion. Other cycles are the emphasis on industrial import substitution, followed by recommendations of industrial export promotion and, now, the beginnings of some disenchantment with industrial export-led growth and a new turn to primary export restrictions. Another fashion cycle is the switch from investment in physical capital to investment in formal education, followed by disillusion in formal education and a turn to informal education and motivation; also the swings between functional literacy and mass literacy campaigns. Another cycle is that between pessimism and euphoria about world food production. The debates on agriculture versus industry, large-scale versus small-scale techniques, formal versus "informal" sector, deteriorating versus improving terms of trade, material versus social objectives, growth versus the environment, and others have found in turn a clustering of views round alternating sides of the pendulum. The importance and the irrelevance of development aid, as viewed by both donors and recipients, represents another swing. One could go on.

To the extent that these swings of the pendulum are indications of important underlying forces, research donors should clearly be concerned with them. But often, they by-pass the important issues and, looking back even only a few years, or even months, one is astonished at the

problems that vexed the profession, for which the wisdom of hindsight it is now clear that the really important issues lay elsewhere. It would be nice to be able to predict where the next break-through in research is going to be, so that we can prepare ourselves for it. Yet, such a forecast is logically impossible. If I, or anyone else, knew where the next break-through was going to be, we should already have performed it and it would be the last break-through.

One insurance against becoming a mere follower of fashion is to continue to support research on unfashionable topics. Who knows, they might hit the headlines tomorrow. Energy has ousted the conventional ways of looking at security, money and trade; concern with the environment has driven out old-fashioned growth; and the politics of bargaining are upsetting the assumption of atomistically competing units. A project on "Man (or better, Woman) energy, the environment and equality" sounds at the moment of writing as irresistible as the well-known best seller entitled "I made love to a goat for the CIA and saw God". Yet, the burning issues of 1980 may be quite different ones.

## II. THE ROLE OF THE SOCIAL SCIENCES AND THE SOCIAL SCIENTIST IN DEVELOPMENT STUDIES

Development research in the developed countries has been criticised on several grounds.

(1) First, there is the charge of academic, scientific, intellectual or cultural imperialism or colonialism.<sup>1/</sup> The critics see a close parallel between the operations of the more ruthless mining companies and the developed country research teams. They move into the country with their already designed research projects, trying to "mine" for data and statistics, using locals for semi-skilled activities like interviewing, filling out forms, interpreting, but preserve for themselves the monopoly-earning activities of basic research design, processing and publishing. The "researched" country, having been stripped of its data, suffers the humiliation of seeing the results published in the journals or books of the advanced industrial countries, adding prestige and glory to the foreign professors and their institutions. Sometimes, as in the case of certain multinational firms and the CIA, mining is combined with undermining; the research is used to interfere with the democratic processes of the country and to further the aims of foreign powers.<sup>2/</sup>

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<sup>1/</sup>This charge can be interpreted broadly, when it covers also the subsequent two charges, or narrowly, when it is confined to "mining".

<sup>2/</sup>See Appendix.

(2) Second, there is the charge that "Western" concepts, models, paradigms and the questions asked - both the Agenda for research and the filing cabinets - are inappropriate for understanding the utterly different circumstances of developing societies. Here again, an analogy is drawn between the inappropriate industrial and agricultural techniques and the concepts, models and methods of economics and other social sciences. These alien concepts and models determine inappropriate policies and either divert attention from the real problems or become apologies for existing power structures. Excessive sophistication, esoteric irrelevance, ignorance and false beliefs conveyed by these doctrines are opportunistic and serve vested interests. Heavy emphasis on capital/output ratios, savings and investment ratios, the notion of unemployment and employment, aggregate income, and others have, it is argued, misled policy makers (or strengthened them in their class interests) and have concealed the importance of institutional and other structural changes, such as land reform, corporate reform, tax reform, credit and banking reform, the creation or strengthening of an independent, honest and efficient administrative service, or an egalitarian educational system. The paradigms of "Western" social science serve as blinkers or escape mechanisms, preventing scholars and policy makers from seeing and acting upon the strategic fronts.

(3) A stronger version of the charge of opportunistic irrelevance is the view that advanced country research is part of the system of international capitalism, in which underdevelopment makes possible the growth of the capitalist countries of the West. Private foreign investment, the multinational enterprise, international trade, international monetary arrangements, universities, and, indeed research itself, all reinforce the dominant position of the advanced, industrial countries, together with a small class of privileged people in the underdeveloped countries and serve the exploitation and the continuing underdevelopment of the majority of people in the poor countries. The framework of research is essentially an apology and justification for the neo-colonial apparatus of exploitation.

(4) Fourth, there is the charge of domination and dependence in a rather different sense. The complaint here is not that the developing countries are exploited (as in the charge of intellectual imperialism or neo-colonialism), nor that the concepts and models are inappropriate or ideological. The trouble is simply that, as a result of the location of funds and the accumulation of skills, the scholars from the developed countries have gained a superiority and that this superiority, combined with the institutions and attitudes derived from it and reinforcing it, prevent research institutions and attitudes in the developing countries from growing to strong and independent status. Both the incentive and the capacity to generate new ideas and to carry out indigenous research on relevant problems are weakened by the operations of the foreign scholars and institutes. The relationship between the foreign pro-

fessor and the local workers is often that of patron and protégé. The patron will try to get jobs, write references, arrange a fellowship to the metropolitan country for his protégé. But the relation is one of dependence of the developing country researcher on the favours of the foreign patron. Only more inward-looking policies towards research (it is argued), more cooperation with countries at similar levels of development and the pulling down of a curtain against the stunting influence from outside, hold out hope for the growth of realistic and relevant research, based on self-reliance, self-confidence and autonomy. It is a kind of intellectual infant-industry argument.

(5) Fifth, there is the charge of illegitimacy. If this amounted merely to saying that scholarship should be confined to the territory within a scholar's national boundaries, it could be dismissed without further discussion. But a question of moral (though not intellectual) legitimacy is raised when research leads to the recommendation of actions, the cost of which are borne entirely by other people. There is something, if not illegitimate, at any rate distasteful in people from safe and comfortable positions recommending revolutions or painful reforms, or, for that matter, the maintenance of the status quo, to others.<sup>1/</sup> This is, of course, a general point, not confined to research on poor countries. It raises the much discussed question of the moral responsibilities of the social scientist. But it arises in particularly acute form if the subject of investigation are countries on the government of which we have no influence.

To what extent are these charges justified? The analogy between mining or quarrying and searching for knowledge is surely false. The more nickel, copper or gold I have, the less is left over for you. This is not so with knowledge. We all can draw on the stock of knowledge and my discovery does not normally deprive you of intellectual profits from it, though it may deprive you of recognition for the discovery.

More importantly, there are common standards of scholarship, which assert its universality and the solidarity of any one scholar with the international fraternity of other scholars. Commitment to the search for knowledge, to scientific objectivity and to telling the truth as one sees it, know no national frontiers. In addition to the intrinsic

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<sup>1/</sup> Ronald Dore remembers "being at a conference in Singapore when Wertheim gave a paper on the obstacles to development, which he identified as landlords and capitalists, ending with the stirring prediction that the masses were already awakening and that the duty of the scholar was to help them sweep away their enemies. An Indonesian there was somewhat indignant. 'Fine for you, Professor Wertheim. But I'm the sausage that's going to be fried in the fire-- though I'm neither landlord nor capitalist.'" Ronald Dore goes on to say (in private communication) how the same point arises when we preach income redistribution in even a mild form to our colleagues from poor countries, whose reference group is the international academic community rather than peasant incomes in their own countries.

value of this commitment, loyalties to universal values that cut across frontiers have their political value in an age when nationalism, a powerful Christian heresy, and ideologies have become dominant secular religions. In this sense, therefore, there cannot be African, Asian and Latin American criteria for truth or validity. Mining companies can be nationalised; criteria for truth cannot. <sup>1/</sup>

The problem is, however, complicated and sometimes confused by people mistaking economics for a form of logic, truth for validity and criteria of truth for its empirical content. For if economics is equated with a form of logic ("the logic of choice"), it would follow that there is only a single, universal economic science from China to Peru and no separate economics for Africa, Asia or Latin America. <sup>2/</sup> Yet clearly, if we turn from the standards and criteria by which we judge evidence, methods and conclusions to the content of our work, it should be plain that very different propositions are likely to be true for different societies. In this sense, it is perfectly legitimate to speak of "African", "Asian" or "Latin American" economics or politics or sociology. It is this confusion between validity and truth, to which some economists have themselves contributed, which is partly responsible for what appear to be nationalistic attacks on the "legitimacy" of "Western" social science.

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<sup>1/</sup> Michael Faber reminded me of John Austin's "In vino, possibly, 'veritas'", but in a sober symposium "verum". "What needs discussing rather is the use, or certain uses, of the word 'true'." J. L. Austin, Philosophical Papers edited by J. O. Urmson and G. J. Warnock (Oxford, at the Clarendon Press, 1961), p. 85. But for the sense in which I use it, Pilate's question holds no terror; "true" will do for my purposes.

<sup>2/</sup> This is the view of, e.g., Lionel Robbins. "It has sometimes been asserted that the generalisations of Economics are essentially "historicorelative" in character, that their validity is limited to certain historical conditions, and that outside these they have no relevance to the analysis of social phenomena. This view is a dangerous misapprehension." An Essay on the Nature and Significance of Economic Science (Macmillan, first edition, 1932, second edition, 1946), pp. 80-81. See also Ludwig von Mises, Human Action, (Hodgy, 1949). Sir Geoffrey Wilson tells me that about twenty years ago, when he was in Ceylon starting up the Colombo Plan, he sent to Sydney Caine, who was then Vice-Chancellor of the University of Singapore, a memo suggesting that the economics of the countries of South East Asia might better be studied locally than at the London School of Economics and that an institute might be set up for the purpose in South India where, incidentally, foreign scholars might find a home and see what it was like to work in local conditions. Caine's reply "led me to think that I had blasphemed in church--economics was economics, whether in London, Delhi, Tokyo or the moon, etc., and I retired covered in confusion at ever having had such an unclean thought."

It is part of scholarship to recognize the limitations of propositions established and possibly applicable to one region (or period) but not, or not without modification, to another. It is understandable that territorial and temporal claims of the validity of certain theories have tended to be excessive and that there is, for this reason, an element of "imperialism" in the generalisations of social science. But this sense of "imperialism" is, of course, entirely different from the one discussed above. Thus, it can be argued that important elements of "scientific socialism" are an extrapolation and universalisation of the experience of industrial England between 1780 and 1840, when inequalities increased and when, in the midst of fairly rapid industrial progress, the poor may, for a time, have become poorer. Ricardo and Malthus universalised the temporary pressures on land of the rapidly growing population of England, while technical progress lagged behind. The so-called General Theory of Employment, Interest and Money is a rather special theory, applicable to the grossly underutilised resources in industrial countries during the depression of the 1930s. Neo-classical economics, with its nicely calculated little more and little less, its assumptions of maximising behaviour and atomistic competition, may be regarded as a generalisation of certain principles of petty-bourgeois housekeeping. And so one could go on. The Ricardian theory of distribution, the Malthusian theory of population, the Marxist theory of the increasing misery of the masses, various theories of secular stagnation, secular inflation, secular shortages of dollars, food or raw materials, or secular doom, may all be projections onto a vast historical screen of the snapshots of a few years or decades and the magnified protests and responses to which these short-run experiences give rise. The designers of these theories suffer from a high elasticity of expectations.

If these doctrines had made more limited territorial and chronological claims, if they had confined themselves to their time and their place, nobody would have paid much attention. They derive their interest and their significance from the grand design, the magnificent extrapolation, from magnifying the trivial into the false. But it is quite legitimate (in the service of universal truth) to criticise orthodox Western models for their excessive claims, for their "intellectual imperialism". It is in the interest of honest work to assert that in Africa, Asia, Latin America, at very low levels of development, in a different demographic setting, in tropical climates, in a different international system, etc., they order things differently. Yet, such limitation of excess claims, if it is legitimate, is such that it must be recognised as legitimate by scholars wherever they may be. There have been sociologists (like Karl Mannheim), anthropologists (like Levy-Bruhl), linguists (like Benjamin Lee Whorf), and philosophers of science (like Thomas Kuhn) who have argued that the criteria of truth and validity themselves vary from time to time and place to place, according to their context. But I believe it can be shown that at least some criteria of truth and validity cannot be dependent on social, cultural, linguistic or other existential factors, indeed that even asking questions about differences between

beliefs and theories presupposes logically universal and fundamental criteria of truth.<sup>1/</sup>

Those who claim that bias enters into social paradigms and theories and that "Western" social science is an apology of exploitation or a diversion maneuver are not always clear about the precise manner and form of entry. There appear to be at least four possibilities:<sup>2/</sup>

1. Bias determines the content, and thus the validity, of the analysis psychologically.
  - (a) The analysis is consciously false; the propositions are lies.
  - (b) The distortion is semi-conscious; there is wishful thinking, special pleading.
  - (c) The distortion is subconscious or unconscious; the conclusions are rationalisations.

It may, of course, be that one group of men implant what they know to be false notions into the minds of others by manipulative efforts. If the victims are not aware of being manipulated, their beliefs would fall under (1c), whilst the activity of the manipulators (propaganda, conditioning) falls under (1a), at least as long as they have not fallen victims to their own devices.

Freudian rationalisation is a method of resolving conflicts peculiar to the individual, whereas rationalisation here considered serves to resolve social conflicts. Ideology is for society what the resolution of guilt through self-justification is for an individual. But tensions in the structure of society will tend to manifest themselves in the psychological problems of individuals and the two spheres are not strictly separable.

In all three cases, (a), (b) and (c), only false statements are contaminated. Ideology is defined as "false consciousness". Bias may provide a motive for finding a logical (as well as an illogical) basis for a desired conclusion, but the analysis is not then distorted. There is a sphere of objective thought.

The Archimedean point (i.e. the point from which the doctrine of biased determination is itself lifted into objectivity) is given by exposing the motives. (There is, however, a danger that this effort itself is contaminated by unexpressed bias.)

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<sup>1/</sup> See Steven Lukes "On the Social Determination of Truth" in Modes of Thought: Essays on Thinking in Western and Non-Western Societies, edited by Robin Horton and Ruth Finnegan (Faber and Faber, London, 1973).

<sup>2/</sup> The classification follows closely my Introduction to Gunnar Myrdal, Value in Social Theory, edited by Paul Streeten (Routledge & Kegan Paul, London, 1958), pp. xxxvi-xli.

2. Bias determines the content and thus the validity of analysis by affecting the structure of thought (categories, presuppositions, premises, paradigms). Contamination is not a matter of individual or even social psychology, but everyone in a given situation who thinks at all has to think in a certain biased way. Probing of motives cannot eliminate implicit bias, for it is an essential condition of all thought.

Nowadays, a similar point is made by stressing the manner in which language influences the way we see, select, and analyse events, and thus opens the door to bias. First, it enters not only -- as is generally recognised -- into the selection and criticism of evidence, but also into our classifications and frames of reference. Particularly in social studies do we take our vocabulary from the field of study itself. Thus the valuations of the market place and of the political arena are carried unobtrusively into scientific analysis.

Secondly, language introduces a bias by adopting identical terms for situations that are similar in some respects, dissimilar in others. To use the same concept, or model, or metaphor to refer to different situations is a source of both danger and opportunity. Danger, because the reference may distort or misrepresent the facts; opportunity, because it may enlarge our vision by drawing attention to hitherto unnoticed features.

Thirdly, there is the danger of seeing real essences behind terms of mere classification.<sup>1/</sup>

In whatever manner we analyse the seepage of bias into analysis, it follows that not only false, but all statements under this second heading are "ideological" and hence logically suspect, unless areas are cleared which are claimed to be exempt from bias. Thus some writers say that

- (a) only at certain historical periods (e.g. in a class society), others that
- (b) only certain fields of study ( e.g. social studies), others that
- (c) only certain classes (e.g. the bourgeoisie) are subject to contamination, have "false consciousness".<sup>2/</sup> But the corollary that only certain periods, fields of study, classes or men are free from

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<sup>1/</sup>In so far as only the emotive use of words is stressed, the linguistic analysis should be classified under 1. Bias enters at the psychological level and could be eliminated by a purged language.

<sup>2/</sup>The expression occurs in one of Engels' letters to Mehring. Cf. Franz Mehring, Geschichte der deutschen Sozialdemokratie (1921) Vol. i, p. 386.

bias, can, of course, itself be a fruitful source of ideology. Combining (a), (b) and (c), we arrive at the Marxist view that there is a proletarian social science in the late stages of capitalism to which truth is guaranteed. Particularly Georg Lukács has argued that only proletarian class-conscious thought represents reality "adequately". Mannheim (in his earlier work) believed that only the "socially unattached" (i.e. radical) intellectuals can seek the required "dynamic synthesis", a "total perspective", that overcomes the inadequate, partial and biased conceptions of other groups. Hegel thought that reason revealed itself to philosophers (particularly Hegelian philosophers) at a certain stage of history. Nearer home, Marshall, Pigou and others in the tradition of Benthamism imply that in the midst of interest clashes only the State is an agency that can see and promote disinterestedly the public good.

On the other hand, some authors argue that it is not only valuations and bias, but other extraneous spheres that determine thought, e.g. social or economic conditions (as contrasted with interests, aspirations, valuations), natural environment, nationality, race, generation, geographical region. This determination may be conceived as either casual, or as an expression of some kind of unity, like that which links the characteristics of a personality.

The Archimedean point cannot be reached empirically, nor logically, but only metaphysically. The "intellectuals", "the working class", the "Third World", the "Wretched of the Earth", or "action", "commitment", "a synthesis" or "an absolute sphere of values", guarantee objectivity. Or, using the linguistic approach, only a "perfect language" that exactly fits the facts, could enable us to pull ourselves up by our own bootstraps. The attempt to save the theory from self-contradiction succeeds only through an arbitrary step into metaphysics. The choice lies between dogmatism and absurdity.

3. Bias had merely selective significance. It does not affect the content or validity of thought, but its direction.

- (a) It may be positively selective: bias determines that a proposition is made then and there. The questions asked are value-determined, but not the answers. The relation between bias and theories is not causal, but like the kind of determination by which a question "determines" an answer.
- (b) It may be negatively selective, preventing certain propositions from being made in certain situations. Thus questions relevant to developing countries may never be asked by social scientists from rich countries.

An Archimedean point is not here required, for validity is independent of valuations. The distinction, however, between type 3 and type 2 ideology is blurred when we remember that an inadequate, partial conception of reality may lead to bias not by commission but by omission. The Archimedean point would consist in scaling down the claims of the theory to less generality; but it then often loses all interest.

4. Valuations determine whether certain propositions are understood, recognised, publicly accepted. Again, no Archimedean point is required.

According to which of these views 1. - 4. is held, the role of criticism is (1) to show up and make explicit the more or less sinister motives in the false explanations of the opponents, or to psychoanalyse their theories, (2) to analyse the structure of their thought, (3) to fill in gaps in the selection, or (4) to relate ideas to their social setting. It is also obvious that these four views have radically different implications for the question as to what extent unexpressed value premises invalidate social theories. Yet, eminent exponents of these theories of ideology have shifted uneasily between self-destructive and fairly obvious positions. To say (a) that the tests of logic change with one's values is open to the old objection to scepticism: if the theory is untrue, no more is to be said; if true, its own objectivity must be denied. On the other hand, to say (b) that we meet with obstacles in our attempts to be impartial in thinking about social and political matters, especially in different settings, serves as a useful reminder that social scientists, too, are human. But the ambiguity between these two views (a) and (b) lends apparent force to many theories that reject all "Western" paradigms.

It is likely that the limitations shown up by those combating the excess claims and correcting the distortions of biased ideologies will ultimately benefit work in and on the developed countries themselves.<sup>1/</sup> There is mutual illumination which a shutting off and "going it alone" would impede. This is a bonus, but even if it were not so, theories with excess claims are not "true for Europe but not true for Africa": they are simply not true.

I conclude that, although the laws of logic and the criteria for truth must be universal, the concepts, models, premises, assumptions, paradigms, theories or questions in the social sciences are in some respects peculiar. There may be an African economics, distinct from a European economics; there can be no African truth.<sup>2/</sup> By rigorous analysis, by accumulation of evidence and by bringing out explicitly value premises, errors and biases can be reduced. But there will always remain a residual of ideology. And this residual element may

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<sup>1/</sup> See Page 27.

<sup>2/</sup> Spelt out less aphoristically, this means: there exists a set of propositions about social and economic phenomena and their relations which is generally true of Africa (at least for the time being), but not generally true elsewhere; but there can be no set of exclusively African criteria for what is true.

be particularly misleading if transferred from the experience of industrial countries to developing countries. It is for this reason that the assertion of the universality of the criteria for truth must be qualified, although the remedy cannot be found in "indigenous" theory construction or in erecting a barrier against "alien" doctrines.

One important element of truth in the charge of "Western" intellectual domination is the ideological component of "Western" theories just discussed. There is a second element of truth in these charges.

While the stock of fundamental knowledge is a public good, to be drawn on by anyone anywhere, the resources that enable scholars to conduct research are, of course, scarce and the recognition, prestige and fame that are the reward of successful work are competitive and by no means a free good. While a scientific discovery can be used by anyone, only one man can make it. And he reaps the monopoly rent of recognition.<sup>1/</sup> Moreover, recognition attracts funds and funds make it possible to gain recognition: a cumulative process that will tend to penalise ill-endowed centres of learning. No money: no ability to train and attract good scholars: No recognition: no money. It is then understandable if institutions starved of funds in the developing countries should resent the well-endowed foreigners who use their work in the developing country to gain further recognition and hence even more funds. If this invasion is then accompanied by concepts, models and paradigms that are irrelevant, unrealistic or ideologically biased, if these concepts, models and paradigms make excess claims, if the behavior of the foreigners is tactless, insensitive or patronising, it is understandable that the indigenous scholars will charge the foreigners and their doctrines with being dominant and arrogant and that they will not wish to engage in joint ventures.

There is also a feeling that opportunities to present, exchange, develop and communicate ideas and to implement them are much greater in the developed countries. Doers and thinkers meet at beautiful country houses like the Villa Serbelloni in Bellagio or in Ditchley Park

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<sup>1/</sup> "The comparison that Lagrange made of Newton is worth repeating in this connection: assuredly Newton was the greatest man of science, but also the luckiest. For there is but one system of the world and Newton was the one who found it. Similarly, there is but one grand concept for general equilibrium and it was Walras who had the insight (and luck) to find it." P. A. Samuelson, "Economists and the History of Ideas" American Economic Review, vol. LII, Number 1, March 1962, pp. 3-4.

or in Burgenstock or in Königswinter or at Wilton Park, and from these meetings clusters of ideas and policies emerge.<sup>1/</sup> In rich countries, there is a continuing generation and exchange of ideas between policy makers and scholars from which paradigms and programmes are crystallised. Nothing comparable exists in the Third World. Different paradigms, incorporating variables that are omitted, especially political and social factors, would create a different picture of reality and different programmes for action. It is felt that existing opportunities for such a crystallisation are inadequate or absent and that, in particular, the United Nations bodies have failed to generate the ideas on which policy makers could draw.

Finally, there is clearly justification in the fifth charge, (viz. moral illegitimacy) though the term "illegitimacy" is unfortunate. It is all too easy to be a radical, a revolutionary, a reformer, or, for that matter, a conservative, for another country.<sup>2/</sup> The moral commitment to objective research may conflict with the immorality of non-commitment to action.

Having said all this, it must be reasserted that a universal commitment to scientific research and to presenting its conclusions as one sees them are fundamental to all scholarship. Such a commitment ignores national boundaries and unites scholars wherever they may be. It follows from this commitment that the scholar, in presenting the results of his work, will not be concerned with tact, tactics or diplomacy. He will not keep silent merely to spare feelings and he will not mince words. It would be a form of inverted snobbery and condescension if scholars from developed countries thought it necessary to treat "sensitive" problems of the developing countries with kid gloves, although this is what has largely happened, even in the very terminology of this sentence. Such diplomacy and inverted snobbery has reduced the standard of work in this field. Tact and diplomacy are, of course, necessary if recommendations are to be adopted by governments. But this must not affect the content of basic research, partly because it offends against the principles of scholarship and partly because policies based on blinkered analysis are bound to fail.

Such frankness and even bluntness is entirely consistent with, indeed is demanded by, empathy with, and imaginative understanding of the problems analysed, though not necessarily sympathy for all that is done. Some outside criticisms have, it is true, failed in their sensitivity to the social complexity. But equally, some sycophantic or "diplomatic" work is at bottom patronising and hence equally insensitive.

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<sup>1/</sup> Michael Faber tells me that "when Danilo Dolci held a conference of notables on his problems of development, he chose Palma di Montechiaro (the true home of the fictional Gattopardo), where the heat was intolerable, the village hall was filled with the stench from the open gutters outside, the din from the square made the speakers inaudible, and the fragile electricity supply could be counted upon to break down."

<sup>2/</sup> See below, p. 38.

Are Western scholars more liable to impose biased or inappropriate concepts and models than scholars from the developing countries? Many of the "alternative" models used by Third World intellectuals have, of course, Western origins. Marxism is a Western doctrine. So are Intermediate Technology, Structuralism, Planning, Cumulative Causation and Growth Poles.

Myrdal, Singer, Perroux, Ivan Illich and Fritz Schumacher are all "Western" thinkers and Prebisch is only marginally an "underdeveloped" scholar. Padma Desai quotes as illustrations of "innovative economic contributions based on indigenous conditions and talent" the early Soviet economists, Evgenii Preobrazhensky and G.A. Fe'ldman; the Chinese shift from the Marxist emphasis on the revolutionary potential of the industrial proletariat to that of the rural masses;<sup>1/</sup> and the Indian efforts at evolving an intermediate technology such as the Ambar Charkha and the Chinese campaign to produce steel from backyard blast furnaces, though both attempts were unsuccessful.<sup>2/</sup> These are not altogether good examples of indigenous innovations of ideas and, in any case, the social science research content of the Chinese and Indian innovations is small, whereas the Russians are surely for this purpose "Western". The new patterns have grown more out of praxis and experience than out of systematic research. Indeed, those who take a Marxian rather than a Keynesian view of the relation between the power of ideas and praxis, see evidence that solutions to social problems are worked out by men and women going about their daily work, by politicians, party officials, administrators, extension workers, and that the grand theories distill these practical experiences.<sup>3/</sup> It would be arrogant, as well as wrong, to believe that only research is the source of new knowledge.

Paradoxically, the doctrine of the limits of transference may itself be regarded as a typically Western product and therefore as

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<sup>1/</sup> But Bakunin, a thinker with intuition in many respects superior to Marx, had seen a revolutionary potential of the peasants and predicted revolutions not in the most industrialised countries, but in underdeveloped rural societies like Spain, Russia and China.

<sup>2/</sup> Padma Desai, "Third World Social Scientists in Santiago", pp. 63-64, World Development, vol. 1, No. 9, September 1973.

<sup>3/</sup> A synthesis of the Marxian and Keynesian view is presented by Max Weber: "Interests (material and ideal), not ideas, dominate directly the actions of men. Yet the 'images of the world' created by these ideas have often served as switches determining the track on which the dynamism of interests kept the action going." Max Weber by Marianne Weber. (Tübingen, 1926.)

non-transferable.<sup>1/</sup> This line leads us to the dilemma of the Cretan liar. More sinister, there is a short step (it might be argued) from the doctrine of the need to evolve alternative styles of thinking to the doctrine of "separate and equal" and from there to apartheid. It is quite easy to give the call for alternative systems of thought a nasty racist ring. The doctrine of non-transferability may be interpreted as an unpleasant form of Western neo-colonialism. But, at least logically, this trap holds no danger. If non-transference must not be transferred because it is Western, transference is O.K. Rejection leads to acceptance.

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<sup>1/</sup> In an interesting report on a seminar in Poona, Andrew Shonfield reported that he "was frankly surprised at the violence of the antipathy shown by the Westerners towards the Western way of life" compared with the enthusiasm for large-scale facilities to produce cement, electricity and steel of many "Easterners". Fritz Schumacher "emphasized the need to protect, above all else, the process of 'organic growth'. Planning of development on this view would be as much concerned to avoid the introduction of new manufactured products ... which would put the established (producers) in the villages out of business." "Whereas Schumacher foresees spiritual disaster in any attempt to speed up development through a programme of large-scale industrialisation (Colin) Clark asserts flatly that it cannot be hurried up at all." "Alternatives to Backwardness", Encounter, No. 99, December 1961.

Similarly, the "decision to introduce English education was taken not without an acute controversy. The story of this controversy, known as the controversy between the Orientalists and the Anglicists, is quite familiar to all students of the history of Indian education. The curious fact is that the Orientalists were almost all Englishmen in the service of the Company, whereas almost all Indians of repute were Anglicists. ...It was the strong position taken up by Macaulay that forced the issue in favour of the Anglicists." G. Ramanathan, Educational Planning and National Integration (Asian Publishing House, 1963), p. 21. And Macaulay wrote in his famous Minute on Education: "I am quite ready to take the oriental learning at the valuation of the orientalist themselves. I have never found one among them who could deny that a single shelf of a good European library was worth the whole native literature of India and Arabia." From Macaulay's Minute on Education, in Sources of Indian Tradition, by Theodore de Bary, Stephen Hay, R. Weiler and Andrew Yarrow (Columbia University Press, 1958, published in Britain by O.U.P., London).

In spite of some Western critics of Western modernisation (since joined by Ivan Illich), Max Weber identifies the claim to a single universal future as an important characteristic of Western civilisation. "A product of modern European civilisation, studying any problem of universal history, is bound to ask himself to what combination of circumstances the fact should be attributed that in Western civilisation, and in Western civilisation only, cultural phenomena have appeared which (as we like to think) lie in a line of development having universal significance and value." Max Weber, The Protestant Ethic and the Spirit of Capitalism.

I conclude that knowledge itself is a common good and that its pursuit unites scholars across the world<sup>1/</sup>; that scholarship rejects diplomacy and tact, though sensitivity and imaginative understanding are essential in social studies and tactics are in order if implementation is desired; but that the concepts, models and theories are often partial, inadequate, irrelevant, or biased in a manner which ignores or distorts the relevant problems; and that the infrastructure of research: seminars, conferences, country houses, institutes, universities, travel, grants, sabbaticals, etc., is subject to increasing returns, so that that polarisation, dominance and dependence will tend to be established. There is an intellectual and economic infant industry argument for encouraging research in the weak periphery, even if it is initially of a lower standard than research carried out in established centres, but there is no argument against exposing ideas to world-wide scrutiny and criticism.

### III. EQUAL OR UNEQUAL PARTNERS IN RESEARCH

Clearly, the commitment to universal standards of scholarship in no way reduces the need to encourage the growth and self-confidence of indigenous research institutes. It is now a cliché to say that research in developing countries must be sensitive and must be collaborative. When in Rome, do as the Romans do; better still, get a Roman to do it.

Although research links with departments or institutes in LDCs are now generally advocated and, less widely, adopted, it is not always clear what the purpose of these links is. One might distinguish between the following objectives from the point of view of the scholar in the developed country. (Different objectives would be listed if we looked at joint ventures from the point of view of the developing country or its scholars.)

(1) In order to conduct research in a particular country, a link with a domestic institute is helpful. It provides a source of data, collectors, interviewers, interpreters and other useful local resources. These are essentially ancillary services.

(2) The link may be used for more or less blatantly political and tactical purposes. It may facilitate gaining approval for conducting the project from the authorities; it may remove criticism in the local press and by public opinion, it may make it easier to gain access to certain sources of information or it may be an essential condition for this.

(3) A link may be thought necessary in assisting in the dissemination and application of policy-oriented research. Domestic

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<sup>1/</sup>It is this unity of scholarship that gives us the right, beyond a general appeal to human rights, to send petitions to foreign governments to protect the life and work of colleagues in danger of being imprisoned or tortured for their ideas. If this were not so, such petitions, based on professional solidarity rather than human rights, would be a form of intellectual imperialism.

institutes have often special links with government agencies and other policy-making bodies and the dissemination of research conducted in developed countries is facilitated when it is channelled through these institutes.

(4) In some cases, the prestige of foreign institutions is greater than that of domestic ones in the developing countries. A link may then serve to add "respectability" and acceptability to the work of a local institute, even though the foreign link is only tenuous: perhaps confined to an occasional exchange of scholars, brief visits, or only a letterhead.

(5) The link may be intended to build up and strengthen indigenous research capacity, whether of individual skills or of whole institutions. It is generally recognised that such capacity is highly desirable in itself and is a necessary condition for development. Outside links may then be a useful form of promoting this. If it were thought that this could be done better through teaching and assisting in curriculum design, the reply would be that research is best learned and taught by doing it. Teaching by itself (without research) is less effective in teaching how to do research, and would also attract less well qualified people.

(6) A link may be desired in order to improve, through the association, the teaching capacity of the sponsoring institution.

(7) Finally and ideally, the local institute may contribute professional expertise at the highest level. The indigenous scholars, being more familiar with the society and the local problems, may improve the design of the research project, may help to ask the right questions and prevent errors and may contribute to the right balance of emphasis. One may be even more ambitious and aim at what Susanne Hoeber Rudolph calls "participatory theory construction", an attempt to overcome the partial and fragmented vision of all social theory by complementing a "Western" approach by an "Eastern", a "white" by a "black" and "brown".<sup>1/</sup> If what I have said about the imperialist claims of partial social theories <sup>2/</sup> is correct, such participatory theory construction has remedial value and can contribute to a more comprehensive and truly universal theory of society. Just as the vision of a mountain changes according to the point from which we observe it, so African, Asian, Latin American or European partial visions may be brought together. An approach that joins different partial visions can give us a more accurate picture of reality. Participation may be the cure not only for political discord but also for intellectual distortion.

Some of these objectives 1 - 6 may be pursued jointly, but conflicts may arise. Collaboration agreements and joint ventures, in scholarship as in business, may be forms of window-dressing. They may pretend that dominance and dependence do not exist where they do.

In such a situation, objective 5, help in building up research capacity (which may include help in reducing a heavy teaching load or

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<sup>1/</sup>"Reflections of an Indian Scholar," mimeo.

<sup>2/</sup>See Pages 10-11.

help in reducing the time spent on supervising and organising research of others, by the few who could be better employed in doing the research themselves), may have to precede objective 6, genuine collaboration.

This roundabout way of strengthening research capacity raises important and difficult issues. The problem is sometimes presented as a trade-off between strengthening indigenous research skills and research quality. At least three questions should be asked.

- (1) What is the purpose of the support: is it research or training for research? If training, is the intention to create or improve individual skills or whole institutions?
- (2) What is the time horizon? Is it important to have results of the highest quality soon or is it preferable to forgo speedy results for the sake of more and better results later?
- (3) What risks is the donor prepared to take? The results of backing underdeveloped research in order to strengthen its capacity are even more uncertain than backing highest quality research. Some projects will fail or be disappointing. Is there a case for diversification in order to spread risks or, resources being scarce, should they be concentrated to reduce the risk of failure?<sup>1/</sup>

If it were merely a question of timing and risk-taking, the problem would be simple. The real difficulty is that there is an inconsistency between the teacher-pupil relationship and the equality of international collaboration. It is not arrogance, nor intellectual neo-colonialism, nor the assumption of superiority, but the fact of superiority which is at the root of the trouble. No amount of sensitivity, tact and courtesy can get round this fact, which may be more important in causing hostility and resentment than the propagation of wrong models or the existence of superior material resources. Ironically, the contribution of foreign scholars, where it is most realistic and relevant, may be most resented. For it is uncomfortable to be told by foreigners what is right and to have one's errors corrected by them.

The most fundamental argument for international cooperation in development is that human beings, wherever born, should be able to develop to the fullest extent their capacities, both in order to fulfill themselves and to contribute to the common heritage of civilisation, of which the stock of knowledge is part. In the light of this, the so-called trade-off between quality and capacity appears as an inter-temporal choice with an element of risk. But, human beings being what they are, how the fact of initial superiority and inferiority can be acknowledged and removed is a much more difficult question.

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<sup>1/</sup> In his discussion of this problem, Edgar Edwards makes the additional point that policy-makers in developing countries "should regard research by their own nationals, particularly on sensitive issues, as being better grounded in local knowledge and possibly more concerned with national welfare and long-term implications than research conducted abroad or by visiting scholars." Employment in Developing Countries, mimeographed, p. 75.

For objectives 1 - 4 collaboration is often more honest and more practical if both partners get something out of it. There is no objection to introducing an element of trading: you help me in getting data and I help you with designing a curriculum or in giving lectures and in reducing your teaching load. Arrangements for mutual benefit, not all of which need directly relate to the research project, are possible and often desirable.

A practical conclusion for donors who desire joint ventures in research is that funds may be required for consultation and for preparing the ground for cooperation. A particularly suitable area for feasibility or pre-investment or pilot studies would be grants for the exploration of suitable scholars and institutes for cooperation in research and the precise conditions for collaboration.

Another practical conclusion is the need to encourage and finance scholars from developing countries to conduct research in developed countries on the problems of these countries and on the problems generated by the world system of international relations. Only through such reciprocal and symmetrical arrangements can the idea of international cooperation of equal partners in research be realised.

So far, I have talked about links in general. Should there be specific links between particular universities or departments and institutes in developing countries? There are arguments for and against. The case for such links is that there is stronger commitment, a clear responsibility, advantages of continuity and of getting to know one another well. On the other hand, it may mean reduced flexibility, so that one is bound to accept students or staff members from the linked institutions to the exclusion of others who might contribute more, or that one engages in a series of joint research projects for which other partners would have been more suitable.

If participation and collaboration achieve complementarity, can there be also specialisation, a division of labour between research in developed and in developing countries? Basic research being a human-capital-intensive activity and often also equipment-intensive, it might be argued that most of it should be conducted in countries where sophisticated human capital and institutions are relatively plentiful and that developing countries should confine themselves to adopting and adapting basic research from the industrial countries and direct their own efforts at research that can be applied quickly and can best be done locally. Yet, such a conclusion would be misleading. First, the ability to adopt and adapt requires a research capacity not too dissimilar from that in the developed country. Workers must be able to appraise critically what is useful and what is not, and how the useful can be adapted. This ability presupposes research capacity of the same order and of the same depth as that of the original workers. Secondly, in view of the international market in professional social scientists, developing countries may have to attempt to match, or nearly match, the research opportunities their best brains are offered abroad if they wish to keep them. And keeping them may be a necessary condition for carrying out useful applied research with quick-yielding results. For both these reasons the need of an indigenous basic research capacity in order to adapt and the need to plug the brain drain -- developing countries cannot simply confine themselves to applied, quick-yielding, locally relevant research plus adaptation of research from abroad.

But there remains the question of balance and direction. If scholars are induced not to leave the country but carry out the kind of irrelevant, esoteric, excessively sophisticated, abstract work that gains prestige in the centres of learning of the developed countries, little is gained. To re-channel the external brain drain into an internal brain drain brings no benefit to the community. How this can be prevented raises difficult questions of academic prestige, social concern, conflicts between social and professional priorities and individual motivation. On these grounds a case might be made out for some closing in of the scholars of the developing countries, for reduced contacts with the international community of scholars, if such turning away encourages attitudes more in line with social priorities. On the other hand, if the scholars in the developed countries recognise that what is regarded as "the best" in one setting might be the enemy of the good in another and adjust, not only their research priorities, but also their pecking order of recognition and prestige, the objections to international cooperation across wide-open frontiers are weakened or removed. Paradoxically, by lowering the claims of prestigious subjects and "standards of excellence", by making analysis more flexible, standards less universal and policy recommendations more diverse, greater universality and better international collaboration can be achieved.

#### IV. A CLARIFICATION OF THE MEANING AND PURPOSE OF INTERDISCIPLINARY STUDIES

Earlier I discussed briefly the charge that "Western" economics does not apply to the developing countries and, if it is applied, leads to biased analysis and policy. The Keynesian analysis of employment and unemployment, the monetary analysis of inflation, the static theory of comparative advantage, of the tendency to factor price equalisation and of the gains from international trade, the heavy emphasis on investment and capital accumulation -- these and other doctrines have been heavily criticised as inapplicable to apparently similar phenomena (unemployment, inflation, international trade) in developing countries.

One important line of attack on "Western" concepts and models is to say that the boundaries of the disciplines tackling these problems have been wrongly drawn. If the voices of discontent are louder than the voices proposing constructive alternatives, this is due, it is argued, to the absence of an appropriate body of knowledge, a discipline more relevant and more adequate to the reality of the developing countries.

With almost monotonous repetition, we hear calls for interdisciplinary studies (or, more accurately, for the breakdown of boundaries between disciplines) at conferences, in papers and books on development problems. Yet, relatively little is done about it. This is partly due to lack of clarity on what is meant by the plea and partly due to the fact that the only forum where interdisciplinary studies in depth can be conducted successfully is under one skull, and that such skulls are scarce.

There are three quite distinct reasons for interdisciplinary, multidisciplinary or supradisciplinary work. Each has different methodological implications. First, a practical problem (improving nutrition, introducing new varieties of crops, controlling population growth, planning a town) may call for drawing upon and applying several disciplines. In the cooperative effort the disciplines are not transcended but brought together to solve a particular set of practical problems. The prevalence of planning at all levels has contributed to the cooperations between different disciplines. The planner has to draw on all relevant knowledge and skills. This practical need to bring all relevant methods, data and information to bear on the solution of a specific problem does not affect the methods used in the contributing disciplines. On the contrary, it is just because they are specialists in their fields that the different members of the team have a contribution to make to an integrated solution. We might think of them as members of a Royal Commission investigating problems of conserving our environment or deciding upon a family planning programme or planning a new town.

Second, it may be the case that certain assumptions, concepts or methods, hitherto applied only to one sphere of activity, yield illuminating results when applied to another, previously analysed in quite different ways. There has been some invasion of economic concepts and methods into the territory of political scientists and sociologists. Thus, the assumption of maximising behaviour has been fruitful, up to a point, in analysing the behaviour of consumers, firms and farms. Its success has encouraged its application to political activities such as voting and party formation. It has also been applied to the exploration of politically feasible income distributions. Calculations of economic returns have been extended from profit-making investments to education, health, birth control and the allocation of time between work and leisure and of leisure time itself. Occasionally, though much less frequently, concepts used in political theory have been applied to economic problems. Albert Hirschman's use of "voice" as an alternative to "exit" is an interesting example.<sup>1/</sup>

Third, it may turn out that for a particular time or region the justification for having a separate discipline does not hold. This justification for a discipline consists in the empirical fact that between the variables encompassed by this discipline and those treated by another there are few interactions and the effects of any existing interaction are weak and damped. Only then are we justified in analysing causal sequences in one field, without always and fully taking into account those in others. We may all agree that society is a system and that all social variables are related, but with growing differentiation of functions and standards, some relationships are stronger than others. This justifies us in separating, say, business responses from family responses, or economics from anthropology.

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<sup>1/</sup> Albert O. Hirschman, Exit, Voice and Loyalty, Harvard, 1970.

If interdependence between variables normally studied separately is strong, or, though weak, if reaction coefficients are large, or, though small, if they change size for moves above a critical level, there is a case for breaking down the boundaries between disciplines. This is sometimes called transforming parameters into dependent variables. Family ties and economic calculus, land tenure and responses to incentives, religious beliefs and commercial motivation may in this way interact. Where interdependence of this kind occurs and where the interdependent variables belong to different disciplines, there is a case for interdisciplinary work.<sup>1/</sup>

It is possible to draw two quite different conclusions from such interaction. First, it might be said that what is called for are not interdisciplinary studies but a new discipline that constructs concepts and builds models appropriate to the conditions of underdeveloped societies. In this case, we should have to discard concepts like employment, unemployment, underemployment, income, savings, investment and construct altogether new terms.

Second, and less radically, the existing concepts and models may continue to be used, but their content may have to be changed or their definition modified.

The difference can be illustrated with the concepts "capital" and "investment". Conventionally, "investment" is defined as the addition of physical pieces of equipment, plant or stocks in order to raise the future flow of products or services above what it otherwise would have been. "Capital" is the stock of these items that has resulted from past flows minus depreciation through use and obsolescence.

Now it is possible to enlarge this concept so as to cover all forms of expenditure that lead to a larger flow of future output, not only those that result in physical items of machinery, constructions or inventories. This would include "investment in human capital", such as education, health and, at low levels, nutrition; possibly expenditure on birth control if we are concerned with raising income per head; it would also include investment in productive knowledge that is not incorporated in machinery or in people; it would further include expenditure on building social institutions and on shaping human attitudes. All this can, in principle, be covered by an enlarged concept of "capital" and "investment," as long as one condition is fulfilled: there must be a fairly systematic connection between the devotion of current resources (that might otherwise be used for unproductive current consumption) and the resulting flow of

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<sup>1/</sup> An important attempt to discover which variables and which interactions may be appropriate under which circumstances is to be found in Irma Adelman and Cynthia T. Morris, Society, Politics and Economic Development, (Baltimore: Johns Hopkins Press, 1967).

extra output. These resources need not be the only condition for the enlarged flow of output, but they must be systematically linked to this output by a fixed set of technical coefficients or at least by a range that is not too wide.

But if the link between the devotion of current resources and extra output is only tenuous, so that a given result can be achieved with widely varying inputs, or the same inputs can lead to widely varying results, or if the results can be achieved without using any current inputs, or if in spite of large current inputs, no results can in principle ensue, the notions of "capital" and "investment" become inapplicable and we have to focus on those factors on which the outcome systematically depends.<sup>1/</sup> The output of a factory may be within wide limits a function of the degree of capacity utilisation which, in turn, will depend upon the quality of the management; the result of a family planning programme may be only tenuously linked to the money spent on clinics, doctors, nurses and contraceptives but largely depend upon changeable economic, social, cultural and religious attitudes of couples. The quality of the administration, the system of land tenure, the solidarity between different classes, the ethnic origin of the entrepreneurs, their power over the workers, the history of the country may be more important determinants than the amount of resources. If this is so, no new wine can be poured into old bottles; the bottles themselves have to be changed.

In either case, we may in the process of analysing social phenomena in underdeveloped countries incidentally gain new insights into those in advanced industrial countries. Studies of the caste system in India may illuminate trade union behaviour and demarcation (jurisdiction) disputes in England or the role of women in Western Europe; scrutiny of the capital/output ratio may change our view of the production function. Research into small-scale, intermediate technology may be useful for reducing environmental damage in advanced countries; research on the social services may teach us how to improve our own; a wider concept of capital may throw new light on our own problems of industrial management. If this happens it will be a bonus over and above what we had bargained for. Research can thus be made not only cooperative but also mutually enlightening.

Let us illustrate how this would work out between, say, economics and anthropology. In the first case of interdisciplinary work - the team approach - anthropologists will be used for their traditional training. If a land reform or a birth control programme or a tourist project or even a research project is proposed, they will be able to point to "constraints" in the beliefs and mores of the people, or they

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<sup>1/</sup>"As Voltaire said, an incantation will destroy a flock of sheep if it is accompanied by a sufficient dose of arsenic." Official Papers by Alfred Marshall, Macmillan, 1926, p. 40.

will be able to point to beliefs or institutions which can be mobilised and on which the proposed reforms or projects can be built. Nothing new or radical is required here.

The second case is more interesting. I suspect that economic method could illuminate some anthropological work and probably the other way too. While I know of some cross-disciplinary work of this kind between economics and political science, I do not know of any between economics and anthropology.

The most interesting possibilities are opened up by the third case, whether in its reformist or radical version. It is quite clear, for example, that an agricultural production function in many underdeveloped countries should count among its inputs, not only land, labour, fertilisers, water and power, but also levels of education of the farmers, nutritional standards, distance from town, health, systems of land tenure of family kinship. All these variables are likely, in some societies, to be systematically related to agricultural production.

Still, while we stick to the notion of a "production function", a status conscious anthropologist will complain that he is being used only to provide fodder for the canons of the economist. A self-respecting anthropologist might refuse to have all the important questions asked by the economist and to be reduced to a handmaiden, supplying low class empirical material for the high-class analytical structure of another discipline. Questions of status and precedence are, of course, not of concern to serious scholars; on the contrary. John Maynard Keynes looked forward to the day when economists would have become like mechanics, when they "could get themselves thought of as humble, competent people, on a level of dentists...."<sup>1/</sup>

But it may turn out that the whole notion of a production function is wrong or misleading. Perhaps there is no systematic relationship between inputs, whether of fertilised, irrigated land (physical capital) or of educated farmers (human capital) and crops. It may be that output depends upon variables that have been constructed and analysed by anthropologists: the relationship between majority and minority groups; religious beliefs (the Protestant ethic); or kinship systems. Or again, at a different level of discourse, it may be that large increases in output beyond a decent minimum are not a crucial component of development either at this stage or ever. The society may have opted for an alternative style of development, in which the ever-growing production of material goods is rejected. It prefers containment of wants and aspirations to growing production to satisfy ever-growing wants and infinite aspirations.

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<sup>1/</sup> John Maynard Keynes, "Economic Possibilities for our Grandchildren", reprinted in Essays in Persuasion, London, Macmillan, 1933.

Or, through a shift in valuations, unemployment may be converted into leisure. If this is the case, the crucial questions will have to be asked by the anthropologist or the sociologist. He has to construct the concepts and it may be that it is then the economist's turn to fill the boxes constructed by the anthropologist. Which of these possibilities should be realised will depend, partly, upon empirical conditions, but, ultimately, upon valuations and the choice of a style of life.

What is the conclusion for those dispensing money for policy-oriented research? The first case (members of each discipline solving a practical problem together) presents no problem. Teams can be, and have been, assembled who put their heads together to solve a particular problem, each member bringing his expertise with him. Interesting organisational, managerial and administrative problems arise about the levels and the forms in which the heads should be brought together and about the relative weight to be given to each in the light of possibly controversial objectives, but this is not our concern here. The second and third cases (extension of methods to other fields and changing concepts and models to include "outside" variables) are much more difficult. The only solution is to back individuals and groups of individuals who are interested in and excited by this work, who read each other's drafts and discuss their ideas among themselves and who are sensitive to the limits of the techniques in which they were educated and in which they have acquired a certain vested intellectual interest. I suspect that such backing will be more successful than backing chairs, institutes or projects that declare, programmatically, that they are committed to interdisciplinary work, though institutional change clearly must accompany attitudinal change. A modest practical proposal is to give grants to scholars for the purpose of reading and catching up in fields outside their narrow professional expertise, so as to familiarise them with what has gone on in these fields. Problem-oriented survey articles, critically summarising the literature and pointing to gaps, without respect for the traditional boundaries between disciplines, can also be initially a help in introducing outsiders to the work done by groups of workers.

Interdisciplinary work raises some of the same issues as collaboration between scholars from different countries, as is shown by our use of language: the "frontiers" of development economics, "poaching" on other people's territory, intellectual "imperialism" as an attempt to extend the "frontiers", etc. Here, as for international collaboration, considerations of sovereignty, status, power and legitimacy are out of place. The language of research should be quite different from the language of diplomacy or power politics and should have no room for "legitimacy" or "sovereignty".

Applied development economics clearly is a service discipline: it gets its input from engineers, scientists, agronomists, technologists; it delivers its output to policy-makers. It is noteworthy that research

on the "inputs", essentially research in the natural sciences, has been more readily welcomed by the developing countries than the research at the next stage of processing. This is so because, as we approach the final use of the product, viz. policy, both valuations and biases are more likely to colour the analysis. Problem-oriented, applied, usable research can overcome these objections, not by emulating the natural sciences in being "value-free", but in making the valuations explicit.

Economic analysis has found certain assumptions convenient. Among these are, e.g., optimising behaviour of atomistic units; convexity (e.g. absence of increasing returns and of indivisibilities), diminishing marginal utility; constancy of certain determinants such as tastes, knowledge, institutions; absence of uncertainty or the construction of certainty equivalents or of "contingent commodities", the prices of which are all known; fragmentation of power and hence inability of any one unit to affect the behaviour of other units. These assumptions, while convenient in yielding determinate equilibrium solutions, have been criticised and have occasionally been relaxed. Some of these relaxations amount to breaking down the barriers between disciplines (e.g. stepping into politics). The only criterion for this must be the realism and relevance of the resulting research and, beyond that, direct and indirect implementation of the results of research for the benefit of men.

#### V. AREAS FOR RESEARCH: AN ILLUSTRATION

Let me illustrate the impact of policy-oriented development research: the problems it raises, the responses it evokes, the impact it has.

One may distinguish, very broadly, between three schools of thought, each advocating a different strategy to eradicate poverty and reduce inequality.<sup>1/</sup> They may be called, for want of better names, the Price Mechanists, the Radicals and the Technologists.<sup>2/</sup> The Price Mechanists argue that low production, low productivity, inequality and unemployment can be eliminated by setting the "correct" prices,

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<sup>1/</sup>In line with most current discussions, I focus on inequality in the distribution of income and assets. But these are only a small part of the problem. More important are inequalities of power or access to power, status, prestige, recognition, satisfaction from work, conditions of work, degree of participation, freedom of choice, and many other dimensions. I am concerned with efficient increases in equality and am not concerned with so-called trade-offs between equality and growth.

<sup>2/</sup>See F. Stewart, "Technology and Employment in LDCs" (mimeographed), who makes the point of the need for a three-pronged attack.

which serve both as signals and as incentives.<sup>1/</sup>

There is a powerful, growing and vocal group of development economists who argue that much, if not all, of the disappointments with development efforts are due to faulty government policies. Governments have set the wrong price for capital (too low and often rationed, encouraging excessive scale and underutilisation of capital, discriminating in favour of large firms and encouraging take-overs of local by foreign firms), for labour (too high, contributing to unemployment and underutilised capacity and discouraging exports) for the foreign exchange rate (overvalued, discouraging labour-intensive exports and encouraging high-cost import substitution), for the products and services of public enterprises (too low, subsidising the private modern sector). If only governments were to set the right prices, economic growth, as well as jobs and justice, would triumph. Indeed this group argues, many of the evils attributed to foreign investment, the multinational enterprise, the wrong technology, inappropriate products, the dominance of the developed country, the terms of trade, international inequality, etc., are really due to "distortions", to faulty pricing policies which convey the wrong signals and provide the wrong incentives.<sup>2/</sup>

Most people would agree that "getting prices right" is not enough. Some would say it would go a long way towards combining more growth (and more efficient growth) with greater equality, others would say that the contribution would be only marginal, but all would agree that other things would have to be done as well, if only additional marketing efforts for the extra exports generated by the "right" price policies, or better facilities to improve the capital or labour markets. (But some would argue that the "right" prices themselves would provide sufficient incentives for the creation or improvement of these institutions.)

But to say "getting prices right is not enough" is open to two diametrically opposite interpretations. It might mean either that by itself it would make a contribution to the eradication of poverty and to greater equality, though this contribution would be greater if other things were done as well. But getting prices right is better than nothing. Alternatively, it might mean that while correct pricing policies combined with structural reforms, and in particular the redistribution of assets, would contribute to growth and equality, by themselves they might make matters worse or simply alter the manifestation of inequality.

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<sup>1/</sup> The function of prices to serve as signals and as incentives can, of course, be separated.

<sup>2/</sup> Some members of the school believe that all this is due to lack of understanding of basic economic analysis, others that the "distortions" serve entrenched vested interests. Which of these views is correct makes, of course, a difference to the policy prescriptions.

No doubt the "wrong" price policies can impede development, reduce employment, strengthen monopoly and aggravate inequality. But it does not follow that the "right" policies necessarily do the opposite. They might simply lead to different forms of the same evils. Let us assume that land and real capital equipment are scarce, while unskilled labour is plentiful. The supply of labour is growing faster than that of land and capital. We allow competition to prevail and factor rewards to be determined by marginal productivity. And we allow for a fair degree of substitution between labour and capital. Then rents per acre and real returns on capital will be high and rising (reflecting the growing relative scarcity of land and capital), while the wage rate will be low and falling (reflecting the growing abundance of labour). Producers will pay much to get hold of the scarce resources of land and capital equipment and will offer little for the abundant supply of workers seeking jobs. Low wage costs will tend to expand employment and output; the extent depending on the elasticity of substitution. Processes, sectors and products<sup>1/</sup> that are labour-intensive will be encouraged and those requiring land and capital discouraged. Foreign capital will be attracted from higher-wage, lower-profit countries. High profits and low wages will tend to encourage domestic savings and hence increase the supply of capital inside the country. Moreover, there will be incentives to invent new methods and products that use labour and save land and capital. All this is fine and as it should be.

But these desirable incentives depend upon wages being low (and, if labour grows faster than land and capital, falling), while rents, interest and profits are high (and rising). If in such a society the distribution of land and capital were to be very equal (peasant proprietors or socialised ownership of means of production, though the latter would raise questions as to how the state determines wages and the disposition of the surplus and also about the unequal distribution of power), the functional differentials would not matter because personal or household equality would still prevail. What a family loses on labour income, it gains on property income. But if property (land) distribution is unequal, if the ownership of assets, including access to educational opportunities, is highly concentrated, inequality might increase, even if the share of wages went up as a result of a fairly high elasticity of substitution. The difference between a high-wage, low-interest and a low-wage, high-interest policy is that in the first case the evil takes the form of unemployment, in

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<sup>1/</sup> Usually, more attention is paid to processes than to sectors and products. But it may be more rewarding to encourage labour-intensive sectors, like the non-organised sector, and appropriate products, than processes which are often dictated by the choice of product and sector. Cf. Frances Stewart, loc. cit.

the second of inequality between wage earners and property owners. "Getting prices right" may therefore transform inequality within the working class (between those with and without jobs) into inequality between workers and owners of assets.

To conclude: correct pricing is certainly not enough where ownership of assets is concentrated and, by itself, may make matters worse. This does not mean that correct pricing, combined with other policies, has not an important part to play.

This brings us to the second school of thought: the Radicals. This school believes that what matters is to redistribute assets, power and access to income-earning opportunities. Only through such "structural" and institutional reforms, whether peacefully and gradually or through revolution and quickly, can growth and equality be achieved. To some (extreme) members of this school, there is only one road to salvation. The whole revolting, suffocating mess must be flushed away. What is to take its place is irrelevant. Even to ask that question reveals a desire to preserve the Establishment. To destroy is also to create.

"The fiery anarchist agitator Bakunin ... was saying something of this kind: the entire rotten structure, the corrupt old world, must be razed to the ground, before something new can be built upon it; what this is to be is not for us to say; we are revolutionaries, our business is to demolish. The new men, purified from the infection of the world of idlers and exploiters and its bogus values -- these men will know what to do. The French anarchist Georges Sorel once quoted Marx as saying 'Anyone who makes plans for after the revolution is a reactionary'."<sup>1/</sup>

Those advocating this line are open to the criticism advanced by an examiner of the development paper in the Oxford Final Honour School: "Several candidates, having argued convincingly that a revolution would be a necessary condition of economic development, in a certain country, concluded that it would be a sufficient one". Even, or rather especially, revolutionary juntas must plan signals and incentives<sup>2/</sup> for development

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<sup>1/</sup> Isaiah Berlin, *Fathers and Children*, The Romanes Lecture (Oxford, 1970), p. 26.

<sup>2/</sup> Signals and incentives need not be material ones; they may be moral ones. But many socialist economists have argued that the price system comes into its own under socialism.

in considerable detail.<sup>1/</sup>

Other less extreme members of this school advocate either expropriation with compensation, though unless the compensation falls short of the pre-expropriation value of the expropriated asset, there will be no redistribution. Others again would confine redistribution to additional assets as they accumulate over time, and would bring about a more gradual redistribution, say by the transfer of a certain proportion of annual savings and investment to low income groups. Whether such incremental redistribution is possible in the face of an unequal power structure is controversial.

Both revolutionary and evolutionary, both average and incremental redistributive reforms have failed because of a failure to provide the signals and incentives to make the assets now owned by the poor at least as productive as they were when owned by the rich. Soviet Russia had a long struggle with its peasants. The Soviet hammer has been more successful than the sickle, largely because of failure of price incentives. In Cuba, "Market 'anarchy' has been replaced ... by the anarchy resulting from the interplay between chaotic decentralisation (chaotic because micro-economic units, even if totally unselfish and devoted to collective welfare, have no accurate signals to guide their actions into socially optimal channels), and authoritarian centralisation, which try as it may, is unable to coordinate and direct efficiently every decision involving resource allocation."<sup>2/</sup> Cuba's sugar estates and Chile's copper mines under Allende might have contributed more to the incomes of the

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<sup>1/</sup>The methodological similarity of the Price Mechanists and the Revolutionary Marxists reveals their common origin in 19th century liberalism. The early Utopian socialists were more realistic and are more relevant today in their emphasis on detailed planning of institutions and incentives.

<sup>2/</sup>Carlos F. Diaz-Alejandro, Journal of Economic Literature, vol. XI, No. 1, March, 1973, p. 92.

poor had signals and incentives not been neglected.<sup>1/</sup> Ironically, some members of the Radical School commit the same error as the Price Mechanists: they mistake a necessary for a sufficient condition. In this way, they contribute to a redistribution of inequality, not to its reduction, to a perpetuation of poverty, not to its eradication.

Both the Price Mechanists and the Radicals are (often implicitly) optimistic about technology. They believe either that the technologies appropriate for the eradication of poverty and for the promotion of greater equality already exist, or that the "right" prices or the redistribution of assets will automatically provide the incentives to invent them. The third school, the Technologists, are not so optimistic.

They approach the solution of the problem of poverty, unemployment, inequality and low productivity like that of putting a man on the moon, or nearer home, discovering new high-yielding varieties of wheat, maize and rice. Industrial technology, public health, low-cost housing, birth control, nutrition, crops for small farmers, urbanisation, require the concentration of brain power and research resources. The Technologists are right, in so far as neither prices and incentives nor "structural changes" can solve a problem where the appropriate technical solution just does not exist: where it has to be invented or discovered and where the incentives are too weak or too slow-working to produce "automatically" the right solution.

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<sup>1/</sup>"...Chile's resource situation has worsened under the impact of a drastic decline in aid from the West and virtual elimination of private inflow of foreign capital -- so that the economic regime is under severe stress, calling particularly for improved export-performance, much the way we face this necessity. And yet Allende's advisors have given up the "sliding" exchange rate scheme of the earlier Frei government, which worked so well, and the balance of payments situation in consequence has continued to deteriorate disastrously. On a recent visit to Santiago I found that the system had broken down to a point where the divergence between the official and the unofficial parities was of the order of 1 to 10 -- thus resulting in a situation where selling foreign exchange at the official rate had become an act of honest idiocy that no one, including the distinguished members of the United Nations Secretariat in Santiago, was willing to perpetrate! Ironically, the resulting sabotage of the Allende regime's admirable efforts at socialist transformation has been far more effective than anything the ITT or the U.S. State Department could have planned or even implemented!" Jagdish N. Bhagwati, "India in the International Economy: A Policy Framework for a Progressive Society", Lal Bahadur Shastri Lecture, delivered before the fall of the Allende government.

Consider, by way of illustration, the need for a capital-saving, efficient technology to provide jobs for all willing and able to work. With existing technology and fixed technical coefficients, only between 1 and 2 percent of the additions to the labour force can be employed, if we assume that the labour force in developing countries grows at 3 percent compared with 1 percent in developed countries, that income per head is one tenth and that the savings ratio out of these lower incomes is one half. An appropriate technology to employ only the extra workers entering the labour market each year would have to be such that the investible resources per worker would be only one sixtieth of what they are in developed countries. Even allowing for some substitution between labour and capital, this is a large hole in the production function to fill.

Technological innovation, in this context, should be interpreted broadly. It includes innovation in institutions. Just as the appropriate hardware may be non-existent, so appropriate institutions may have to be invented. Management, administration, organisation, like physical techniques, have been developed in the West to solve labour scarcity, to meet the demand of high incomes, in an environment of temperate climate. Rural institutions to meet the needs of a large, rapidly growing, poor rural labour force make quite different demands on the institutional imagination than those we have tended to transfer from the developed countries.

The technological solution would appear to be particularly appealing both to researchers in the developed countries and to policy-makers in the developing countries, because, on the face of it, it does not violate vested interests and therefore seems to escape political opposition. It appears to lie beyond ideology: ideologies of both the "right" Price Mechanists and the "left" Radicals. It seems to tackle problems in a scientific, practical, workmanlike manner.

But technology is both result and cause of income, asset and power distribution in the national and international system. As the "Green Revolution" has shown, if the distribution of assets like water, fertilisers and credit is concentrated, it is the large farmers with controlled water supply who benefit, in some cases at the expense of the small farmers and landless labourers. A technology, specifically invented to overcome food shortages for the growing number of poor people, has reinforced and aggravated rural inequalities. Once again, the technologists like the Price Mechanists and the Radicals, may aggravate poverty and inequality or change its form.

Less than 2 percent of total R&D is spent in the developing countries and only a fraction of this on the problems of poverty. But even if a substantially larger proportion were spent, this would not mean that poverty would be eliminated, if the institutional arrangements and the power structure necessary for dissemination and application are weak or absent. And organised interests will oppose measures that hurt them.

absent. And organised interests will oppose measures that hurt them.

The conclusion of the discussion is by now plain. We have an instance of the Theory of the Second Best, according to which A + B + C yield the desired results, but A or B or C by themselves, far from being "better than nothing", may move the system away from the desired goal.<sup>1/</sup> Only a three-pronged attack, combining signals and incentives, institutional

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<sup>1/</sup>The three schools clearly do not cover all relevant issues. In particular, I have left out the school that advocates using fiscal policy to distribute consumption goods and services to the poor. Disregarding the difficulty of differentiating at very low income levels between unproductive and productive consumption, such redistribution as the sole measure of eradicating poverty and promoting equality has serious drawbacks. First, developing countries do not have an adequate fiscal machinery. Second, in order to maintain equality, redistribution of consumption goods and services would have to grow not only in absolute terms, but also as a proportion of national income. This would raise administrative, economic and political difficulties. Third, people may wish to earn their income rather than have it doled out. None of this implies that subsidising consumption of the poor has not a supplementary part to play. It is by now well known how public services, like health and education, without other reforms, largely benefit the middle class.

Another area that I have neglected is the international impact on strategies for equality. Can countries pursue policies for equality, while remaining wide open to communications, foreign investment, the multinational enterprise, foreign technology and foreign products, and the whole structure of international relations with advanced industrial countries? As I say in the text later, this may be regarded as a particularly fruitful area for rich-country research.

Also left out of the discussion are intertemporal choices. By keeping the income of the lowest 40 percent down for a number of years, a country (it has been argued) may be able to raise them to a higher level after a period than if it had raised them earlier. This is the proper formulation of the choice "growth versus equity".

Finally, I have not discussed the important problem of how equality, once established, can be maintained in the presence of increasing returns, cumulative processes and unequal distribution of inherited characteristics.

reforms directed at the redistribution of assets (including access to education) and technical and institutional innovation, promise results. The precise combination of price policies, asset redistribution and technological research will depend on a number of factors that will vary between countries: on the readiness of vested interests to yield, on the elasticity of substitution between factors on the nature of the interdependence between sectors, on the degree of concentration of ownership, on the productivity of assets when redistributed, on the fiscal system, etc. But it is only on the three legs of this tripod that efficient redistribution can rest.

## VI. IMPLICATIONS FOR RESEARCH PRIORITIES

Two opposite conclusions might be drawn from the discussion in section V. One would be that we should identify the priority areas and then tackle them jointly, by rich-poor country cooperation. In particular, the interaction between (1) signals and incentives, (2) structural changes in the distribution of assets, access to power and access to education and (3) technology could provide an Agenda for joint, international, multidisciplinary research.

The other approach would be to say: yes, these are the important problems but they are not suited for joint research. Purely domestic issues should not be on the Research Agenda of rich country scholars and institutes for four reasons. First, there is little rich countries can do to implement policies for wholly domestic matters of the developing countries, whereas rich countries can act in the international arena. Second, research on purely domestic issues gives rise to the charges of mining and, possibly, undermining, discussed on p. 7. I have argued that such charges are often based on a confusion, but the political and psychological obstacles are nonetheless real.

Third, there is something paradoxical (though not contradictory) in Foundations, Agencies or Institutes from rich countries advocating the need to strengthen indigenous research capacity as an important ingredient in development and then doing the work themselves. I have argued that a somewhat Boehm-Bawerkian approach<sup>1/</sup> can make the two consistent, but again some resistance may remain.

The fourth objection is the most serious one and I am schizophrenic about it. Assume a careful analysis leads to the conclusion that

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<sup>1/</sup>Boehm-Bawerk was a famous Austrian economist who reasoned that greater production later resulted from devoting current resources to more "roundabout" methods of production, e.g. rather than training students, training trainers of students.

a radical redistribution of assets and power is a condition of progress. Can we then tell citizens of other countries to adopt these radical changes which may require a revolution? At the level of independent, objective analysis, there is nothing wrong with saying such changes are necessary, where they are seen to be.<sup>1/</sup> But for an outsider to say this may be condemned not only as an easy option (nothing is easier than to be radical for another country), but also as counterproductive. These changes, by definition, are going to hurt some people. If these people can point to the outside agency as the source of inspiration of these changes, this may make it more difficult for the progressive group to carry out their intentions. What some may regard as supporting and well-wishing outside pressures, others will see as the kiss of death, or at least an embarrassing embrace. So here is the dilemma. Honest research bids us expose the political constraints and point to the radical solutions, but it may be both improper and counterproductive for foreigners to recommend painful and possibly bloody domestic reform.

The second approach (ruling out domestic issues) leads to the conclusion that the appropriate area for joint work is where the actions of rich countries impinge on the poor countries, what in the current jargon is called the interface: international trade, including adjustment assistance, aid, capital movements, the multinational enterprise, international monetary reform, energy, the environment, the sea and the sea-bed, migration, transfer of technology, the direction of research and development expenditure in rich countries, science policy, international taxation. These are areas in which data are available without intrusion and in which analysis and concern by the rich can lead to action and improvement by the rich. International issues could therefore be regarded as those suited par excellence for development research in and by developed countries.

There are objections to such an ordinance of self-restraint. The self-restraint can work properly only if it is obeyed universally. For in the light of the previous discussion if scholars not obeying the ordinance move into a developing country, advocating a partial solution, such as the importance of getting prices right, without stressing the other components of the strategy, they might inflict more harm than good. Balancing action to neutralise such damage might then be needed. Equally, if the partial conclusion were reached that a revolution is necessary condition for development, would it not be facile to permit this to be presented to another country, without work on the details of other policies that would have to accompany the radical reform? Since not everybody can be prevented

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<sup>1/</sup> There is a school of thought that advocates "participatory observation", "participant intervention", the role of "militant cum observer" and "liberation anthropology". Without going into the merits of this case, such activities are bound to remain a minority interest.

from working on internal problems of the developing countries, anybody must be free to do so, if only to counteract one-sided and biased research. 1/

Secondly, if, as has been argued, preoccupation with prestige-endowed topics that are irrelevant to the development efforts of the developing countries leads to internal brain drain<sup>2/</sup>, attention to relevant, though domestic, topics by scholars from rich countries may stimulate interest in these topics and attach indigenous prestige to them. Work by a foreign scholar on income distribution in Brazil has, if not initiated, certainly provoked, a good deal of excellent domestic work.

Thirdly, the distinction between international and internal topics is analytically untenable. The international system penetrates national affairs and vice versa.<sup>3/</sup> This aspect was neglected in the previous section, but clearly the tripod is not practical for a small country, dependent on foreign trade, foreign investment and foreign technology. It is through the blast of international relations that the otherwise steady tripod may be upset.

Fourthly, the best research is inspired by what the researchers think they can do best and what interests them. It would be a pity if there were no funds to back these efforts.

Finally, I have argued that scholarship has its standards, which ignore the maxims of etiquette, diplomacy and tact. If, after honest research, in the light of the evidence, politically sensitive conclusions emerge, they should not be suppressed, dressed up or softened. But it is well to remember that cooperative research calls for standards of cooperation.<sup>4/</sup> Jagdish Bhagwati, in his

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1/ Another example are some of the currently fashionable doctrines of project appraisal. These profess to cover domestic social objectives, such as income redistribution and higher savings rates. Yet, if propagation of these doctrines were to proceed, while the critics of these methods kept silent, the analysis and policies would be one-sided and biased.

2/ T. N. Srinivasan, "The State of Development Economics", mimeographed paper distributed at the OECD-IBRD Belgrade Conference of Directors of Institutes of Development Research and Training.

3/ Thus British aid to overseas education can be analysed only in the context of the educational systems of the developing countries. International trade policy cannot be isolated from the domestic allocation of resources.

4/ Here again, work on the Brazilian data of income distribution has a moral. The complaint in Brazil is that officials sympathetic to a critical analysis provided a foreign scholar with the data but that he, without reference to them and without consulting them, published them speedily in a foreign journal. Revolutionary or radical mining and imperialism is regarded as just as objectionable as conservative mining.

contribution to the Panel discussion on "What we need to Know" at the Princeton Conference on International Trade and Finance (March 1973), tells the story of how Frazer, the great anthropologist from the pre-Malinowski, pre-Radcliffe-Brown era, when asked if he had ever visited the exotic areas he wrote about, replied: "I only write about savages, I don't mix with them." Bhagwati goes on to note that the Foundations, AID and the World Bank, supported by the jet, had removed any obstacles and Frazerian inhibitions to the pursuit of knowledge, if not pleasure. But there may be a conflict between opportunities and objectives. Just as the hordes of tourists in search of exotic sights destroy the very mysteries they have come to see, so scholars may interfere harmfully with the processes they have come to study.

In addition, there is a certain process of selection at work, encouraged by high living standards abroad, and the relative absence of equally high academic standards. Some years ago, Dudley Seers wrote a memorable article on "Why Visiting Economists Fail". Perhaps the time has come to write a companion piece on "Why Failed Economists Visit". T.N. Srinivasan, in his already quoted Belgrade paper on "The State of Development Economics" notes, rather generously and politely, that "there are situations in which the value of the contribution of the resident foreign economist covers his marginal cost."

I find it difficult to give a clear answer to the questions about the limits of research by scholars from rich countries, except that research into international issues, and particularly those areas in which action by rich countries can contribute to development, is in some respects more appropriate and safer, in spite of the objections mentioned, than research into entirely domestic issues of the poor countries. The "adjustees" of adjustment assistance are more appropriate subjects for research than the victims of a foreign revolution. But if the "interface" penetrates into largely domestic issues, no barrier can halt the progress of research.

Yet, there is another problem. Even where national interests coincide, and where appropriate policies would lead to common gains (such as access to imports from LDCs), there is a question about the division of these gains. In other areas, national interests may clash. Certain gains for the developing countries may be available only at the expense of developed countries. Are such problems of bargaining and possibly confrontation and conflict appropriate areas for rich country research? Alternatively, there might be schemes of self-help, where the best policy would be for developing countries to turn away from the rich and encourage arrangements among themselves (regional integration, payments unions, etc.). Could such problems be studied in developed countries?

In spite of the suspicion that such research may be nationally biased and self-interested, I can see good reasons why it could be usefully conducted by rich countries in cooperation with poor. First, until the work has been done, it is not always clear whether the game is a zero sum one or whether positive sums may be available for distribution. It is quite legitimate to investigate the conditions in which private foreign investment, freeing trade or permitting immigration help both groups of countries and when it is beneficial to one at the expense of the other.

Second, even where interests clearly conflict, informed bargaining is often to the advantage of both partners. Representatives of multinational enterprises often insist that they prefer to negotiate with well-informed, hard-headed officials from developing countries to having to face ignorant and incompetent ones (though OPEC may have gone too far for them). The short-term advantages that may be gained from the latter, they argue, are not worth the recriminations, regrets and retaliatory actions that spring from the later reactions to ill-informed bargains.

Third, developed country scholars are quite capable of detaching themselves from the narrow national interests of their own country and of analysing conflict situations that can be resolved to the advantage of the developing countries. Some of them are eager to do this. The exploration of areas of potential bargaining power is only beginning. What is needed is more information about this power and greater solidarity between competing developing countries to exploit it jointly. A preliminary list of areas to be investigated would include the following:

- Commodity agreements combined with restrictions of supply in commodities or groups of commodities for which world demand is price inelastic, so that producing countries can jointly raise the price of the agricultural commodity or mineral or a group of such commodities in order to reduce substitution, and improve their income terms of trade.
- Contracts with the multinational enterprise, covering a host of clauses, containing conditions for foreign operations.
- The threat of confiscation or nationalisation of foreign-owned assets or restrictions on new investment or the imposition of various conditions on such investments.
- The threat of refusing or slowing down or tying debt service on past debt or of refusing continuing freedom to remit profits and repatriate capital.
- The threat to diversify monetary reserves, to shift them across frontiers or to demand conversion into gold.
- Alliances with developed country interest groups wishing to import low-cost commodities, such as independent retail chains or importing agencies, combined, possibly, with "dumping".
- The threat to deny overflying rights to airlines. The use of bargaining power based on ability to prohibit the presence or passage of troops.

Other areas might include refusal to cooperate on the control of narcotics or in the prevention of global pollution or refusal of military facilities.

One would hope, however, that areas of cooperation will give more scope than areas of confrontation and conflict. There are ways in which the

talk of international cooperation can be backed by action. One way to give reality to this notion is for the rich countries to encourage exchange of ideas and experience between developing countries. In many respects they can teach one another more than developed countries can in such areas as export promotion of labour-intensive products, family planning programmes, rural development, including smallholder schemes. Countries which have recently undergone an experience are much better equipped to communicate it than countries that had gone through this experience decades or hundreds of years earlier, in quite different demographic, climatic, international and social conditions. This is another important argument for the encouragement and finance by developed countries of "offshore" research on an international or interregional basis.

The upshot of this discussion is that international and "interface" issues are more appropriate items on the Agenda for Research of rich country institutions than purely LDC domestic ones, but that this is subject to a number of qualifications. More particularly, (a) development is a complex process in which international relations cannot be severed from domestic ones; (b) self-restraint will never be universal and if one group of researchers (possibly belonging to the same school of thought) occupies itself with these issues, scholarship requires that their work be subject to the criticism of others in the profession; (c) focusing interest in high-prestige centres of learning (the USA, Europe) on purely LDC domestic issues may correct the internal brain drain (the diversion of efforts to irrelevant prestige topics) and stimulate indigenous, relevant research in the developing countries; and (d) the universality of scholarship draws no political frontiers, though the existence of such frontiers may impede the work and colour the outlook of scholars. On the other hand, work on international issues may be as suspect as work on domestic issues, precisely because the national self-interest of rich countries is involved. In spite of these qualifications, I would conclude that the emphasis of rich country research in rich countries should be on "interface" issues.

#### VII. MONEY: THE ROOT OF ALL GOOD

Quoting proverbs is like reading tea leaves: you can always find what you are looking for. Proverbial wisdom is wise because it hedges its bets. It is irrefutable, because it is selfcontradictory. "Many hands make light work" seems to offer advice to a personnel catering manager wishing to determine the number of staff he should recruit. But proverbial wisdom protects itself by coupling with the proverb its antidote: "Many cooks spoil the broth". Similarly, the impecunious scholar turning his troubled conscience to sources of finance finds ambiguous advice from "pecunia non olet" and "he who pays the piper calls the tune". In principle, it should make no difference where research money comes from, as long as no intellectual, moral or political conditions are attached to the methods and conclusions of the work. In practice, what matters is not only freedom from ties but also whether outsiders, on whose cooperation the work depends, see it in this light. However self-effacing the CIA or the US Defense Department or, indeed any American source may be, many Latin Americans regard all US money as tainted. Yet, the rational reaction to suspect sources is not to reject them but to demand full and open disclosure of the purpose, methods and origins.

At one time, it was possible to distinguish between public and private (charitable) sources of finance. Public sources clearly insisted on, or were thought to insist on, returns on their money, which meant support for their policies. Private charitable sources were regarded as more disinterested, more prepared to support genuinely independent research. This distinction, if it existed, broke down with the revelation of the various private foundations that the CIA had used to channel funds for its purposes. On the other hand, government departments may genuinely divest themselves by channelling their funds through independent bodies, such as the University Grants Committee in Britain or the various Science Research Councils, so that independence is safeguarded. For reasons such as these, the public-private distinction does not coincide with that between heteronomy and autonomy.

Another distinction may be drawn between national and international sources. National sources might be thought to be concerned with promoting the interests of the nation, international sources those of the world community. But international organizations have their own partial and vested interests and it is doubtful whether they are inclined to support more independent research than some national bodies.

More important, in my view, than the source or channel of funds, viz, whether they come from public or private bodies, from national or international ones, or whether they are channelled directly or indirectly (through intermediate bodies), is the question whether they are highly concentrated or whether there is genuine, not just legal or formal, dispersal. There are obvious advantages in concentration. Wasteful duplication of effort and overlaps can be avoided; attention can be drawn to gaps in our knowledge; data, concepts and methods can be standardised. But there are also dangers in concentration. The principal danger of heavy concentration and centralisation of research funds is the diversity, originality, criticism and heterodoxy are liable to be discouraged in favour of a uniform and possibly premature orthodoxy or a swinging with fads and fashions. The pressures of the professionalisation of a subject in the direction of conformity to the standards evolved by the profession are, in any case, powerful. If they are further strengthened by a single grant-dispensing Research Council, the chances for critical, independent work are weakened.

Against the obvious benefits of concentration and centralisation must be set the less obvious losses resulting from weaker questioning of the prevailing orthodoxies. The safeguard of original, independent, critical research must therefore be sought in a multiplication of channels of funds between which the applicant has a choice and an encouragement of small, independent, uninhibited scholars. An additional safeguard is not to entrust the decisions solely to established academics working in the same field as the applicant. Academics from other fields, officials, politicians and other laymen may be useful members of grant awarding bodies and appointments committees. They can help to break through the crust of conformity. Multiplication of channels in turn calls for a single place where these sources are listed, so that potential applicants are fully aware of the opportunities.

Should research be commissioned by grant-giving, policy-making bodies, according to their needs and priorities as they see them, or should

the initiative come from the researchers? The arguments are by now well-rehearsed. On the one hand, the flicker of original, first-class research is such a rare thing and depends so much on individuality and motivation, that any spark that is stuck should be carefully fanned. Interference that imposes conditions or makes requests, may extinguish these precarious and precious flames. Again, innovation and originality do not usually thrive under the strait-jacket of officially commissioned projects. Donors are preoccupied with the urgent problems of the day and the near future; researchers are better at raising more fundamental issues of greater importance in the more distant future. Further, useful and applicable knowledge often grows unexpectedly out of what may at first appear basic and useless knowledge. For these reasons the purposes of policy are often best served by leaving scholars free to identify the problems and set about solving them in their own way. At least there should be an area in which such free pursuit is guaranteed.

On the other hand, problems of action are urgent and data and analysis are needed for informed policy. Unless the donor and client (the policy-making department) can specify what he requires from the contractor, there will be waste, gaps and a failure to focus on what is usable.

This dilemma can be overstated. There is no need to opt between a clearly specified take-it-or-leave-it attitude when the customer lays down clear specifications, and complete laissez-faire. If universities and research institutes are represented on the grant-giving bodies by men and women who understand the needs and attitudes of scholars and if the needs of the policy-making bodies are spelled out clearly, the gap can be greatly reduced. Many good, competent scholars and even more graduate students look for appropriate topics to work on and if Ministries, Councils, International Agencies or Foundations make it known on what topics.

They require more information and analysis, many are eager to respond. Letting priorities be known and continuing a dialogue between policy-makers and researchers, perhaps on joint committees, will greatly contribute to bridging the gap between the needs of policy-makers and the incentives and temperaments of independent scholars.

#### VIII. SCIENCE AND CRYPTO SCIENCE

If the social sciences are a "soft" technology compared with the "hard" technology of the natural sciences, development studies have been regarded as the soft underbelly of "economic science". I have heard it equated to Economics minus Logic. In the attempt to emulate the colleagues practising "hard" economics, mathematical methods have sometimes been brought to bear on issues for which they were not appropriate.

In his Romanes Lectures, Sir Isaiah Berlin illustrates how what was once revolutionary doctrine has become Establishment doctrine, by Turgenev's Fathers and Children.

"The victorious advance of quantitative methods, belief in the organisation of human lives by technological organisation, reliance on nothing but calculation of utilitarian consequences in evaluating policies that affect vast numbers of human beings, this is Bazarov, not the Kirsanovs. The triumphs of the moral arithmetic of cost effectiveness which liberates decent men from qualms, because they no longer think of the entities to which they apply their scientific computations as human beings ... this today is rather more typical of the establishment than of the opposition."

Growing concern with social objectives: employment, equality, the environment, has led to calls for the "dethronement of GNP" which (erroneously) has been regarded as an economic objective. But if there was a fault in the preoccupation with GNP, it was excessive attention to a simple quantitative index, irrespective of the valuation implicit in its sets of weights, i.e. of its composition, distribution and the manner in which it was produced. We are in danger of repeating this very same fault in attempting to extract simple indexes for the social objectives. The proportion of the GNP earned by the bottom 40 percent, or the Gini coefficient are just as inadequate and, by themselves, misleading measures of what we are getting at when we try to reduce inequality as GNP is an inadequate measure of productive capacity. Inequality of income distribution touches only a small portion of the vast, multidimensional problem of inequality. There is inequality of ownership of assets, of access to earning opportunities, of satisfaction from work, of recognition, of ability to enjoy consumptions, of power, of participation in decision-making. (cf. P.30 fn 1) The call for greater equality, for a genuine community of equals cannot be answered simply by measures that reduce the Gini coefficient or any other simple measure of inequality. It is possible to envisage a technocratic society, where decisions are highly centralised and in which a few enjoy the satisfaction from power and creativity while the many carry out boring and disagreeable tasks in a hierarchic structure and in which the Gini coefficient was zero.

The danger of social science research that attempts to emulate the "hard" sciences is that it focuses on the measurable and neglects the rest. Some of the most important obstacles to the eradication of poverty and the promotion of greater equality lie in areas in which measurement is still very difficult or perhaps impossible. Amongst these are the following:

- (i) Unwillingness of governments to grasp the political nettles: land reform, taxation, labour mobilisation widening access to education
- (ii) Linked with this, elitism, nepotism, corruption.
- (iii) Behind these again, various forms of oligopoly and monopoly power: the power of large landowners, of big industrialists, of the multinational enterprises.
- (iv) In a different field, but sometimes equally disruptive, the power of organised labour unions and the obstacles to an incomes

and employment policy. In some countries, the government could be described, not as the "executive committee of the bourgeoisie" but as the executive committee of the trade unions.

- (v) Restricted access to educational opportunities and the imbalances in education and the resulting job certification that both reflect and reinforce the unequal structure of power and wealth.
- (vi) Weak entrepreneurship and defective management and administration: of public sector enterprises, of private firms and of the civil service.
- (vii) Lack of coordination between Central Plans and Ministries, Central Plans and regional, local and project plans; too many countries are long on planning, short on administration.
- (viii) The weakness of structure, area of competence, recruitment, training and administration of the U.N. Agencies charged with development combined, sometimes, with a narrowly technocratic approach, encouraged by the origin and organisation of these Agencies and their politically "non-controversial" approach.
- (ix) There are, of course, also the terrible facts of mass slaughter of ethnic minorities (often entrepreneurial and therefore hated) and political opponents, imprisonment without trial, torture, expulsion, the large sums spent on armies and the police and other horrors.

The list is not exhaustive but merely illustrative. But it shows that the temptation to select the quantified and the quantifiable at the expense of other, possibly more important, areas should be resisted. The important question is: What are the springs of development? Many would stress the importance of entrepreneurial and managerial motivation, attitudes and education. We do not know what characteristics make for the social selection of an innovating, entrepreneurial group. Neither innate characteristics nor education nor religion can explain why some societies at certain periods are better and quicker at innovating than others at other times. Innate characteristics are distributed according to normal distribution curves; the level of scientific education is quite high in many societies in which innovation is poor and vice versa; and all kinds of religion have proved to be consistent with innovation. What we need is an explanation of why, with the right education, innate characteristics and religion, the ablest and fittest, the "Best and the Brightest", the creative innovators, are not attracted to business but, instead, to politics, universities or the civil service.

Economists have chased for hundreds of years the sources of economic growth and development. Land and natural resources (the Physiocrats), labour (John Locke and the classical economists), capital (Marx, Harrod, Domar), education (T W Schultz, Gary Becker), achievement motivation (David McClelland) and most recently research and development have, in turn, been scrutinised and, when examined in detail, found wanting. The experts looking at the facts tell us in each case that the factor they investigated

is not very important for development. At the end of the day (and of this paper) we must confess that we do not know what causes development and therefore lack a clear Agenda for Research. But we must try to resist the temptation to behave like the drunk who has lost his key and looks for it not where he dropped it but under the street lamp -- because this is where the light is.

#### APPENDIX

Review of The Rise and Fall of Project Camelot, Irving Louis Horowitz, MIT Press 117s (paperback 21s), from New Society, 11 January 1968.

In December 1964 the American Army and the Department of Defense launched Project Camelot. It proved far the largest grant ever handed out for research in the social sciences: \$4 to \$7 million for three to four years and, as this was to be only a feasibility study, with expenditure of perhaps \$50 million to follow. A number of international scholars received a letter telling them that "Project Camelot is a study whose objective is to determine the feasibility of developing a general social systems model which would make it possible to predict and influence politically significant aspects of social change in the developing nations of the world."

What interested the army, translated from the tortuous deodorised jargon, was why revolutions happen ("the predisposing conditions and the precipitants of the potential for internal war") and how to prevent or stop them ("counter-insurgent prophylaxis"). Seven months later the exercise was called off, not because it was intellectually ill-conceived and un-scholarly, but because it had caused a row in Chile and threatened to upset United States relations with Latin America. The craft union demarcation (=jurisdiction) dispute between the State and Defense Departments over political and military competence, the politicians' contempt for social studies and the army's doubt about the ability of this type of software to support their hardware, all contributed to the final Presidential veto prohibiting government-sponsored research which, in the opinion of the Secretary of State, would be bad for foreign relations. Even the scholars were less concerned with criticising the design and method of the project than with the degree of military supervision and the method of channelling research funds. Professor Horowitz has assembled in this fascinating volume some of the contributions, academic, political and official, to the debate on the affair and has written a stimulating account and evaluation.

The main methodological faults of the project were the implicit value premises: the identification of revolution and radical social change with social pathology, and of order and stability with social health. Revolutionary movements became "anti-systems activities", indications of "severe

disintegration" and "destabilising processes". Abt Associates, one of the consultants, suggested that techniques developed for reliability and quality control could, by analogy, be applied to social systems: rebellious groups then become failures in a particular component of the system. Professor Horowitz is right in saying that "... the preminence of a 'systems' approach rather than a 'problem' approach led to exaggerated model building techniques that obfuscated rather than clarified major issues."

But the central issue, on which the contributors express a wide variety of views, is this: should social scientists accept money from the government or government agencies in return for work on foreign countries for the government? It seems that at least five distinct questions arise: the source, the terms, the use, the area and the conjuncture.

As to source, some contributors think it is so tainted that the autonomy and integrity of scholarship is threatened by the bargain. This position seems to me to be untenable, partly because all sources are potentially tainted (more about this later) and partly because it remains true that pecunia non olet; he who pays the piper may call the tune, but not the way in which it is played. If the scholar, while disclosing the source of his funds, is left free to pursue truth, there can be no objection to the source. The safeguards of this freedom include the availability of a multiplicity of alternative sources, a high proportion of "untied" grants free from specific contractual obligations, fair chances for individual workers as well as teams. It is neither direct government grants nor government requests for specific pieces of research that constitute threats to academic integrity, but the terms on which the money is given. If these permit the scholar to reach the conclusions to which his clearly stated assumptions and the evidence lead him, there can be no objection to the selection of the topic of research by the sponsor. Several contributors argue, to my mind unconvincingly, that agency-financed research is necessarily corrupting.

Professor Horowitz, on the other hand, thinks that the important question of integrity arises from the purpose and ultimate use of the findings. But since any piece of applied knowledge can be put to bad use (for if you know how to improve, you also know how to impede improvement) the rule that the scientist must not be instrumental to abuse, strictly interpreted, implies intellectual abstinence. Horowitz limits his rule to intended use. But unintended, though foreseeable, or even unforeseeable, use may be more objectionable, and the intention may fail.

As to the area, many contributors consider government-sponsored research on and in foreign countries, without the host government's agreement, not only unwise and inexpedient, but ethically and scientifically wrong. Some go further and say that research conducted in foreign countries must be, and must be seen to be, in the service of the host country and its people. But surely the primary obligation is to advance the subject, and is therefore to the international fraternity of scholars, whether inside or outside the host country. The Norwegian Professor Galtung, who was responsible for exposing the project in Chile (there was nothing secret or classified about it), in a searching essay coins the phrase

"scientific colonialism". The Chileans certainly felt that, just as foreign copper companies were mining and remitting profits, so American social scientists were threatening to mine and undermine Chilean society in its most private recesses and to remit the material to the Pentagon. The correct answer to this question was given by Dudley Seers, in a different context. In his article on "Why Visiting Economists Fail" he wrote that "the conscious and public definition of one's role is essential in work overseas". Although this is not always easy, the social scientist must try to make clear in precisely what capacity he is acting.

Finally, and perhaps most difficult, the scholar may have to judge the political conjuncture in which his work is done and published. The American intervention in the Dominican Republic may have made a difference to joining in a study by the American Army of counter insurrection, even if the terms were consistent with freedom and no direct use was intended or possible.

Some of the radical critics of Camelot, such as Professor Blumer, write as if only government contracts pose problems of integrity. As Professor Boguslaw points out in a sensible apologetic, these critics ignore the pressures (of business, of the foundations and of the universities themselves) to bend the scholar's work towards bias, compromise and conformity. It is surely naive to say, as Professor Kalman Silvert does, that "no problem of integrity exists for two polar groups of social scientists: those who work inside governments ... and those ... stay entirely inside the university world ... It is the social scientist working both fields who is in danger of betraying both his masters through the loss of his power of independent analysis". The temptations of betrayal in order to gain an appointment or promotion at a university, or in order to placate courts or boards of governors of academic institutions, are more corrupting than political temptations. On the contrary, government sponsorship and patronage can be a countervailing power against the temptations and pressures of the academic establishment. The contrast, drawn by Silvert, Blumer and others, between the pure groves of Academe and the sullied corridors of power, not only reveals an ideology shared by radicals and reactionaries, but is also false.

This is not to say that there is a harmony of interests or of approach between politicians and administrators on the one hand and scholars on the other. Camelot was an offspring of the army's Special Operations Research Office (SORO), a tail which wags the body of American University to the extent that on the stationery SORO was set in larger type than American University. The director of this organisation, writing, as it were, more in SORO than in anger, makes the point that conflict arises if research has to be justified by a government agency both as immediately useful and as a basic independent. But no contributor to the volume analyses the

the differences between the approach of the scholars and the official, although Professor William Polk in a thoughtful and perceptive essay, discusses related issues. The scholar brings out value premises explicitly, in order to gain clarity and objectivity, the official tends to suppress them in order to get agreement on action; the scholar wishes to sharpen and refine distinctions, the official to reduce and blur them; the scholar is suspicious of "wide agreement", the official of "controversy", when the scholar says the same thing twice, he begins to doubt it; when the official says it twice he begins to believe it; the scholar has time, the official is in a hurry; when the official asks, what shall I do now? the scholar replies, you are trying to change the world; the point is first to understand it. The research proposal of Camelot, obviously drawn up by officials, talked of "orientation" that "is scientific, offering a balanced course between theoretical and empirical work", bringing to bear "all the relevant disciplines and talents. required." Camelot was a beast designed by a committee.

One result of Camelot, as Professor Kalman Silvert says in an interesting contribution, is to accelerate the Latin American desire to diversify academic contacts. Although American social scientists are, by and large, professionally better, the American Department of State and the American foundations have responded to this desire, and it is for the Parry centres and others in this country to respond to the opportunity and to learn from past mistakes. Professor Silvert says that Latin America is not only a neglected area of study but also an exceptionally difficult and diverse one. But it would seem that it is a particularly rewarding one for a scholar interested in comparative politics.

Professor Horowitz subtitles his book "Studies in the Relationship between Social Science and Practical Politics". But the affair raises also another interesting question: is more money for social research always better than less (which is not the same as: is more knowledge always better than less?) ? " Most of the men viewed Camelot as a bonafide opportunity to do unrestricted fundamental research with relatively unlimited funds at their disposal." These are described and generally accepted as optimal conditions. Yet there are disadvantages of size. First, abundance discourages discrimination and the weighing of priorities, while financial constraints impose the need for selection. Economy in thought is encouraged by financial economy. Secondly, large sums of money shift the emphasis of research from the construction of theories to the collection and manipulation of data, because original ideas are scarce, while data collection can always be expanded. In spite of a widely held view that social scientists need more data rather than more theories, the opposite is true, particularly in the field of development. Too much money chasing too few ideas debauches the value of intellectual currency. Foundation grants, research councils, computerisation are encouraging the accumulation of data, while reflection upon their significance is neglected.

Thirdly, large sums of money encourage the mounting of projects by teams, with their accompanying strengths and weaknesses (bureaucratic attitudes and mediocrity) and transform scholars into executives, diverting

their talents from doing research to organising and coordinating it. The lone wolf is losing out if only because of the economies of scale of grant administration.

Fourthly, if the handing out of big money is concentrated in one body, however independent and fair-minded, the "premature crystallisation of spurious orthodoxies" (in the memorable words of the Clapham Committee) can be encouraged. The sums disbursed by our own Social Science Research Council (which spent last year on all forms of research in the social sciences about the same as feasibility Project Camelot budgeted for one year, though this in turn is trifling compared with the \$80 million spent on military hardware) have not yet reached a scale to raise these questions, but the time will come when they do. On a longer view, the threat to learning from an independent, grant-channelling large monopoly is greater than that from agencies of a democratically elected government.



Social Science Research on Development  
and Research Institutes in Africa

by

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## INTRODUCTION

The purpose of this paper is to raise some issues relating to the organisation and state of social science research on development in Africa. No attempt is made to provide a detailed description of the numerous institutes and departments engaged in this task nor of their research programmes and priorities. <sup>1/</sup> I have tried instead to focus on the institutional framework for conducting research on development with particular attention to some structural aspects of social science research institutes. There is also a discussion of the main trends in research, new directions and priorities that seem appropriate at this juncture, the impact of research on policy, staffing problems and relationships with foreign scholars and agencies. In the final section of the paper, the attempt is made to illustrate some of these general points by referring to the rural development research conducted in recent years at one of the African research institutes.

### I. THE INSTITUTIONAL FRAMEWORK

The main centres of social science research in Africa are the university departments, research institutes and government ministries. The great majority of the social scientists are located in the teaching departments, and hence the bulk of the research goes on there with the exception of a few countries where strong research institutes have emerged. The main function of the departments is regarded as the provision of instruction to undergraduate students. Consequently, there are relatively few teaching departments which have developed strong research programmes and policies. Typically, research is assigned a minor role, is of an ad hoc nature reflecting the individual interests of staff members and, therefore, lacks central focus and priorities. Furthermore, only part of the research effort is concerned specifically with issues of development.

In recent years, most African countries have established planning agencies, and in several of them planning units have been set up in the major operating ministries. However, they are typically short staffed and

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<sup>1/</sup> Readers interested in this can find partial information in the following publications: Directory of Development Research and Training Institutes in Africa (Development Centre of the Organisation for Economic Cooperation and Development, Paris, 1972); C.O.D.E.S.R.I.A., Basic Information and Directory of Corresponding Social Science Institutions in Africa (Council for the Development of Economic and Social Research in Africa, Dakar, 1973); an earlier publication from the same Council, Inventory of Research Projects in African Research Institutes (1970); and International African Institute, Current Africanist Research, (1970-1972). In addition, there are several publications which contain information on development research in individual African countries or institutions and on specific topics such as rural development, employment, health and family planning.

the bulk of their effort is taken up with the construction of development plans, formulation of policies, review and appraisal of major projects, and coordination with other ministries. Only in exceptional cases can members of planning agencies and units engage in basic research.

Full-time centres of social science research have sprung up only in recent years. Apart from a handful of institutes such as the East African Institute of Social Research (now Makerere Institute of Social Research), the West African Institute of Social and Economic Research (currently known as the Nigerian Institute of Social and Economic Research) and the Rhodes Livingstone Institute (now known as the Institute for African Studies), which date back to the late forties and early fifties, the great majority of the research institutes were established in the sixties. The research emphasis in the institutes created during the colonial period was primarily on anthropological and cultural studies.

The research centres established in the post-independence period display considerable diversity with respect to geographical coverage, structural arrangements, areas of research and disciplinary focus. While most research institutes have a national scope, a few centres such as the African Institute for Economic Development and Planning (IDEP), African Training and Research Centre in Administration and Development (CAFRAD), the Panafrican Institute for Development, and the Economic Commission for Africa (ECA) have research and training responsibilities covering the entire continent. In addition, there are a few regional institutions whose activities cover a number of contiguous countries. Example of such institutions are the Institut National de Statistiques et d'Economie Appliquée in Morocco, and the Institute for Statistics and Applied Economics at Makerere University in Uganda, which are both largely funded by UNDP and provide training in statistics and applied economics to nationals from different regions of Africa.

There should be a natural division of labour among the continental, regional and national centres of research and training. Given the demands from government and involvement of researchers with development problems of their own countries, it is inevitable that the bulk of the resources of the national research institutes will go towards the analysis of national development problems. The regional and continental research institutions should ideally concentrate on issues which cut across the national boundaries such as migrations across national boundaries, regional integration and cooperation, trade, foreign investment, aid and multinational corporations. They should also be in a position to promote comparative studies of particular aspects of development strategies and policies either by themselves or in collaboration with national centres of research.

The supranational institutions of research and training have done some useful work in assembling data and providing general surveys on the structure, growth and development problems of African economies. They have also been active in organising training workshops and seminars on selected problems of

development. But so far they have made relatively little original contribution to our understanding of the nature of African underdevelopment and of appropriate development strategies. Such institutions operate under well-known constraints and linguistic divisions, and the heritage of different colonial traditions and practices has further intensified difficulties. The result has been that in contrast to the situation in Latin America, relatively little original work has emerged from these institutions. Nor has the network of regional and continental associations linking various research and training institutions developed to a point where it is making a major contribution to the emergence of a distinctive African scholarship on national and international development problems or to the analysis and synthesis of diverse approaches and experiments being carried out in various fields of development. Nevertheless, some hopeful changes have taken place in the last few years, including the revitalisation of IDEP, the strengthening of C.O.D.E.S.R.I.A. and the emergence of new, well-organised associations like the African Association for Public Administration and Management. It is likely, therefore, that in the coming years greater progress will be made in areas where supranational institutions have a distinctive role to play.

## II. STRUCTURAL ARRANGEMENTS

As far as structural arrangements are concerned, most institutes fall within one or the other of three categories. Firstly, there are the continental and regional institutions, mostly funded by the United Nations Agencies, Continental organisations such as the ECA, C.A.F.R.A.D, and IDEP operate as independent entities within the framework of the policy guidelines laid down by their governing councils which are composed of representatives of member states. On the other hand, regional centres of training and research such as the Institutes of Statistics and Applied Economics in Uganda and Morocco may be linked with the university in the host country as in the former case or with the relevant ministry as in the latter.

The second category comprises the great majority of the research institutes in Africa which are linked with national universities. However, the pattern of their relationship displays considerable diversity. In the first place, there are several centres such as the Makerere Institute of Social Research which perform a coordinating role for social science research in the university, and their main function is to provide various research facilities for departmentally-based staff members. A similar role is played by centres which may have a small core staff of their own, such as the Human Resources Unit at the University of Lagos or the Centre for Population Studies at the University of Nairobi, but which rely heavily on teaching members with relevant research interests. At the other extreme are a few university-based institutes which interact relatively little with the social science departments. In between these extremes are centres like the Institute for Development Studies of the University of Nairobi, the Economic Research Bureau of the University of Dar es Salaam, the Institute of Development Research, Haile Selassie University, and the Bureau of Rural Development Studies at the University of Zambia, which have full-time directors and their own administrative and research staff, but which at the same time collaborate

with the social science departments through teaching contributions, research support for departmental members working on development problems and organisation of seminars, workshops and conferences. Furthermore, the status of research institutes vis-a-vis the university tends to vary a good deal. While many research institutes are treated like other departments and faculties, a few like the Nigerian Institute of Social and Economic Research have looser links with the host universities.

The third type of structural arrangement is typified by the National Institute of Planning in Cairo and the Association Algerienne Pour La Recherche Demographique, Economique et Sociale in Algeria, which are set up by the planning agencies in their respective countries. They obviously have a close relationship with the parent ministry but enjoy varying degrees of independence in the choice of research topics, recruitment of staff and other aspects of their activities.

### III. DISCIPLINARY AND AREA SPECIALISATION

The general pattern in Africa is for institutes to specialise either in terms of disciplines or research areas. There are, for instance, a number of institutions concerned with research and training in public administration such as the Ecole National d'Administration in Ivory Coast, Institute of Administration at Ahmadu Bello University in Nigeria, and the Institute of Public Administration in Sudan. There are others, such as the Rural Development Studies Bureau in Zambia and the Institute of Development Research at the Haile Selassie University, which are concerned mainly with research on problems of rural development. New centres are springing up for research and training in population and manpower fields such as the Human Resources Unit of the University of Lagos and the Centre for Population Studies at the University of Nairobi. The Bureau of Resource Assessment and Land Use Planning at the University of Dar es Salaam is concerned primarily with questions of regional and physical planning and land uses.

Several research centres are specialised in terms of disciplines. The Economic Research Bureau of the University of Dar es Salaam, the Centre d'Etudes Economiques at the University of Tananarive and the Nigerian Institute of Social and Economic Research are concerned largely, if not exclusively, with economic research.

It is exceptional to find centres like the Institute for Development Studies of the University of Nairobi and the Institute of National Planning in Cairo which embrace the entire gamut of socio-economic development research and a wide range of social science disciplines.

There is a common tendency in both developed and developing countries for development research institutes to consist predominantly of economists. This has some obvious advantages: training in economics is required for an analysis of most development problems; in a situation of scarce funds for research it makes good sense to aim for a minimum critical mass of specialists

in a single discipline speaking the same "language" and working with similar tools of analysis; and concentration in one discipline also avoids many of the unnecessary clashes and misunderstandings from which multidisciplinary institutions suffer.

The weaknesses of a research institute based solely on economics are also obvious. Analysis of development problems cast exclusively in economic terms can be seriously deficient. It typically neglects such crucial issues as the values, social customs and traditions which motivate and influence people's behaviour, the role played by institutions, the administrative and bureaucratic constraints, and above all the impact of different pressure groups and economic interests in shaping socio-economic policy. Neglect of such factors not only seriously hampers an adequate understanding of a given developmental situation, but may also lead to policy prescriptions which are doomed to failure. The realisation of these problems is behind the rapid growth in Eastern Africa of multidisciplinary institutes and a problem-rather than discipline-oriented approach to the study of development. The pattern in East Africa is now to involve interested specialists from a wide range of disciplines in research projects, evaluation teams, working parties, seminars and conferences. This has undoubtedly contributed to a better understanding of development problems and to more sophisticated approaches to their solution.

#### IV. RESEARCH ORIENTATION AND PRIORITIES

Social science research on any significant scale is of relatively recent origin in most of sub-Saharan Africa, often not going back more than ten to fifteen years. Yet during this short period, there has been a remarkable expansion in the range and volume of studies that have been undertaken on the societies, politics and economics of African countries. For a variety of reasons, social science research on development has expanded at an even faster rate. Because of the numerous studies which have been carried out in scores of social science departments and research institutes in Africa, it is difficult to make any generalisations on the trends and areas of emphasis in development research. Nevertheless, it is possible to identify certain major problem areas which have engaged the attention of social scientists in recent years. Among these may be listed rural development, education, industrialisation, regional integration and in the last few years employment, migration and population.

The majority of research projects in African research institutes are concerned in one way or another with issues of rural development. This reflects the overwhelming importance of rural population and activities in most African economies. It is also a response to the shifting emphasis in favour of rural areas in the development policies of African governments. Rural development research has attracted specialists from all disciplines and has been focussed on a wide range of issues. Prominent among these have been studies on individual crops, marketing and pricing policies, land tenure, agricultural extension and credit, cooperatives and diffusion of innovations. Recently, increasing attention has been paid to non-agricultural rural activities and strategies for integrated rural development.

Education has played a critical role in the evolution of modern African societies. It has thus naturally attracted an exceptional amount of interest from scholars in a wide range of disciplines. The political scientists and sociologists have concentrated on the role of education in class and elite formation and in shaping attitudes on a broad range of socio-political issues. The educationists have been concerned with the internal workings of the educational system, including teacher training, methods of instruction, curriculum and examinations. The economists have attempted to evaluate the costs and benefits of investment in education and its role in meeting the manpower requirements of the economy. With the massive expansion of education over the past decade, the interest has shifted to the question of employment prospects for school-leavers and to the broader issue of the role of education in socio-economic development.

In the field of industrialisation, the emphasis has been on studies of individual industries, the structure and pattern of industrial growth and strategies for industrialisation including import substitution, protection policies and export promotion. Closely related to this is the interest in regional integration. Because of the small size of most African countries and a great variety of interterritorial cooperative arrangements devised during the colonial period, regional integration has provoked a great deal of interest among economists and political scientists. Research has focussed on the mechanics, obstacles and costs and benefits of economic integration.

Migration from rural to urban areas within a country and across national borders has been a notable phenomenon of modern African development. At the same time, the problems of urban and educated unemployment have become a major political issue in many countries, and the international concern with rapid population growth has also been reflected at the national level. As a result of these factors, there has been a rapid expansion of research on the problems of employment, of rural-urban migration and rapid population growth.

Although there has been a big increase in research on development problems in the past decade or so, there are a number of important areas which have either been wholly neglected or only insufficiently explored. Perhaps, the most notable instance of this is research on international economic problems. Africa is probably more deeply involved in the international economy than other parts of the Third World. Foreign trade plays an overwhelmingly important role in the structure and growth of African economies. Foreign investment and foreign ownership of assets in Africa are very extensive. Flows of capital and technical assistance are proportionately greater than in other developing regions. Yet for all that, there have been remarkably few studies on the role of foreign investment, aid and trade in African countries. Perhaps, the single most important issue in most Eastern, Central and Southern African countries in the first decade of independence has been the need for accelerated localisation of all sectors of the economy. Yet few scholars, local or foreign, have felt sufficiently interested to explore the implications of various strategies and policies for Africanisation of economies. In addition, there are very few good studies on the impact of foreign investment and the role of multinational corporations.

Another area which has been generally neglected concerns the relationship between different patterns of development and the distribution of economic welfare and participation among the people. This is all the more surprising when one considers the exciting possibilities for research offered by the wide variety of approaches to development being tried out in different African countries. Although in recent years attention has shifted to the equity aspects of government policies in particular areas, few studies address themselves to the broader issue of development patterns and social and economic justice. Closely related to this is the general neglect by researchers of marginal areas and countries. Large numbers of people live in areas of low potential in semi-arid regions, often leading a nomadic life and largely bypassed by the development that has taken place in the last decade, but it is difficult to find any serious studies which deal systematically with the development possibilities in such regions.

Another area which deserves more than the scant attention it has so far received relates to decision-making processes in the public sector. A great deal of work is going on in the field of development administration, but the focus is on procedural aspects of public decision-making. Since the governments play such a vital role in social and economic development, the need is for in-depth studies of the various forces and pressures which interact to influence or determine government policies.

The preceding discussion has been concerned with development problems which have been emphasised or neglected by contemporary social science research in African universities and research centers. In concluding this section, a few remarks may be made about the desirability of certain changes in the general orientation and approach to research on development problems. In the first place, there is a need to move away from partial and fragmented research to more integrative and synthesising studies which look at the society and the economy as a whole. The post independence years have been characterised by major changes in the policy of African societies. The process of modernisation is bringing about fundamental changes in social and economic structure, and a wide variety of approaches to development are being tried out in different countries. All this offers a unique opportunity to social scientists for analytical and interpretative studies of the transformation process and of the new mould in which African societies and economies are being cast. A similar phase in the history of the presently industrialised countries produced a series of classic works which succeeded in capturing the dynamic forces and delineating the shape of the emerging societies. Few if any such great works have appeared in Africa, or Asia and Latin America for that matter.

Secondly, for a realistic analysis of the nature of African development problems, it is necessary to some extent to liberate oneself from methodologies and theories worked out in the context of the industrialised countries. There is a need in all social science disciplines to develop new concepts, analytical categories and models to fit the situation prevailing in developing countries. In the same way, the data and statistical systems inherited from the developed countries need to be adapted to the conditions of African economies in order to serve the objectives of development policy. All this can, however, only come

about if the social scientists have an intimate understanding of the institutions, traditions and values of people in developing countries.

Finally, for the greater effectiveness of policy-oriented research it is necessary to go beyond the technical analysis of a given development problem. Special attention needs to be paid to the political and administrative feasibility of the array of policy instruments available, as well as to an analysis of the various groups and classes affected by the proposed changes. Nor can policy-oriented research fail to give consideration to the ways and means of mobilising support from the groups that stand to benefit from proposed changes and of reducing or neutralising the opposition of adversely affected vested interests.

#### V. IMPACT OF RESEARCH ON DEVELOPMENT POLICY

The ultimate objective of research on development is presumably to influence the development strategies and policies pursued by a country. This is true of research focussing on fundamental development problems as well as research addressed to immediate policy issues. There are many and complex reasons why the results of a good deal of research are never reflected in development policies. There is not much that can be done about some of these problems, but there are others which are simply due to shortcomings in research design or dissemination of research findings. It is these which are discussed here.

It is possible to identify the main obstacles which stand in the way of more effective utilisation of the research carried out in African universities and institutes. In many countries, the relations between the government and certain sections of the university and especially the social science departments are characterised by mutual suspicion often bordering on hostility. This naturally impedes the emergence of a close working relationship which is a prerequisite for effective use of the academic research capability. A situation of this sort prevailed in Uganda in the late sixties which effectively prevented Makerere Institute of Social Research from making any contribution to the formulation and analysis of development policy. Likewise in recent years, there have been periods of severe strain between the government and sections of the university in Ethiopia and Sudan.

On the other hand, where the research institutes and governments have been able to establish a relationship of mutual trust and confidence as in Tanzania and Kenya, the research programme has tended to focus on national development priorities and has had an impact on some key aspects of development policy.

An important factor governing the use made of research findings is the presence of receptive and trained personnel in influential positions in the government machinery. Without this, it is very difficult to initiate any dialogue on development strategy and policies let alone use the research expertise to investigate these issues. In the governments of some African countries, the absence of a group of individuals of this sort is the main reason for the failure to harness academic expertise to the cause of development. In several countries in Eastern, Central, Southern and Francophone Africa, a good deal of

development research is still carried out by expatriate researchers and transmitted to the government by expatriate advisors. This can be fairly effective over limited periods as demonstrated by the Kenyan and Tanzanian experience in the sixties, but quite clearly it cannot provide an answer to the basic problem of creating an indigenous capacity for executing and utilising research.

Even in countries with a substantial national expertise in the government and the universities, difficulties are experienced in formulating policy-oriented research programmes and devising efficient means for utilising research findings in policy formulation. Too often research is not focussed sharply on policy issues and hence the findings tend to be too general to be useful to policy makers. Furthermore, the great majority of development research pays virtually no attention to the strategy for implementation of research findings including the political and administrative aspects of various policy instruments. Finally, the implementation of research findings often requires the researcher's continuing involvement in a variety of ways, and it is only in exceptional cases that this is forthcoming.

On the government side, only a handful of top civil servants are aware of the ways in which research can contribute to better understanding and solution of the development problems of a country. A number of research institutes have made determined efforts to involve government officials in seminars, workshops and conferences, but these have been only partially successful. It is only exceptional officials who have the inclination or find time to read research papers and comment on them. The technical jargon used in these papers constitutes an additional obstacle.

In the light of these difficulties, what can be done to ensure a wider diffusion and more effective utilisation of research findings in development planning and policies? A full discussion of this important question is not possible here but a few ideas may be mentioned.<sup>1/</sup> The first requirement is for relevant and policy-oriented research along the lines mentioned in the preceding pages. This will in many cases call for a multidisciplinary effort with due consideration given to the institutional, political and sociological environment of a given development situation. The policy dimensions of research must receive greater attention in the design of research projects. To the greatest extent possible, an early and continuing involvement of relevant officials in research design and execution should be achieved in order to ensure sympathetic and receptive treatment.

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<sup>1/</sup> The issues involved in the establishment of an efficient system for determining research priorities and for effective storage, dissemination and utilisation of research findings were discussed at two national seminars organised by the Institute for Development Studies, University of Nairobi. The proceedings of these seminars have been published in *Harnessing Research for Production, Dissemination and Utilisation*, Occasional Paper No. 5, Institute for Development Studies, University of Nairobi, 1972; and *In Search of a System for the Dissemination of Research Findings and Technology in Kenya*, Occasional Paper No. 7, Institute for Development Studies, University of Nairobi, 1973.

Secondly, some means must be devised for dissemination of research findings to key policy makers in relevant and usable form. This function might be performed by "research brokers." These would be located in ministries and charged with the responsibility for coordinating with research centres. Their functions would include the formulation of research priorities as seen by the government, the monitoring of research, the distillation of key ideas and recommendations emerging from ongoing research and their presentation in a concise and clear manner to the relevant policy-makers. In addition to, or as a partial substitute for, this the research institutes might themselves entrust to one of their staff the function of bringing research findings in suitable form to the attention of policy-makers and the general public. Apart from summaries of main ideas and policy recommendations in clear, non-technical language for dissemination to relevant individuals and organisations, use could be made of mass media such as newspapers, radio, television, etc.<sup>1/</sup>

Thirdly, our experience at the Institute for Development Studies has demonstrated the enormous value of organising national workshops on themes of important policy concern which bring together scholars, policy-makers, field officers and representatives of aid agencies and mass media. This provides a unique opportunity for presenting research findings and discussing research priorities, policy issues and problems of implementation.

#### VI. STAFF DEVELOPMENT

Perhaps the single most important problem facing African research institutes lies in the field of staffing. Since the presence of a core of well qualified and committed local scholars is a sine qua non for the viability and dynamism of a research institute in the long run, it is clear that staff development must receive the highest priority in national and international efforts to strengthen the research capabilities of African countries. In the early years of their establishment, most research institutes, especially those in Francophone, Eastern, and Central African countries, were heavily dependent on expatriate staff. Although a significant degree of Africanisation has taken place in recent years, most research institutes in these countries continue to suffer from a lack of senior local scholars.

A number of factors have contributed to this situation. Firstly, there is an overall scarcity of highly qualified social scientists, particularly economists. Secondly, a number of characteristics of research institutes have made it difficult for them to recruit and retain local scholars.<sup>2/</sup> These include the marginal position of institutes in the university structure, lack of tenure in some institutes as compared with "instant" tenure in teaching

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<sup>1/</sup> The Institute for Development Studies, University of Nairobi, has made a start in this direction by appointing a full-time staff member to carry out these functions.

<sup>2/</sup> For an excellent discussion of this problem, see James S. Coleman, "Some Thoughts on Applied Social Research and Training in African Universities", in Taamuli (Dar es Salaam), 1973.

departments, loss of freedom to pursue the research of one's choice, and the pressure to publish to justify one's position as a more or less full time researcher. In addition, research institutes recruiting staff have to compete with governments and international agencies whose offers are not only more attractive financially but also confer greater power. There are of course some compensating advantages in a research appointment - more time for research, generally better access to funds, research facilities and data - but apparently these are not considered adequate to outweigh the disadvantages noted above. The result is that despite vigorous efforts at localisation of staff, most institutes have failed to build a nucleus of stable staff to provide continuity and leadership.

One of the reasons why past efforts at staff development have not come up to expectations lies in the failure to estimate needs and fashion appropriate programmes at the national level. All too often individual ministries and university departments have attempted to develop their own programmes in isolation from each other. This has resulted in much wastage and has prevented the emergence of a well thought out plan for meeting national needs for highly qualified social scientists. If the African countries are to achieve self-sufficiency in professional manpower by the end of the decade, it is essential to estimate the current shortfalls and the likely future needs of all the main users of social scientists. It is only in the light of such information that adequate training programmes can be established and the external agencies make their effective contribution.

The needs of different countries are sufficiently diverse to call for special programmes tailored to their own requirements. In countries with large and well-established universities, the appropriate approach would be to make a big effort to initiate and strengthen their programmes of graduate studies. Such centres of excellence would in time draw students from other neighbouring countries which might not be in a position to launch graduate training of their own. External assistance is needed for both recruitment of specialist faculty members and for provision of scholarships. For countries which are not in a position to develop graduate programmes, the immediate need is for a great expansion of assistance for training overseas. In this connection, an idea worth exploring is the provision of a limited number of research "internships" with international agencies such as the World Bank and the International Labour Organisation which have established large programmes of research on development problems. The internes, who would normally have completed their graduate course work, would be attached to ongoing research projects and encouraged to engage in independent or joint research under the supervision of senior professionals. After a period of one or two years, they would be expected to return to the universities and research institutes of their countries.

## VII. RELATIONSHIPS WITH EXTERNAL RESEARCH COMMUNITIES AND AGENCIES

In general, African research institutes have depended much more heavily on foreign funds and staff than their counterparts in Asia and Latin America. Foreign scholars have come in three capacities: graduate students to collect material for their doctoral dissertations, faculty members on their sabbatical leaves and academics recruited by the institutes either directly or through the assistance of foundations, bilateral or multilateral agencies. There are some instances where research centres have established institutional links with universities in developed countries such as the arrangement between the Institute for Development Studies in Kenya and the University of Glasgow or between the Institute of Development Research in Ethiopia and the Consortium of Mid-Western Universities in the United States, but these have not been adopted on any large scale.

The extensive expatriate presence in certain research centres in Africa has undoubtedly made some important contributions to their growth in the formative stages. It has also resulted in some very high quality research. But at the same time it has created problems of its own. Some research institutes with heavy expatriate dominance have become vulnerable to attacks from certain quarters and especially from their academic colleagues in other parts of the universities. The overwhelming expatriate influence in some research institutes has also had the unintentional effect of stifling local initiative. Finally, the presence of large numbers of foreign researchers, whether attached to the university or operating on their own, has led in many countries to the introduction or tightening of regulations concerning the procedures for obtaining research clearance.

The leadership of most research institutes has passed into local hands, and the proportion of local staff has increased considerably. Nevertheless, for some years to come, there will continue to be opportunities for constructive contributions by expatriate scholars. However, if their potential contribution to research and institution building is to be fully exploited, a much more selective approach to the recruitment and utilisation of expatriate skills will be necessary than has prevailed so far. This will call for changes on the part of expatriate researchers as well as the host institutions.

There are few research institutes which have developed policies for optimal use of expatriate skills. Consequently, there is considerable waste and frustration on all sides. It is not possible here to go into the full details of the ways in which expatriate contributions to the host country and institute could be maximised, but a few general points may be mentioned. First, the host institute must have a clear idea of the research needs and priorities of the country. These should be determined periodically through an appropriate mechanism after discussions and consultation among the interested individuals and organisations. An indicative research plan of this nature should contain an inventory of completed research, listing of ongoing research and assessment of needs and priorities. A document of this nature will prove extremely valuable not only to new expatriate scholars looking around for useful

research topics, but also for the growing number of local graduate students about to launch on their first research. At the same time, by identifying the existing gaps in knowledge, it can contribute to a more efficient recruitment and utilisation of expatriate skills.

Secondly, given the overriding importance of staff development for African research institutes and universities, this should be considered an essential part of the assignment of senior expatriate scholars. There are several ways in which this may be done such as informal instruction to research assistants, supervision of the research work of junior members of staff, joint research projects and faculty seminars on recent developments in the area of their specialisation.

Thirdly, in order to derive full benefit from the research undertaken by expatriate scholars, the host institution should ensure that preliminary results of their research are discussed with the relevant people prior to their departure, that copies of data and other research material are deposited in the local libraries and the finished product made available to all interested parties. In almost all African countries, there are numerous other opportunities for expatriate researchers to act as advisors and consultants to ministries and private organisations.

Over and above this, there are many other ways senior and experienced scholars can contribute to institution building in the host country. These include involvement in the ongoing activities of the institute, development of new programmes, publications in local journals and so on. Expatriates with such ideas and attitudes will be needed and welcomed for many years to come. They will find their stay in the country a stimulating and enriching experience and will be able to contribute a vital element to the development process. In contrast those expatriates whose sole motive in going to developing countries is professional and financial profit, who come merely to collect material for the books they write on returning to their home universities, will encounter an increasingly hostile atmosphere and leave behind them a legacy of bitterness.

A final point should be made concerning the role of multilateral and bilateral agencies in institution building and the strengthening of research capability in African countries. There was a time when such agencies automatically looked to scholars and institutions in industrialised countries for development research. Consequently large resources were given out to help their research and institution building. In recent years there has been a definite change of attitude in this respect, and many donor agencies are beginning to develop programmes of various types of support for research institutes in developing countries. The Rockefeller Foundation has played a critical role in staff development and institution building in social sciences in East African countries. The Canadian International Development Research Centre has resolutely pursued a policy of channelling most of its resources to research in universities and institutes in developing countries. Nevertheless, the fact remains that even today the instinctive reaction of

most foundations and bilateral and international agencies is to look to experts and institutions in developed countries for consultancies, research contracts and advice. Apart from the fact that this hinders the development of research capability in developing countries, it is questionable, to say the least, whether aid funds should be used to strengthen the capacity in industrialised countries for solving development problems.

It is recognised that many developing countries have limited research capacity and therefore for some years to come it will be necessary in certain fields to rely on the skills and expertise available in industrialised countries. The building up of indigenous capacity to identify, analyse and solve development problems is a long and arduous task, but without it no genuine development can take place. Donor agencies can play an important part in this by greatly increasing their support for research, training and institution building programmes.

#### VIII. RURAL DEVELOPMENT RESEARCH IN KENYA: A CASE STUDY

The preceding discussion has necessarily been somewhat general and sketchy. Some of the issues raised above may be illustrated by a study of the rural development research conducted in recent years in Kenya under the auspices of the Institute for Development Studies. The Institute has devoted the bulk of its resources over the past six to seven years to research on rural development problems. In this effort it has worked closely with social scientists from the teaching departments and with officials in various ministries in the Kenya Government. From the outset, expatriate scholars and external aid agencies have also been closely involved in the development of this programme. A study of this experience can therefore throw some light on most of the issues raised in a general way in the earlier sections.

Research on rural development has ranged over a wide field and has developed in response both to immediate policy issues and to an evolving consensus on new problems and new approaches. It was initiated by a group of economists but soon widened its scope to embrace a multidisciplinary team with specialists in extension, communications, development administration, education and rural sociology. From the outset the research has been sharply focussed on real issues and problems in rural development. The major areas covered over a period of six years have been pricing and marketing policies, institutions, rural planning, strategies for integrated rural development, policies for equitable distribution of incomes and services and evaluation of ongoing programmes, policies and projects.

The initial research studies were focussed on marketing and pricing policies. This was in response to the government's need to simplify and rationalise the extremely complicated system of marketing boards and price fixing for most of the important crops grown in Kenya. The Institute staff carried out a series of studies on the pricing and marketing policies for beef, cereals, fruit and vegetables, dairy products, coffee and cotton. In addition there were studies on agricultural inputs such as fertilisers and agricultural fuel and machinery, as well as on the evolution of the marketing board system.

The second major area covered relates to institutions. Here studies have been carried out on such diverse subjects as cooperatives, changing systems of land tenure, local leadership, village polytechnics and self-help efforts in rural development.

In the latter part of the sixties, as a consequence in part of an international conference held at Kericho in Kenya on education, employment and rural development, both the research and policy interest began to shift towards the need for integrated strategies for balanced rural development. The Institute staff carried out a number of studies relating to this broader conception of rural development. These included studies of the administrative and political structures for integrated rural development including the relation between central and local authorities; decentralised, area-based planning elements of a programme of intensified rural development, including government policies in research, credit and extension and the search for innovations in agricultural practises; possibilities for rural industries; local participation in development and decision-making; and preparation of a control and reporting system for the implementation of rural development programmes.

The fourth area of research concerns equitable rural development. Here a number of studies were initiated on patterns of income and wealth distribution in selected areas, the growth and diversification of employment opportunities in the rural areas and development possibilities in marginal drought-prone areas. In addition, pilot programmes were set up to test the effectiveness of new methods for delivering credit and extension services.

Finally, a number of studies have been concerned specifically with evaluation of ongoing programmes and policies. These have usually been conducted by multidisciplinary teams at the request of government ministries. Apart from the evaluation of specific projects such as the cotton blocks scheme in Mbere, the functional literacy scheme, school feeding scheme and artificial insemination programme, a fifteen-member multidisciplinary team from the University carried out a major evaluation of the entire Special Rural Development Programme as it had evolved up to the end of 1972.

In the execution of its research on rural development problems, the Institute drew heavily upon the expertise available in the teaching departments. Without this collaboration it would not have been possible for the Institute members on their own to undertake all these activities. At the same time right from the outset a close working relationship had been established with the government, and in particular with the Ministries of Finance and Planning and Agriculture. The Kericho conference which gave much of the stimulus to subsequent research on rural development had been planned jointly by the University and the government. University members were closely involved in a series of further studies and activities which finally culminated in the adoption of the Special Rural Development Programme (SRDP). In the initial stages of SRDP, a number of the Institute staff were based in the field as area evaluators. Subsequently, the evaluation work was carried out by centrally based staff. At the same time a series of research papers on various aspects of integrated rural development have had an impact on the evolution of SRDP.

The Institute staff has contributed to the government's efforts in the field of rural development in several other ways. First, they have served on a number of working parties set up by the government, such as the ones on agricultural inputs and extension services. They have also served as consultants to a variety of parliamentary commissions. Institute expertise has been utilised in several seminars and training courses for field staff. Most recently, a University team prepared a Manual for District Level Planning in Kenya at government request.

In general, a close and growing relationship with the government has been established. There have been periods of strain, such as when the Institute and some government members have not seen eye to eye on the precise role the University members should play in the evaluation of SRDP, or when University members have been asked to carry out some assignments which should properly be the responsibility of government officials.

Expatriate scholars and external agencies have played an important role, especially in the initial stages, in the development of the Institute's programme of research and training in rural development. The series of studies on pricing and marketing policies were carried out by members recruited under the Glasgow Scheme which was worked out jointly by the Ministry of Finance and Planning, the University of Nairobi, the Overseas Development Administration in Britain and Glasgow University. In the earlier phase, the multi-disciplinary team recruited for research and evaluation of the SRDP consisted predominantly of expatriates and was financed for the most part by the Rockefeller Foundation. More recently, the Swedish International Development Agency has made a grant to the Institute to enable it to continue its work on rural development problems in general and the SRDP in particular.

While foreign scholars and agencies have played a catalytic and innovative role in the Institute's programme of research on rural development, the expatriate dominance in the early phases gave rise to a number of problems. In particular the personal relationships between some expatriates and Kenyans were not all that might be desired. There were also differences of opinion on the role of the Institute in the SRDP as well as priority areas of research. Many of these problems were overcome as the leadership of the project passed into Kenyan hands and new procedures and structures were devised for development of the programme and for liaison with the government. The expatriate contribution nevertheless continued to be quite substantial.

## IX. CONCLUSION

This paper has attempted to raise a number of issues concerning the problems and growth of social science research institutes in Africa. Building up an indigenous capacity to identify, analyse and solve problems is an essential element of overall development, and this purpose can best be served by a full-time research institute which will draw upon all the available talent in a country for its research and training activities. In view of the complexity of the development process, it is important that a research institute should have a multidisciplinary focus.

Staff development continues to be the most important challenge to the emerging research institutes in Africa. The role of external funds and expatriate scholars was critical in the early phases of the growth of the African research institutes, but in the next phase a much more selective approach to the recruitment and utilisation of expatriate skills will be necessary.

Economic Research in India:  
A Brief Survey of Trends, Gaps and Possible Priorities

by

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## INTRODUCTION

In this paper I present a severely condensed and selective survey of recent economic research in India. The survey covers important branches of empirical research completed with Indian data, and research on major issues of economic policy. Pure theoretic contributions and contributions to the theory of econometric estimation are not included. Statistical issues pertaining to the validity of the estimation procedures used in each kind of empirical work are also bypassed except where it had to be noted that gross estimation defects impaired even the broadest findings of a particular kind of work.

The aim is merely to enumerate the kinds of questions which have been tackled, the analytical techniques adopted, and some of the major empirical results which have a bearing on policy issues. This enumeration clearly reveals the important gaps in the work done as well as its methodological deficiencies. In view of these gaps and deficiencies, some topics on which research may be specially encouraged in the near future are indicated in the concluding section.

The empirical work done in India ranges all the way from simple analyses of official statistics to the econometric estimation of large macroeconomic models. And policy discussion, too, ranges widely from questions relating to the development of particular industries and regions, to the optimal allocation of aggregate national investment over long time horizons. Therefore the selection of contributions mentioned here is illustrative rather than comprehensive; and only the main findings of a few typical studies, dealing with each set of important questions, have been summarized.

The discussion is organized under nine headings: (1) macro-economic relationships, including work on the monetary and banking sectors; (2) international economic relationships; (3) public finance; (4) growth and planning models; (5) population and employment; (6) agricultural economics; (7) industrial economics; (8) regional economic development and planning; and (9) concluding observations and suggestions. Areas of substantial and significant work not covered in this list, due to the limitations of space and time available, are: (1) collection and refinement of economic statistics pertaining to various aspects of the economy, and the construction of national accounts and national income series; (2) the estimation of demand functions and their use for demand projections; and (3) the measurement of poverty and inequality. These fields have recently been surveyed by C. R. Rao (1972), M. Mukherjee (1969), N. Bhattacharya (1973), N. S. Iyengar (1973) and P. Bardhan (1973).

This study has benefited from the earlier survey by Bhagwati and Chakravarti (1969) and the surveys of research work done in India in

various branches of economics recently completed under the auspices of the ICSSR. But, as indicated above, the scope and forms of this paper is more limited. As usual, the responsibility for the selection and interpretation of material rests with the author.<sup>1</sup>

### I. MACROECONOMIC RELATIONSHIPS

In the field of macroeconomics we can distinguish (a) studies in which comprehensive macro models of the Indian economy have been estimated, (b) those in which only the monetary sector has been modelled, (c) studies of price level determination, and (d) descriptive-institutional studies of monetary policy and banking.

Meghnad J. Desai's illuminating survey of macroeconomic models of the Indian economy covers eight studies. (Narasimhan 1956, Choudhary 1963, Krishnamurty 1964, Marwah 1964, Mammen 1967, Krishnamurty and Choudhary 1968, Marwah 1969, Agarwala 1970.)

It may be useful to summarize some of the characteristics of these models.

All these models, except the Agarwala model,<sup>2</sup> are based essentially on the Keynesian theoretical system in the sense that the major equations relate to expenditure categories - consumption, investment, etc., - and the specifications treat national income as primarily demand-determined.

In particular, the investment functions are specified like investment demand functions with familiar variables like income, liquid assets, long-term interest rate, rate of inflation, capital-output ratio, the index of industrial profits and capacity utilization. These specifications are somewhat unrealistic in view of the fact that investment in a developing country is, in the main, "supply constrained" (M. J. Desai, p. 22). Constraints on the growth of investment due to inadequate domestic saving, scarcity of imported or home-produced capital goods and intermediate goods, industrial licensing and other government regulations are obviously important, but they appear to have been ignored. Only in one case (Marwah) the import of capital goods is included as an explanatory variable. And Agarwala alone uses capital issues sanctioned by the government as a variable.

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<sup>1</sup>The author is grateful to A. Vaidyanathan and K. B. K. Rao for critical comments on the first draft.

<sup>2</sup>The Agarwala model is derived from the Lewis tradition of dual-economy models. Thus it specifies non-farm employment as a function of farm output. It is also classical in making the general price level depend simply on an autonomous supply of money.

The consumption functions too are neo-Keynesian - having such variables as income, population, liquid assets, the highest previous consumption, lagged consumption, and, only in one case, the ratio of farm income to non-farm income.

Conventional money-demand (or liquidity) and interest-rate equations are also included in all the models. Demand deposits, for instance, depend on the short-term interest rate, excess reserves, and, in one case, non-agricultural income; the long-term interest rate depends on the short-term rate, the lagged long-term rate, and the stock of government securities. But special features of the money-creation process in a planned economy like India are not reflected in these specifications. It is common knowledge that every five-year plan has a substantial planned deficit to be financed by money-creation. A growing proportion of new bank credit goes to the government. There are serious restrictions on bank lending to particular sectors and classes of borrowers. And some of the interest rates in the organized sector are tightly regulated. But these facts do not seem to have been conceptualized in the models constructed; and even the obvious and most important connection between planned government deficits and the money supply has been overlooked.

Government expenditure and government investment have been treated as exogenous variables in most models. Only two models (Marwah and Mammen) separate private and government investment: in one, government investment depends only on income and lagged investment and in the other it is a function of the difference between target and actual income.

Tax revenue appears in three models only as a function of income. And relations linking government revenues and expenditures, and government deficits with borrowing and money-creation are not specified.

On the supply side, the specifications of inputs in agricultural production functions seem to be somewhat arbitrary. In the Agarwala model farm output is a function of farm capital and rainfall; in the Chaudhri model it is determined by farm employment and rainfall; and in the Krishna-murty model it depends on gross acreage and irrigation ratio. In the Marwah model it is simply a function of national income and lagged farm output.

Finally, as Desai has shown, the price determination equations are not related to quantity determination equations in a logically clear framework.

But in spite of their limitations, these models remain pioneer attempts to model the Indian economy subject to data availability constraints. Some simulations with the models have shown that the models can explain the deviation of the real path of the economy from the planned path. Three of the models, for example, could predict serious shortfalls in the achievement of Third Plan output targets and severe price inflation.

In the field of macro model building, a good start has been made, but further work has to be done (a) to improve the theoretical specifications underlying the econometric equation system and subsystems, and (b) to integrate estimated behavioral models with planning models. (See Section IV on Planning.)

Among econometric studies of the monetary sector alone the important ones are Bhattacharya (1972), G. S. Gupta (1972), Khusro and Siddharthan (1970) and Venkatachalam and Sarma (1972).

Bhattacharya tries to show that money supply is not exogenous but ignores the primary government decision variables and the multiplier mechanism. Gupta's paper is interesting inasmuch as it discusses the money-multiplier process in some detail and distinguishes the quantities which the monetary authorities directly control, and the quantities which get determined as a result of private responses to the levels of these quantities. But it stops short of proper parameter estimation.

The Khusro-Siddharthan paper models the behavior of the major assets and liabilities of the banking sector.

Venkatachalam and Sarma present a neat seven-equation model which simultaneously determines three quantity variables (excess reserves, demand deposits and time deposits) and four interest rates. Only the time deposits show some significant responsiveness to the rate of interest. Although Venkatachalam-Sarma present the causal logic of their model more clearly than others, and are aware of its limitations, the most important component of the money supply - currency with the public - remains exogenous, and bank portfolio variations are not specified in detail.

It is evident again that relationships linking monetary variables with variables pertaining to government financial operations, and restrictions on bank lending and interest rates, need to be conceptualized and tested.

Studies of the structure of interest rates in India are due to Krishnamurty (1965), Kaushik (1969), and Pani (1971). Pani's work is interesting for it analyses the rural rate structure and the rural households' demand for credit.

K. N. Raj (1966) has offered the only serious Keynesian study of price determination in India during 1950-1966. He estimates annual changes in "autonomous" outlay - defined as fixed capital formation plus government consumption plus "active" investment in inventories, plus export earnings. Assuming a fixed MPC (.75), he obtains estimated changes in aggregate demand. Using independent estimates of changes in the "real" supply, he derives the price level changes that would balance aggregate demand and supply each year. The predicted changes in the price level turn out to be very close to the actual. A similar procedure is used to predict foodgrain price movements.

Raj carefully explains that his models "are not intended to prove that changes in the money supply as such do not have any effect on price behavior. They carry only the implication that, since the factors causing 'autonomous' changes in outlay are to a large extent responsible for the observed changes in money supply, and since the effect of interest rates on investment is also incorporated in these 'autonomous' changes, it is not necessary to introduce money supply as an additional variable except for taking into account possible 'real balance' effects on aggregate demand. . ." (Raj 1966, p. 76).

This conclusion has been quoted at length for it illuminates significant lines of causation in the Indian economy and many of its implications could have been fruitfully used by model-builders in specifying their money supply and price formation equations.

A very preliminary attempt has been made by Gupta (1972) to frame a model of monetary "policy" which can yield the effect of controlled variables on target variables. Its results are unreliable due to estimational difficulties. But a few literary discussions of monetary policy are noteworthy: Pendharkar and Narasimham (1966), Raj (n.d.), Basu (1971), and Bhatt (1970). Only Raj has fully analyzed all the ramifications of the fact that monetary policy plays a very limited and subsidiary role in the Indian system.

## II. INTERNATIONAL ECONOMIC RELATIONSHIPS

The work done in this field was surveyed in considerable detail by Bhagwati and Chakravarty (1969). Manmohan Singh (n.d.) and V. R. Panchmukhi (1973) have completed two more recent (unpublished) surveys.

Econometric work on international economics has been focused mainly on the measurement of income and price elasticities of Indian exports and imports. Among studies using post-Independence Indian data, we have M. Dutta's paper (1964) in which both exports and imports during 1951-60 were shown to be nonsignificantly sensitive to relative price variations; but exports turned out to be elastic with respect to world industrial production. Da Costa (1965) found not only Indian exports of 11 major commodities during 1953-62 insensitive to relative prices but also to world real income. But elasticities of substitution between Indian and competing non-Indian exports appeared to be significant.

In another study by Dutta of tea exports to the U.K. however, an equation having three relative price variables and an income variable shows up all the price variables to be significant, but U.K. income to be insignificant. Imports of iron and steel, machinery, chemicals and cotton again turned out to be insensitive to prices (Dutta 1965).

A recent study (1972) by da Costa and Shetty of 9 major exports, using more recent (1959-1969) data threw up very inconclusive results.

These findings are not surprising, considering that fixed public sector targets, import and exchange controls, industrial licensing, and internal commodity controls, permit only a very tenuous relationship between import volumes and import prices. And in the case of exports it is understandable that particular exports are sensitive to relevant relative prices e.g. Indian prices deflated by competitors' prices, but aggregates are not sensitive to aggregate relative price indices.

Bharadwaj has contributed two studies (1962 and 1967) in which he verified the Heckscher-Ohlin hypothesis about the factor-intensity of traded goods: India's exports were found to be more labour-intensive than import replacements, as expected, with as well as without the adjustment of measured labour for skill-intensity. But it is now widely accepted that factor intensity reversals can occur under a variety of conditions.

The poor record of Indian exports in the 1950's was usually explained in terms of demand factors and occasioned pessimistic forecasts of export prospects (Patel 1959). But deeper, commodity-wise analyses have brought out the importance of supply factors in explaining the decline in India's share of trade in many traditional commodities (Singh 1964; Cohen 1964). Similar analyses of export performance in the sixties will be very illuminating but they have yet to appear. Many writers have stressed the need for highly disaggregated studies of export trends and prospects. Tea and jute have been studied in some depth (Gulati 1968, Roy 1965, Venkataraman 1958, and Sandhu 1971). But the recent growth of non-traditional exports still awaits detailed analytical study, though a wealth of descriptive material about individual exports has been generated by the National Council of Applied Economic Research, the Indian Institute of Foreign Trade, and Industrial Development Services.

Good general discussions of export policies include Singh (1964) Joshi (1967) and Bhagwati and Desai (1970). Recently the question of competition between domestic demand export has engaged many researchers (Paul and Mote 1970 and V. L. Rao 1972).

The Chenery decomposition identity has been used by some economists to measure the contribution of import substitution to the growth of industrial output. The percentage contribution turned out to be 22 for all industries, and 42, 13 and 11 for investment, intermediate and consumption goods industries respectively over the period of the first three Plans (1951-1966). (Ahmad 1966, Table V). (Desai's comparable estimates for a shorter period 1951-1963 were published much later (1972)). Panchmukhi, too, has tried to decompose output growth into the contributions of technical change, final demand, and import substitution (Panchmukhi 1972). He has also explored the relationship between import dependence and productive efficiency in three sectors (1969).

Quantitative import controls have been the subject of continuous controversy. Bhagwati has been advocating exchange auctions as more efficient instruments of exchange allocation than quotas at the official rate (1962). And foreign economists have often pleaded for import liberalization. But many Indian economists have explained the reasons why Indian policy remains wedded to quantitative controls. First, there is an evident clash of objective functions. The Indian policymakers aim at the emergence of a specific structure of domestic production and regard progressive self-reliance as an objective of policy separate from the growth of the GNP. Consequently they accept a high-cost regime as the necessary price to be paid for the pursuit of self-reliance. The foreign advisers obviously cannot share this rating of goals and simply recommend that choices between imports and domestic production be made primarily on the basis of relative cheapness. The planners insist that if a specific production structure is desired in the long run the allocation of capital and foreign exchange cannot be left to be determined by the market; it has to be administered. And from their point of view any plea for the abandonment of quantitative import and exchange controls is a concealed plea for the abandonment of planning itself. The second set of considerations which strengthens the belief in quantitative control relates to the seriousness of the balance of payments constraint for an economy which confronts large and growing import requirements, inelastic exports, trade barriers in developed economies and the pressure of domestic demand. The advocates of relaxation do not share this concern. Finally, the growing concern for accelerating domestic employment creation also favours a highly restrictive import regime. These arguments are lucidly reviewed by Lakdawala (1965) and Manmohan Singh (1969).

In a regime of comprehensive quantitative controls, the determination of tariff levels loses much of its importance. But Desai (1969b) has documented and critically examined the criteria used by the tariff commission to determine tariff levels.

Effective protection rates have been computed for 18 industries by Panchmukhi, Bhagwati and Desai (1970) for 1961 and 1962, and Sahota (1968) for 1965-66. These measures only roughly indicate the relative benefits of the tariff accruing to different sectors, because they are very sensitive to the treatment of nontraded goods, and the prices used for computations.

Another subject of continuing controversy is the likely effect of exchange devaluation. Although a large number of economists have participated in the discussion, it is remarkable that no serious study of the quantitative effects of the devaluation of 1966 or of a future devaluation has been made. The reason seems to be that the available theoretical models cannot predict the effects of devaluation in the presence of a comprehensive import and exchange control system. Insofar as planning makes the quantum of imports insensitive to c.i.f. prices theoretically expected effects need not materialize.

The field of foreign aid and investment reveals a similar situation. There are a few descriptive studies of India's experience (Narain and Rao 1963, Kidron 1965) culminating in arguments favouring programme aid, long-term aid, untied aid, soft aid, multilateral aid and loan capital in place of project aid, short-term aid, tied aid, hard, bilateral aid and equity capital. But benefit-cost studies of particular projects with a heavy aid component are hard to come by. One of the few empirical studies of the higher cost of goods imported with tied aid pertains to the chemical industry (Lal 1967).

The debt service problem has also not been studied thoroughly at an empirical level. But there are two interesting models of optimal borrowing by Vijay Kelkar (1970) and Bardhan (1968). And methods of estimating aid requirements have been developed and illustrated by Sengupta (1965), Swamy (1965) and UNCTAD (1968).

India's relations with particular groups of countries have been described in a number of works: Eastern Europe by Datar (1973), Sen (1965), Singh (1964), Chishti (1966) and Narain (1957); EEC by Singh (1964) and Kumar (1966); and Asian countries by Ganguli (1956). Many regional studies of India's trade have also been prepared by the IIFT and the NCAER. And balance of payments fluctuations have been studied by Panchmukhi (1968).

### III. PUBLIC FINANCE

We have already noted that the modelling of governmental financial operations has been neglected by macro-model builders. But even apart from model-building, public finance has remained a weak research field in India.

There are, of course, a few good descriptive texts of the Indian revenue and expenditure system (e.g., Cutt 1969 and Bhargava 1970); a slightly more analytic work by Raja Chelliah (1960); the enormous budgetary material produced by the Central and State governments every year; and the six reports of the constitutional Finance Commissions on the division of divisible revenues between the Central and State governments and non-plan transfers (grants) from the Centre to the States. There is also the so-called Economic Classification of the Budgetary Transactions of the Central and (some) State governments in which government receipts and outlays are regrouped into more meaningful economic categories: consumption and investment outlays and outlays by production sectors. But when we look for serious empirical work based on this mass of factual and legal material we do not find much.

On the revenue side, the elasticities of revenues from different Central and State taxes with respect to national or State income have been estimated by many writers; and changes in the composition of revenue have been noted. (See, for example, Sahota 1966). Representative magnitudes

are summarised in Chelliah (1966). But in spite of an increase in the share of indirect taxes in total tax revenue (from about two thirds to four fifths) very little empirical work has been done on the rationale and the allocative effects of the numerous Central and State commodity taxes. The inter-regional allocative effects of divergent sales tax structures in different States also remain unanalysed. Nor can one mention good empirical studies of the effects of import and export duties on particular sectors. The only significant general study of commodity taxation in India is the recent work by Lakdawala (1972). But one does not find the results of any quantitative work on allocative effects even in this study.

Some work has been done, however, on the distributive effects of taxation. The Central Finance Ministry completed three studies on the Incidence of Indirect Taxation (in 1953-54, 1958-59 and 1963-64) analysing the "burden" of commodity taxes on the consumer budgets of households in different expenditure-size groups, with the NSS expenditure data for rural and urban India. But the results could not be satisfactory indicators of the progressiveness of the tax system because only commodity taxes were covered and the base for "burden" measurement was expenditure and not income. More recently the NCAER has remedied these deficiencies in two studies (1970, 1972) on the formal incidence of all the major Central and State local taxes on each income group in the States of Gujarat and Mysore. The Mysore study showed that the tax system as a whole was regressive as between low and middle income classes; but as between middle and high income classes it was progressive in the urban areas and regressive in the rural areas. The multiplication of such studies may eventually make it possible to evaluate the progressiveness of the tax system in the country as a whole.

Three areas of taxation have received some detailed attention in recent years: the taxation of capital and wealth, agricultural taxation, and corporate taxation.

In the field of agricultural taxation, following Gandhi's pioneering work in his Harvard thesis (1966) in which he measured the burden of taxation on the farm and nonfarm sectors in India, and showed that the former is undertaxed, there has been a proliferation of similar studies (Mathew 1968) particularly those attempting to measure State level burdens on farm and nonfarm sectors (Rao 1966b, V. Vidyanager Seminar Papers 1969, Pathak and Patel 1968). Numerous schemes for reforming India's archaic land revenue and (State-level) agricultural income-tax systems into a progressive and comprehensive direct tax system have been proposed (Raj 1959, Khustro 1963 and 1967, Little 1964, Please 1968, Krishna 1972a) - the latest scheme being the one proposed by the Committee on Taxation of Agricultural Wealth and Income chaired by Prof. K. N. Raj (GOI 1972). But the concept of "intersectoral equity" and Gandhi's particular methodology for proving the existence of intersectoral inequity have been seriously questioned (Krishna 1972a).

The welfare effects of taxing the farm sector have also been the subject of some interesting discussion (Rao 1969 and Srinivasan and Bardhan 1969).

Gandhi (1970) has reviewed previous studies of corporate taxation (Lammas 1966, Lall 1967, and Narayanan 1967), noting the absence of serious empirical work, and cautiously presented his own regression results showing that the Indian corporation tax "falls essentially on capital" and not on the consumers. He has carefully analyzed its effects on capital formation and the debt-equity ratio, and presented a strong case for a value-added tax and against the Bothalingam (1968) proposals for a tax on capital.

Gulati (1957) and Krishnan (1972) have explored the possibilities of reforming the taxation of wealth; and Nanjundaiyya (1970) has published perhaps the only review of urban property taxation and its potential.

Three influential reports proposing comprehensive tax reforms should also be mentioned here: the Kaldor report (1956), the Boothalingam report (1968) and the Wanchoo report (GOI 1971a). The two latter are simply collections of ad hoc reform proposals proposed by judges and administrators without any economic analysis. A large number of official reports of the State-level taxation enquiries also belong to the same category, though they contain a large mass of factual information.

Prospects of raising resources for the five-year plans have always aroused keen interest. Samples of earlier studies can be found in Raj (1959), Gulati (1960) and Little (1964). More recent contributions include Lakdawala (1971) and a sub-group of the Planning Commission's Panel of Economists (GOI 1971b).

In the Approach to the Fifth Plan a financial flow matrix has been used as the format for presenting projected resource flows. But Reserve Bank economists had laid the basis for the flow-of-funds analysis in earlier papers by Bhatt (1959), Divatia (1969) and Pani (n.d.).

Actual resource mobilisation has fallen so far short of expectations in the last two plans that the entire technique of resource (revenue) forecasting and planning deserves thorough research by economists. A critical survey of current techniques is essential; and better methods must be proposed. Chelliah has made some interesting suggestions in his ECAFE paper (1966).

The development of objective criteria for federal transfers to the States has received considerable attention, particularly after the Gadgil formula attempted to combine four criteria (population, per capita income, revenue performance, and essential outlays) into a single objective index of eligibility for plan transfers to States (Lakdawala 1967, Krishna 1969). Non-Plan transfers have been traditionally arbitrary due to constitutional rigidities and the varying views of the Finance Commissions.

Recently, the mounting deficits and debt burdens of most of the States have led to further large discretionary transfers. The whole subject of the principles of federal transfers and State finances - including their severe debt problems - is in need of detailed research.

On the expenditure side, the only way to evaluate the vast and growing public outlays on protective as well as developmental activities is to carry out cost-effectiveness or cost-benefit studies of all important projects and programs. Official Central and State Program Evaluation Organizations have produced descriptive accounts of the operation of a few schemes. Some of the reports of the Central Program Evaluation Organization are particularly valuable, e.g., the studies on Community Development, Minor Irrigation, Soil Conservation, Rural Works, and the High Yielding Varieties Program.

But analytical cost-benefit studies by university scholars are very few indeed: the famous ones being those relating to the Bhakra project (Raj 1960), the Hirakud project (Sovani & Rath 1960), atomic energy power generation (Rosenstein-Rodan 1964), the Durgapur fertiliser project (Sen and Datta-Chaudhuri 1970), minor irrigation projects (Lal 1972), and the programming of the Bhakra project (Minhas-Srinivasan et al 1972).

The first three were reviewed by Bhagwati and Chakravarti (1969). The last three stand out as important landmarks in project analysis. The Minhas-Srinivasan study offers a comprehensive simulation-cum-programming model; the Sen and Chaudhuri study presents several alternative estimates of the NPV of the Durgapur fertiliser project based on different price-sets; and Lal's study is a thorough application of the Little-Mirrlees technique of project evaluation to the problem of choice between irrigation techniques.

It is obviously necessary that such studies of public programs and projects by Universities and independent research bodies multiply, so that in due course we may have an adequate literature on the welfare effects of public spending and the feedback from it may improve the use of public funds in the future.

We cannot however overlook the fact that only a negligible fraction of public spending in India is influenced by benefit-cost analysis of any kind. The current nonplan expenditures are governed by trend projections and/or political pressures and the plan outlays are determined by macro-planning exercises and inter-ministerial and intergovernmental bargaining. The only purpose for which project analysis is and will perhaps be used in the near future is to make yes/no decisions on some individual projects in individual sectors. No fine ranking of many projects is called for because the projects that come up for analytical scrutiny even in any single sector are very few indeed.

We can only hope that the growth of the literature of project analysis will itself create a growing market for such analysis.

In the field of public debt it is difficult to mention any recent study except a very illuminating paper by Rangarajan (1971) pointing out that 85% of public debt is held by the government's own or government-controlled "captive" institutions, and perhaps an active secondary bond market may never emerge in India so long as bond investment levels and their (low) yields are maintained by legislation and administrative action.

#### IV. GROWTH AND PLANNING MODELS

The most important preoccupation of research in planning in India has been the derivation of consistent or optimal sectoral output and investment targets for the successive five-year plans. In the consistency models, the target rate of per capita income growth is exogenously assumed; final and intermediate demands are projected with this assumption; and then, using sectoral capital-output ratios, the allocation of investment is derived.

In the optimising models, on the other hand, some welfare function, e.g., the discounted present value of consumption over the planning horizon is maximized subject to constraints. The objective functions and constraints are, of course, specified in a variety of ways in different models. In particular, terminal conditions have been specified either as given stocks or given post-terminal growth rates. In one case they were determined by the linear growth of sectoral investments. (Sandee 1960).

A number of well-known models of both types have been computed by official and nonofficial Indian economists and foreign economists interested in Indian planning. The important consistency models include the Mahalanobis model<sup>1</sup> (1955), the "Dimensional Hypotheses" paper for the Third Plan (GOI 1958), the Reddaway model (1965), the PPD model (GOI 1964b), the Manne-Rudra model (1965), the Material and Financial Balances exercise (GOI 1966a), and the most recent Chakravarti model (GOI 1973).

The optimizing models include the Sandee model (1960), the Frisch model (1960), the Manne-Weiskopf model (1970), the Eckaus-Parikh model (1968), and the Gupta model (1973).

It cannot be proved that the targets and allocations which finally appeared in the plans were directly derived from these models, but it remains true that some of these models influenced broad allocations in particular plans. Thus the Mahalanobis model influenced the Second Plan, and the PPD exercises (GOI 1964, GOI 1966) influenced the Fourth Plan and the Chakravarti model (GOI 1973) has determined the Fifth Plan allocations. The other models have been either exploratory exercises in planning

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<sup>1</sup>This model can be treated as a consistency model for it involves no optimization.

methodology, or designed only to provide insights about the long-term implications of alternative middle-term allocations.

A well-recognized feature of the optimizing models has been that they have thrown up infeasible saving rates. Tendulkar (1969) however has presented a model in which the savings of two classes of income-recipients are constrained.

Employment planning has not been integrated with investment planning in most of the models constructed so far. Gupta, however, has attempted such integration in his recent model (1973). And Sen (1960), Srinivasan (1962) and G. Mathur (1965) have derived criteria for the choice of techniques from different forms of intertemporal optimization. These studies brought into focus the basic fact that labor-intensive techniques will increase employment (consumption) in the short run but reduce long-run growth by reducing surplus generation and the growth of the capital stock. More recent theoretical contributions on this subject are P. Das Gupta (1968) and A. Dixit (1968).

Spatial programming models have been constructed by A. Ghosh (1968), and regional distribution of activity in particular sectors has been programmed by M. Datta-Chaudhuri (metallurgy and machine-building) (1967), Randhawa and Heady (agriculture) (1966), and Kanbur and Neudecker (rice) (1966). Some interesting work on the simultaneous optimisation of investment, location and phasing in key sectors is reported in Manne (1967).

Minhas (1968) proposed a separate planning model for the farm sector which would yield the allocation of land, the nonland inputs levels, and the prices required for making output match an exogenously specified sector of demands. But it has not yet been used for actual projections.

Some work has also been done on short-term models to deal with contingencies by P. Desai (1961) and Chakravarty, Sandee and de Boer.

It is unfortunate that though model-building has been refined continuously, the actual performance of the economy during the third and fourth plans has fallen so far short of plan targets that the reliability of the established plan formulation procedures is being seriously questioned.<sup>1</sup> There is growing awareness of the fact that the entire effort to improve planning models has been directed to the rather limited goal of securing the consistency or optimality of a few aggregate quantities - outputs and outlays. Even in the quantitative work (1) manpower (employment) planning, (2) regional planning, and (3) contingency planning to deal with shocks,

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<sup>1</sup>The latest manifestation of the controversy about the planning procedure is the resignation of a Planning Commission member who disagrees with the draft of the Fifth Plan.

have not been integrated with macro national planning. Future research will doubtless have to concentrate on these relatively neglected areas. But, in addition, the "system" aspects of Indian planning will require at least as much research as the quantitative aspects. The word "system" here refers to the system of interactions between the policy instruments (incentives and deterrents) which the government effectively controls and the universe of private decision-making. The optimum distribution of decisions between multiple-planning levels within the government itself is also a "system" question which will need investigation.

It is remarkable that work on macro economic models reviewed in Section I above, and planning models, reviewed in this Section, has remained unrelated. Neither the macro model builders nor the planning model builders have attempted to integrate behavioural equations with planning models. The only important econometric estimates used by planners are those of the income elasticities of demand. Once again the recent Gupta model (1973) is an exceptional attempt to incorporate behavioural relations in a planning exercise. Since such composite models can be obviously more realistic and useful than the pure econometric models and the pure non-stochastic planning models, we should expect more work on such models in the future. (See Section 9).

#### V. POPULATION AND EMPLOYMENT

The three post-Independence Censuses have produced an enormous amount of factual material about the Indian economy and society. In addition to the usual demographic data, the Census reports contain detailed ethnic, linguistic and educational statistics, material on urbanization, and a large number of comprehensive single-village studies. Only a small fraction of this information has been utilized by researchers so far.

Demography has emerged as a separate field of research in India, particularly since the Census authorities and the family planning programme began to provide liberal grants for population research. We shall note here only a few typical studies of population control policies in India.

A lucid description of the evolution of family planning in India is available in a recent paper by D. N. Chaudhri, the Policy Director of the Department of Family Planning, Government of India (1972). It describes the expenditure on the programme in each plan, the number of couples "protected" and contraceptive means supplied, regional differences in the success of the program, its administrative organization and its present and future targets. And the two best critical evaluations of the program are due to Banerji (1971, 1972). He points out that at least 3.7 million couples must be protected every year to reduce the birth rate from 4 to 3% over a 15-year period; but the achieved protection rate has averaged less than a million a year during the 16 years 1956-1972. The extremely low efficiency - 28 sterilization or IUCD insertions per technical person per year - has made the programme prohibitively expensive.

Demographers have been trying to identify the important determinants of fertility among the many social, economic and 'communication' factors which may conceivably influence it. But, as one might expect, the evidence does not suggest clear conclusions; in different regions different sets of factors appear to be dominant. The literature in this area has been surveyed in the forthcoming ICSSR survey of demographic research. We may however draw attention to "An Econometric Analysis of Population Growth" n.d. by Krishnamurty. He correlated the birth rate and the death rate with the average per capita income of 4 preceding years, trend and a dummy variable. The death rate equation also had government welfare expenditure as a variable. It is interesting that in both the equations the per capita income variable was highly significant and had the right (negative) sign. The income elasticity of the birth rate was  $-.93$  and that of the death rate  $-1.78$ . Since the time series used in these equations covered a very long period (1922-1960), while the family planning program is very recent, these results suggest that in the demographic transition in India the influence of growth on fertility may be stronger than the influence of fertility reduction on growth.

In the field of employment, it is convenient to survey the work done on (1) the measurement of open unemployment and underemployment and the characteristics of the unemployed, (2) choice of techniques, (3) migration, (4) labour supply (participation rates), (5) trends in the occupational distribution of the labour force, (6) wage trends and differentials, and (7) urban unemployment and manpower planning. In the field of employment the largest volume of literature relates to the concept and measurement of unemployment and underemployment or surplus labour in the rural areas. The conceptual controversy has centered around the various alternative criteria for defining the state of unemployment: idle time, zero marginal productivity, the excess of labour 'available' over the labour 'required' on the basis of suitable average productivity norms, minimum income or consumption, and willingness or availability for more work. Each of these criteria (and mixed criteria) have been defended and debated. (See particularly Raj 1957, Dandekar and Rath 1971, Dantwala 1956, Mitra 1968, and Krishna 1973). And, of course, the distinctions between idleness at some periods of the year and idleness throughout the year, and between the underemployed willing to move to urban jobs, and those looking for extra work near their villages and supplementary to their present work have been emphasized (Deshpande 1959, Bhattacharjee 1961).

If we concentrate only on studies in which measurement has been attempted, the NSS has used the time and willingness criteria; Dandekar and Rath (1971) the income criterion; and Mehra (1966), Ganyum (1962), Rudra (1973), Singh (1967), Mitra (1968), and Ahuja (1972b) the average productivity criterion. The marginal productivity criterion has not yielded reliable estimates of surplus labour for the simple reason that labour time 'used', rather than labour time 'available' has been included in the production functions. (See Section VI on Agricultural Economics).

The Dantwala Committee (GOI 1970) has presented the most thorough critique of the Census, the NSS and the Planning Commission estimates of unemployment; but the Committee's own failure to compute any estimates on the basis of NSS data has been criticised (Krishna 1973). The most valuable part of the Committee's work, however, was a detailed analysis of the distribution of the unemployed by other characteristics -- age, sex, education, occupation, etc. Information about the dominance of the landless, and of specific age and education groups among the unemployed, and regional differences in the profile of unemployment is clearly crucial for making employment policy.

The Indian theoretical literature on the choice of techniques is well-known. (See the section on Planning). But attention must be drawn to the few empirical single-sector studies of the economic characteristics and implications of alternative techniques and the possibility of determining optimal mixtures of techniques (Bhalla 1964 and 1965, Krishna 1972b, Sen 1960, and Lele 1970).

Descriptive studies of interstate and rural-urban migration have been conducted by Lakdawala (1963), Rao and Desai (1965), and Zachariah (1964), but as far as we know functional analyses of the factors determining migration behaviour have been lacking so far.

The same is true of the work on the participation rate, particularly the female participation rate. Visaria (1971), Sharma (1973) and Mukherji (1973) have done some interesting work to identify the major economic and sociological factors which may explain variations in the participation rate over time, and across different States and social groups. But again no labour supply functions have been estimated to test alternative hypotheses.

The most careful statistical studies of trends in occupational distribution are due to Kothari (1960), the Thorners (1962) and J. Krishnamurti (1967, 1971); of industrial wage trends and differentials to J. Krishnamurti (1971); and rural wage trends and differentials to Bardhan (1970) and Sethuraman (n.d.). But attempts to explain wage behaviour are missing again. A single notable effort is that by Papola (n.d.) who has shown that, in spite of detailed wage regulation, the time-trends of wages in the organized industrial sector have been systematically correlated with the trend of labour productivity with a lag.

Studies of urban unemployment, specially educated unemployment, have been made with Employment Exchange data; but the inadequate and changing coverage of these data, and extensive registration by the employed in search of better jobs, have vitiated the reliability of these data. Many researchers have tried to develop some usable estimates of urban unemployment by adjusting the Exchange data with additional information obtained by the NSS (Bahadur 1968).

Hardly any work has been done on the informal urban sectors (unorganized industry and service activities) which absorb disguised unemployment. Even the NSS has covered this sector in only two rounds.

In view of the growing unemployment, and imbalances between the demand for and supply of particular kinds of skills, manpower planning exercises are badly needed. The PPD and the IAMR have turned out some estimates of demands and supplies for selected professions - engineers, doctors and metallurgists, for example (vide reports listed in the Manpower Journal 1973) but very few overall manpower planning exercises have been undertaken. There are three interesting models constructed by P. R. Panchmukhi (1967), Ramanujam (1967) and Burgess, Layard and Pant (1968) to project the demand for educated manpower by categories, and a survey of the techniques of manpower planning by S. P. Agarwala (1969, 1970). And some interesting work has been reported by S. P. Gupta in his recent book on planning models (1973).

The "returns to education" approach to the explanation of manpower flows has also caught on (vide the numerous studies edited by H. N. Pandit (1969)). But educational outlays and admissions, and salary structures in India are determined by so many irrational, institutional and legal factors that direct quantitative manpower planning will be needed as an indispensable guide to policy. The returns estimates can be used only to design incentives for channeling manpower flows.

There is a general consensus in India about the proposition that a slow rate of accumulation is the primary constraint on employment growth, and therefore investment must be stepped up as rapidly as possible. Population policy cannot ease the employment problem in the short run; nor can the participation rate be controlled in a democratic polity. But the rate and pattern of technological change will have to be regulated in some sectors. A massive rural works program will have to be maintained, and manpower (educational) planning introduced. All these topics will require intensive work in the coming years. The relevance of the product-mix to employment growth has also received some attention recently (Krishna 1972b) and will become another fruitful field of research.

## VI. AGRICULTURAL ECONOMICS

The literature in this area is very voluminous indeed. But the important contributions can be conveniently reviewed under three headings: (1) studies of the growth and composition of output, input requirements, and technical change, (2) studies of price movements, supply response and marketing, and (3) studies of the institutional structure of agriculture.

The major work in which a decomposition identity was used to break up output growth in Indian agriculture during the decade 1951-1961 into the contributions of different factors is the paper by B. S. Minhas and A. Vaidyanathan (1965). Similar work was done by Giri (1966), Chatterji (1966), and Shetty (1969) for different time-periods and by Parikh (1966b) and others with State data. All these studies show that up to the initiation of the high-yielding varieties programme in 1966-67,

area growth and changes in the cropping pattern accounted for all but a small fraction of output growth. But they fail to answer the more interesting question of the relative contributions of different factors to yield growth itself.

Time series production functions have also been used to decompose output growth in individual states. (Krishna 1964, Parikh 1964, Venkataswarlu 1965, and Shetty 1969). Growth in the Punjab was shown in these studies to be due mainly to the expansion of acreage and irrigation. But like most time series studies these exercises are beset by the multicollinearity problem and therefore cannot disaggregate inputs finely enough to separate the true separate contribution of each.

In addition to time series studies we have at least thirty-five cross-section production function studies listed in C. H. Shah's survey of agricultural economics research (1971). A majority of them fit the log-linear form to the recent farm management survey data. The definitions of dependent variables and the lists of independent variables differ widely in these studies and many equations are clearly underspecified or misspecified. But many of the better ones have generated a consensus about two important propositions: (1) that subject to the knowledge and resource constraints under which they operate Indian farmers are fairly good optimisers in the sense that estimated values of the marginal products of individual inputs are close to their acquisition costs; and (2) that returns to scale in traditional agriculture appear to be constant. Programming studies, however, tell a different story: they have revealed considerable slack in the realization of the income-potential of existing resources. (D. K. Desai 1961, Jai Krishna 1961, Kahlon and Johl 1962, Majumdar 1966, and Agrawal 1969).

The apparent inconsistency between the results of production function and programming studies of resource-use efficiency needs to be explained. It may be due to the fact that only a few aggregative inputs can be included in production functions; and in programming studies - particularly the earlier ones - all the important constraints were not specified. In more recent programming studies, the constraint structure has covered availability of funds and/or labor in different parts of the year, soil quality differences, the total rotational cycle, etc. But, on the other hand, the cost and income data used implicitly assume a changed technology. Therefore the results reflect the possible income potentials of technological change rather than the efficiency of traditional agriculture. Now that many baskets of innovations are available it is likely that programming will be used increasingly to choose optimal innovations for different classes of farmer in each region.

Two other questions have been vigorously pursued in production studies in India: the question of the existence of disguised unemployment (zero marginal product of labor) in agriculture, and the relationship between farm size and productivity. In most of the estimated production functions

the coefficient of labor actually used turned out to be positive, as it should have, for we can expect it to be zero only if the labor input is defined as the labor force in existence. Desai and Mazumdar (1970), however, obtained an interesting result: in the regressions for farmers not hiring labor at all the labor coefficient was not significant. In other words farms using only family labor were using more of it than the market wage would justify.

The existence of an inverse relationship between farm size (measured in acres) and productivity (measured as output per acre) has been repeatedly confirmed by farm survey data for many regions. To cite a very recent study (Bardhan n.d.) in six different regressions of crop output per acre on net sown area for districts in South, North and Central India, the correlation turned out to be negative, and in three regressions covering Punjab and M. P. it was non-significant.

An abundant literature has grown out of the effort to verify and explain the inverse relationship (Khusro 1964, Mazumdar 1963, Rao 1966a, Sen 1964, 1966). Theoretically, the relationship is not surprising for the elasticities of input substitution even in traditional agriculture are close to unity (Sahota 1966). Given a high elasticity of substitution, some farms can be using more land and less of other inputs per unit of land, and others less land and more of other inputs per unit of land to produce a given output. The average product of land would then be higher on the latter farms. The evidence on nonland inputs does indeed show that small farms have a higher irrigation ratio, a higher cropping intensity, and a higher labour input per acre.

Numerous hypotheses have been offered for explaining these phenomena - the emergence of large farms as aggregates of the relatively less fertile plots of the small farms, the poorer management of large farms due to the leisure preference and absenteeism of landowners, disincentives for laborers working on large farms and so on. But the most plausible explanation seems to be the dualism or the imperfection of the labour market: the larger farmers using more hired labour face a higher market wage than the opportunity cost of the family labour of the small farm.

The deep interest in the size-productivity question has been doubtless due to its profound policy implications. If small holdings are as "efficient" as or more efficient than the large ones, an agrarian reform which breaks up large farms need not entail a heavy cost in terms of output loss; though, of course, if a different criterion of efficiency, net surplus for example (and hence the farm sector's contribution to accumulation and growth) is used, the conclusion may change.

In recent years - especially after the availability of authentic area, yield, and output growth rate calculations by the DES - some work has been done to explain inter-regional growth differences (vide numerous papers in the IJAE 1972). Again, irrigation turns up as the most important

explanatory factor, because it has been a precondition of all the other important innovations. But many researchers have also emphasized the importance of inter-regional differences in the quality of entrepreneurship or management. The proverbial performance of Punjab and the Punjabis settled in other States (northern U.P. and northwest Rajasthan) has been compared with that of the other States and local farmers in the same areas. But the explanation of these differences inevitably leads into a discussion of the cultural-sociological intangibles which determine the motivations of different social groups.

Output projections have been attempted by the NCAER (1962) and the Planning Commission (GOI 1964a). The alternative techniques of projection have been reviewed by Krishna (1969), but the only methodology actually used by planners is to project the growth of major inputs (land, irrigated area, improved seeds, and fertilizer), multiply the projected input levels with standard yield multipliers (the so-called yardsticks) and add up. In at least two plans, projections derived by these methods have proved to be unacceptably wide of the mark. The main reasons, apart from the droughts for which the projectors make no allowance, lie in the neglect of lags in the diffusion of innovations, interruptions and shortages in the availability of inputs and the slow growth of farmer participation rates. The multipliers used were also optimistic, derived as they were from experimental conditions. And they were not additive though they were used as such.

Projection is indispensable for planning. It is therefore necessary that more research effort is devoted to projection with better and more realistic models.

The input whose demand has been studied most carefully is fertilizer. Ashok Parikh (1965, 1966a, 1969) tried several alternative demand function specifications. But Gunwant Desai (1968) has obtained the best results with cross-section regressions of district-level fertilizer consumption on irrigated area and the relative price of fertilizer. Both variables turned out to be significant. Desai has used his results to project fertilizer demand. His estimates reveal likely economic absorption to be less than that computed from the doses officially recommended on technological grounds. Minhas and Srinivasan (1966) have also computed optimum fertilizer consumption separately for owners and tenants raising traditional and HY wheat varieties under rainfed and irrigated conditions. Again their results show that the traditional farmers can profitably use more fertilizer than they do; and the HYV farmers should use less than the officially recommended doses.

The employment effects of technological change, especially farm mechanization, have recently aroused renewed concern. The important contributions include D. K. Desai and Gopinath (1973), Kahlon et al (1972), C. H. H. Rao (1972), Billings and Singh (1971), and Krishna (1972c).

Most of these studies have used the observed changes in the labour requirements per hectare due to mechanization and/or accompanying innovations to project the macro employment effects in the regions under study. In the Billings and Singh exercise, farm employment in the Punjab is projected to decline by 17% and in Maharashtra to grow by 4% by 1983-84. Sethuraman has computed the indirect expansion effect of increased input demands from wheat and rice farmers growing HY varieties (1971). And Krishna (1972c) has shown that the positive indirect final and intermediate demand effects of growth with assumed technical changes will outweigh the direct negative effects, but the net growth of employment may still fall short of labour supply growth. The effectiveness of the input-output technique for measuring and separating all the important direct and indirect effects has been demonstrated.

Two studies have attempted benefit-cost analyses of tractorization: Rao (n.d.) and Desai and Gopinath (1973). But as Krishna has pointed out the high benefit-cost ratios of tractorization estimated by them are due to misattribution. Productivity effects which are really due to innovations other than tractorization are wrongly attributed to tractors. Primary data clearly show that the irrigation ratio and cropping intensity are higher on non-tractor farms and yet all or most of the irrigation effect and the intensity effect is credited to tractors in the benefit-cost studies.

Numerous studies of the factors explaining the rate of diffusion of innovations have been completed. An interesting result is that, as in the West, the time path of the proportion of adopters describes an S-shaped curve. (Bose 1969, Shetty 1967). But the evidence about the relative effects of different factors influencing adoption yields no conclusive results. Economic factors, such as the holding-size or assets owned, tenure-status, access to credit and the irrigation ratio turned out to be dominant in some studies; but in others "informational" factors such as education or exposure to extension, or social factors such as age and social status were found to be more important. (Shetty 1967, 1968, Desai and Sharma 1966, Kaushal and Malik 1970).

In the field of supply response and marketing and price analysis, the first important set of studies examined the relationship between relative crop price movements and the allocation of land between different crops. After the two original studies (Krishna 1963, Dharam Narain 1965) showing that, except in the case of a few crops and areas, Indian farmers do reallocate land systematically in response to relative price changes, there has been a spate of similar studies testing the same hypothesis, with essentially the same methodology, in different regions of India. But alternative specifications and alternative measures of the weather and price variables have been tried. And the influence of uncertainty has been explored; some evidence of risk aversion has turned up. (Dayal 1965, Acharya and Sengupta 1966, Jai Krishna and Rao 1965, Kaul 1967, Maji, Jha and Venkataramanan 1971, Parikh n.d., Pillai 1969, Satyanarayana 1967, Sengupta and Sen 1969, and Sethi 1966).

There is only one study of the aggregate agricultural supply function (for the Punjab); but the elasticity of aggregate supply has been computed from the elasticities of the acreages and yields of individual crops. The aggregate supply elasticity turned out to be positive for the period 1907-46, and negative for five districts for the period 1951-64 (Herdt).

Rigorous work on the marketing of farm products in India has discredited the popular rhetoric about the profiteering and exploitative character of marketing in India. The two major works by Uma Lale (1971) and Ralph Cummings (1967) clearly show that (1) petty trade, being overcrowded as an absorber of disguised unemployment, is fairly competitive; concentration in trading activity is very modest in the areas studied; (2) inter-market price movements are sympathetic and therefore it is difficult to hold that Indian markets are spatially very imperfect - except when government regulations such as food zoning disrupt normal flows; and (3) marketing margins, which vary from 10 to 40% in the case of different commodities, do not seem to be excessive in view of real storage, transport and interest costs.

There is much descriptive literature on marketing patterns, cooperative marketing, and the market regulation legislation and its effects, e.g., the reports of the Directorate of Marketing, Munshi 1957, Kulkarni 1958 and 1959, and Jasdanwala 1966.

Cotton marketing, including the futures market in cotton, has been studied very intensively by Dantwala (1937), Pavaskar (1969), Venkataramanan (1966), and Krishna (1964b).

A number of marketable surplus functions have been estimated for individual food crops and for groups of crops with time series data as well as cross-section data. In cross-section functions the surplus is found to be positively correlated with holding-size, or output (Krishna 1965, Vyas and Maharaja 1966, Kahlon and Vashishtha 1968). In time series studies or indirect estimates (Thamarajakshi 1969, Bardhan 1964, Krishna 1962, Nowshirwani 1967, Krishnan 1965) the evidence about the price elasticity of the surplus is inconclusive; both positive and negative elasticities have been estimated.

Many writers (Jai Krishna 1969, Sharma 1970) have noted the recent shift in the traditional pattern of weekly (or monthly) deliveries over the season: one or two prominent peaks. The increased holding power of farmers and government price and procurement policies have produced a more erratic pattern.

When we turn to work on the institutional structures the most massive body of literature relates to land reform. There is hardly any variety of land reform which Indian States have not legislated: tenancy reform to fix rent ceilings and confer security of tenure and acquisition rights on tenants; consolidation of fragmented holdings; the imposition of ceilings on landholdings and the redistribution of surplus land; and the

encouragement of cooperative and collective farms of all types. Numerous descriptions and evaluations of these reforms are available. (GOI 1963, Khusro 1958, Dantwala 1961, Minhas 1970, Dandekar 1962, 1964, Krishna 1961, Ladejinsky 1964, 1965, Raj 1969, Joshi 1971). Almost all these reforms, except the abolition of some intermediaries and the consolidation of some holdings in some States, have failed.

There has been a general intellectual consensus about the desirability of redistributing land on the ground that (1) neither large-scale commercial agriculture nor the organized industrial sector can absorb the rural labor surplus; (2) the output loss due to a less skewed holding structure will not be unacceptable thanks to the size-productivity relation in Indian agriculture; and (3) a drive to reduce inequality in the distribution of the main rural asset is imperative in view of the Indian commitment to socialism. There are very few issues on which there has been some controversy: whether the landless or the very small farmers should be given priority as recipients of redistributed land; whether redistribution is meant to create a stable small-farm structure or to serve only as a stepping stone to cooperativisation/collectivisation; and whether consolidation would not be easier after the State undertakes basic investments on the improvement of large tracts to be consolidated (Joshi, op.cit., Minhas, op.cit.).

The present upshot of the whole discussion of the failure of reform is simply that legislation will remain infructuous without the unionisation and politicisation of the potential beneficiaries of reform.

Another body of literature that should be mentioned is that pertaining to rural development administration and extension; and the semi-official cooperative and panchayat structures created to increase people's participation in development. A good recent survey of these topics can be found in a series of nonofficial studies sponsored by the Ministry of Agriculture.<sup>1</sup> The multiplication of special employment-intensive rural works programs has also induced many evaluations. (Jodha 1973, Gupta 1971, Ahuja 1973a, Krishna 1973, Minhas 1972). But the pathology of rural administration, as indeed of India's general administration of which it is an inseparable part, is so deep-rooted that little improvement can be predicted until the weaker sections of the community acquire a greater share of effective political power. As the awareness of this truth deepens, researchers are turning to studies of the movements of the rural poor. (Bandopadhyay 1973, Everts 1971, and Muthiah). As many as nineteen research institutions are currently surveying the history and present status of peasant movements and organizations in seven States.

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<sup>1</sup>Vide papers presented at the Conference on the Adaptation of Administration and Institutions to Changing Levels of Agricultural Development, 1973.

## VII. INDUSTRIAL ECONOMICS

One of the dominant themes in recent research in industrial economics has been concentration of economic power. Three officially sponsored inquiries have stimulated the discussion: the Mahalanobis Committee Report (1964), the Dasgupta Monopolies Inquiry Commission Report (GOI 1965a), and the Managing Agency Enquiry Committee Report (GOI 1966b), though the basic statistics were put together by many individual writers (Mohnot 1962, Joshi 1965 and Hazari 1961). The first committee found excessive concentration in the corporate sector. The second put together comprehensive data both about overall concentration and concentration in individual industries and recommended monopoly control legislation (which was enacted in 1969). The third of the above commissions recommended the discontinuation of managing agency in some industries and its continuation in others. The methodology of the Dasgupta Commission has been criticized extensively by Hazari (1965), Prakash (1966, 1967) and Sandesara (1966). They demonstrated that the commission underestimated overall concentration and overestimated product concentration. Among nonofficial studies the most important one is due to Hazari (1967b) who measured the share of private corporate assets held by each of the top 20 "business groups" in 1951 and 1958. The Monopolies Act (GOI 1969b) has been critically examined by Oza (1970). In an interesting study Chaudhri (1966) has shown that most of the large firms in India do not even spend much on research.

The complex system of industrial licensing in India has also been the subject of many inquiries (GOI 1966c, GOI 1969a and Hazari 1967a). The evidence clearly shows that licensing has increased concentration; it discriminates against the small and medium entrepreneurs; and the long lags in the processing of applications entail heavy loss to the economy. Two Reserve Bank of India studies (1968, 1969b) have shown the lags between incorporation and project completion to be as long as three or four years. The government has recently (1973) streamlined the licensing system once again after a series of new inquiries.

Good general assessments of industrial growth and diversification during the first three plans can be found in Desai (1969a) and Medhora (1968). Phagwati and Desai (1970) have offered the most comprehensive critique of the whole range of Indian industrial policy.

There is also, of course, an enormous literature on individual industries, their evolution, present problems, and demand and supply outlook, and on small industries, which Sandesara has reviewed (1970). The two studies which introduced much-needed realism in the early small industry debates are due to Dhar (1958) and Dhar and Lydall (1961). They showed on the basis of carefully collected field survey data that in many industries small units have low capital-labor ratios but very high capital-output ratios in comparison with large units. Prasad's work (1967) contains a wealth of economic data on capital intensity and

labor and capital productivity in a large number of cottage industries.

Production functions of the Cobb-Douglas type have been estimated for the iron and steel industry by Sarkar (1965), cement industry by Gupta (1972) and the cotton textile industry by Rao and Anjaneyulu (1972). Functions for total industrial output in the Census/Survey sector have also been estimated by Murty and Sastry (1957) and Y. Yeh (1966).

Panchmukhi (1973) has subjected these studies to detailed technical criticism. He shows that many of the results remain unreliable due to some rather elementary statistical deficiencies: the use of variables in current prices when time series data are used; the use of only four to nine aggregative observations to estimate two or three parameters; and the neglect of problems caused by changes in the coverage of the time series, and of the effect of government controls.

Diwan and Gujrati (1968) have estimated CES production functions for all the 28 CMI/ASI industries. Their major finding is that in a large number of industries the elasticity of employment with respect to output is low; the elasticities of substitution are also quite low (less than 0.5); the wage rate seems to have risen faster than the marginal product of labor; and there are substantial economies of scale.

Cost relations have also been estimated. Gupta (1968) found evidence of average cost following the text-book U-form in five industries, an L-shaped form in 18 and a horizontal or gently falling linear form in six. These findings are remarkably similar to the results of cost curve research in the West.

Changes in the size structure of industries have been studied by Sarkar and Ghosh (1963), Hingorani (1964) and Krishna (1964a). It appears that within the organized sector the relatively small units have been losing ground to the larger units in respect of the share of output.

Productivity trends in the industrial sector as a whole and in particular industries and industry groups have been analyzed by Prakash (1969), Sandesari (1969), Mehta (1969), and Krishna and Mehta (1968). In the organized sector as a whole capital-intensity and the capital-output ratio have been rising at a rapid rate - the former at about five per cent a year - labor productivity has also been rising, but at a slower rate than the average compensation of labor.

The existence of unutilized capacity has stimulated a great deal of research, especially since the recession of 1967. (NCAER 1966, Koti n.d., RBI 1969, 1970, Patnaik 1966). Many alternative explanations have been offered: insufficient competition, shortages of complementary inputs and equipment, administered prices, etc. But an exceptional analysis which carefully blends supply and demand factors together in the explanation of capacity utilization is the one offered by Paul (n.d.).

Price fixation by the Tariff Commission and other official bodies and cost-price relationships have been studied by Rosen (1957), NCAER (1965), Sanyal (1966) and Paul and Mote (1967).

### VIII. REGIONAL ECONOMICS

In this field we find studies on (a) the general economic regionalization of India, (b) development-oriented surveys of small rural regions (groups of villages, blocks, districts, or river valleys), (c) development-oriented surveys of metropolitan urban areas, and (d) studies of industrial location and specialization.

Studies in the general regionalization of India have been surveyed by Rao and Bhat (1972). Official attempts to delineate regions include those of the Planning Commission (GOI 1965) and by the Census of India under a joint Indo-Soviet research project (Sengupta and Sdasyuk 1968). Important contributions by individual researchers include L. S. Bhat (1967), V. L. S. P. Rao (1964), C. D. Deshpande (1948), A. Mitra (1965) and M. N. Pal (1969). It should be noted that most of these studies have been completed by geographers. They have used a variety of physical criteria for regionalization, and regionalized large masses of data. It is now necessary that multidisciplinary teams of geographers, economists and scientists process these data in order to develop regional projects.

The cities of Hyderabad, Greater Madras, Bangalore, Kanpur, Delhi and Calcutta have been the subjects of detailed studies in connection with the preparation of their master plans. This literature has been surveyed by S. M. Alam (1972).

Similarly all the major river valleys have been surveyed in connection with river valley development projects. (P. Dayal 1972).

Comprehensive rural development projects for small groups of villages, individual blocks, or sometimes districts, have been prepared in India on a considerable scale. Most of them are simply compilations of official data, but some projects prepared (and sometimes administered) by recognized voluntary or semi-official agencies deserve special mention: Baroda, Etawah, Musahiri, Nilokheri, Wardha, Rangpur, Kanchanpura, Jamui, Pratappur, Santhal Parganas, Koraput, Dandakaranya, the IAD projects of the KVIC and the Growth Center projects. A history of rural development projects is currently under preparation. The first volume covering the Baroda and Etawah projects has already been published (Kavoori and Singh 1967).

The older descriptive studies of the Location pattern of the major industries in India by Lokanathan (1932), Thadani (1952) and Mehta (1955) have now been superseded by more rigorously quantitative studies. In one of the most important recent contributions, Alagh, Subrahmanian and

Kashyap (1971) have used State-wide ASI/CMI data to compute industrial specialization coefficients for all the States of India for 1956 and 1965. The coefficients show that only three States where industrialization began the earliest have the most diversified industrial structures; the seven very latecomer States have the most specialized structures; and five States stand in the middle. They have also discovered the existence of block-diagonality in the inter-industry flow structures of some States, which leads them to suggest that the States' plans for industrial development should aim at exploiting fully the two-way linkage potentials of their key industries. Localization coefficients of individual industries and industry groups for each State, and regional employment multipliers have also been computed. Their studies have thrown up important suggestions for coordinating national planning for the development of core sectors with planning for industrial development in each State. But it is necessary that ex-post analyses of past patterns are supplemented by forward-looking studies of regional development potentials.

Many State-level input-output tables are being constructed. Tables for Gujarat and Rajasthan have already been made and Punjab and Haryana tables are under construction. Analyses of commodity flow patterns have also been initiated in the Railway Board and the Sardar Patel Institute. The effort to generate input-output tables is subject to a serious limitation. Coefficients for the (extremely small) set of activities covered by the ASI have a firm basis but the other parts of the coefficient matrices will remain very unreliable until a massive effort is made to collect additional data for the farm and informal industry and service sectors.

There have been numerous attempts to quantify and explain inter-regional disparities in agricultural and industrial growth. The numerous symposia on the effects of the new HY varieties in the IJAE and the special "Review of Agriculture" numbers of the EPW typify the discussion of agricultural growth. Industrial growth disparities have been reviewed by Dhar (1968).

The evidence in these studies suggests that, on the whole, disparities in agricultural growth are increasing whereas industrial development has tended to level down regional differences.

#### IX. CONCLUDING OBSERVATIONS AND SOME SUGGESTIONS

Any general survey of a body of social science literature always gives rise to three sorts of evaluative questions: (a) questions about the relevance and importance of the topics pursued from the viewpoint of policy formation; (b) questions about the quality/methodology of the work done from the viewpoint of contemporary standards of scientific work in the field; and (c) questions relating to the utilization of the knowledge generated. We can try to make a few judgments on each of these questions on the basis of the foregoing review.

The agonising Western debates on the relationship between positive and normative economics, and the value-neutrality of economics,<sup>1</sup> never seem to have interested Indian economists. For they have worked explicitly and continuously on every field of policy, influenced opinion formation, legislation and administration, and also tried to evaluate the effects of policy. In their policy orientation, they have drawn simultaneously on at least five major traditions: the Western neoclassical reformist tradition, the Fabian tradition, the Marxist tradition, the institutionalist tradition, and the Gandhian-utopian-decentralist tradition.

The pervasive influence of Indian economists on policy and the sources of their own policy preferences can be clearly perceived if we recall the major policy concepts which they have successfully shared with Indian policy makers in the last 25 years; and their obvious links with particular schools of thought. Labor legislation, progressive taxation, federalism and market regulation are plainly due to Western reformism. Radical agrarian reform legislation has been influenced by Gandhian idealism, institutionalism as well as the Marxist socialist tradition. Cooperationism, village self-sufficiency, bhoodan, panchayati raj and the sarvodaya and antyodaya movements have been inspired by Gandhian and utopian thought, and partly by Maoism. Emphasis on public sector heavy industry, import substitution and self-reliance is the product of nationalism and Russian influence. Central planning and licensing of all investments is a Fabian-Russian gift; so is the nationalization of insurance and banking and the progressive expansion of the public sector so that it controls at least "the commanding heights" of the economy. Detailed quantitative commodity and exchange controls are wartime measures which were later converted into regular instruments of planning. And the emergent control of monopoly is Marxist in its rhetoric, and American-British in legislation.

In view of this plurality of influences, often contradictory, it would be clearly wrong to hold that the Western European-American influence has been dominant. Many policies, such as priority for capital goods, import substitution, extensive commodity and exchange controls, centralized investment licensing, and sometimes the dominance of public investment and ownership in certain fields, have been steadily maintained with the broad support of Indian economists in spite of voluminous Western criticism. Also, the recent neoclassical theorising about distortions and optimal intervention has had no influence. And, apart from a few exercises, the field of effective benefit-cost analysis based on neoclassical welfare economics remains strictly circumscribed.

Similarly, of course, the influence of utopian and Marxist traditions has been only partial and spasmodic.

With regard to methodology, too, full-blown pluralism has characterized Indian research. Thus, production function, supply function, and demand function studies have been clearly marginalist while econometric

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<sup>1</sup>For a recent discussion see Myrdal (1966) and Ady (1973).

and monetary model building has been predominantly Keynesian. Planning exercises have liberally used input-output and programming techniques. Programming has also been used in numerous sector studies; and the use of other OR techniques is growing rapidly in institutes of technology and business administration. Regression analysis is all-pervasive; and the estimation of simultaneous equation models has begun to flourish.

As for the channels through which economists influence policy we may mention civil service training, committee and commission membership, ministerial advisory work, seminar and conference participation, popular writing, and broadcasting. Very recently the States have been setting up their own Planning Boards, and economists have been appointed members, and in some cases, vice-chairmen, of the boards constituted so far, along with politicians and civil servants. And some economists/econometricians have always been members of the Central Planning Commission and their macro model computations have decisively influenced major allocations in all the five-year plans.

Thus, Indian economists obviously do not need any persuasion to engage in policy-oriented research. They have been engaged in such research on a fairly large scale and they have been using all available methods of attacking the problems perceived by them, and a variety of institutions to influence public opinion and policy. But the relationship between their theoretical, empirical and policy-oriented work needs deeper reflection.

In most areas of policy, except a few (noted below), policy "advice" is not really derived from any rigorous professional work. It is formulated on the basis of meager data, some ad hoc reasoning, strong ideological-valuational preferences, or some legal-administrative common sense, even though relevant theoretical and statistical work is being done at the same time. The two universes of theoretical-statistical work and policy advice are only tenuously connected. The economist leads, so to speak, a double life: the life of a professional theoretical or empirical researcher, and a 'common sense' policy adviser.

There are a few exceptions to this proposition. Some policy shifts are directly traceable to rigorous theoretical reasoning or empirical work. Thus, for example, the shift to broadbased farm price support in 1964-65 followed numerous rigorous empirical studies of farm supply response which weakened the prevailing unverified assumptions about farmers' insensitivity to price movements. Inhibitions about the promotion of capital-intensive techniques were diluted by theoretical demonstrations of the opportunity cost of short-run employment generation in terms of surplus generation and long-run growth. An anxiety about the productivity effects of land reform has been reduced by the finding that productivity per unit of land can be higher on small farms, and some highly productive farm technology-mixes are scale-neutral.

It is significant that in all these cases theoretical/empirical work was done explicitly and consciously to derive policy implications.<sup>1</sup> But in numerous other policy areas, theoretical-empirical work and policy advice have been effectively divorced. It should be added, of course, that this situation is not peculiar to India; and it does not reflect the weakness of individual professionals. It reflects the limitations of the present state of development economics, if not economics itself.

The causal systems of complex mixed economies have simply not been adequately and realistically conceptualized so far. Many important objectives, structural/institutional constraints and variables which influence real-world decision-making and interaction processes do not enter the available theoretical models. Therefore, very often the models neither have predictive efficacy, nor yield many usable policy implications.<sup>2</sup>

This proposition is not meant to suggest a rejection of theoretical abstraction, for without it we would understand real causal processes even less. It only emphasises the need for more completely specified theoretical models. In fact, development economics will now be able to contribute more effectively to policy-making only after some theoretical progress is made in the construction of better models. Theoretical work in development economics should, therefore, be given very high priority.

Meanwhile researchers can continue to fill important data gaps and give the best advice they can on the basis of some ad hoc reasoning. It may be that the common-sense reasoning of trained economists on some policy issues is somewhat better than the non-economists'. But it should be recognized that in the present state of development economics the economist qua economist is not necessarily a better adviser than an intelligent generalist. Before listing some research gaps, it may be useful to indicate briefly the nature of the more completely specified theoretical models we need.

In a mixed developing economy millions of households or, more accurately, small household-firms, household-farms, and pure labor-supply households, co-exist with a small sector of large modern units in the private sector. And the private sector co-exists with a substantial public sector engaged in production, marketing and financial activity. And then there is the government's regulatory apparatus. The private units are free to take consumption decisions, saving and portfolio decisions, labor supply, education and migration decisions, land use decisions, many production

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<sup>1</sup>This suggests, incidentally, that at least avant-garde Indian economists have understood and used Max Weber's neat formulation of the relation between positive and normative propositions: "The inversion of 'cause and effect' propositions into 'means-ends' propositions is possible whenever the effect in question can be stated precisely." (Max Weber, *The Methodology of the Social Sciences* (Tr. Shils and Finch), Free Press, New York, 1964, p. 40.)

<sup>2</sup>This inadequacy of development economics is being widely appreciated. See Ady, op. cit., Balogh & Balacs (1973).

decisions, and borrowing and lending decisions, subject not only to the normal natural, technological and financial constraints, but also the constraints imposed by a complex mass of legislation and administrative operations. The government, on the other hand, tries to create and operate this control system in the face of a vast universe of private decision-making.

The economy is also mixed in the sense that numerous objectives, not always complementary, are being pursued by policymakers simultaneously: rapid growth, full employment, distributive justice, and self-reliance. In addition, there is "mixedness" due to the fact that the commitment to democracy permits and promotes the free interaction of many organized, and progressively organizing, economic class interests with unequal economic power, and the resultant motley of class pressures constantly tilts the rational pursuit of declared objectives. Finally, there is the fact that policy is diluted in the course of implementation by an administrative system which has interests and preferences of its own.

It is obvious that any models of private behavior in this situation which do not specify important government-control constraints which the private agents obey, ignore, or circumvent at a cost, are incompletely specified. And any models of the effects of government action which do not specify the plurality of objectives, the operation of important class pressures, the administrative distortions of policy intentions, and the obedience-defiance reactions of private decision-makers will be poor predictors. Perhaps this explains why important phenomena like the large-scale evasion of land reform legislation, steeply progressive taxation, and commodity controls, and the growth of bribery, black money, smuggling, resale of licenses and import entitlements, the leakage of foreign exchange and multiple-price markets are not explained by available models.

Most of the models we have are either pure models of private gain maximization with very few legislative-administrative constraints; or pure "models" portraying a command economy where private decision-making and defiance are assumed to be non-existent and quantities and prices ordained by the government are expected to materialize simply because they have been ordained. Mixed economy models in which the behavior of private agents is constrained by the existence of a massive regulatory system and the operation of government is constrained by large-scale private action have yet to be constructed and tested.<sup>1</sup> Other aspects of mixedness mentioned above can also be specified in these mixed economy models. Such models are likely to mirror the reality of causation in mixed

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<sup>1</sup>The small recent literature on "The Firm Under Regulatory Constraint" (Averch and Johnson 1962 and Takayama 1969) can be the basis of further work on mixed micro models. We can model different kinds of household firms and farms under different sets of government control constraints; and also government operations under the constraints of private behavior.

systems much better than the models in use at present.

Maximization of private or public economic welfare subject to constraints, and the balancing of supplies and demands, will still remain powerful foundation-ideas for modelling economic reality. But it is necessary to capture the true objective functions of different economic agents and the full spectra of the constraints which restrict their decision variables.

The failure to model the "mixed" reality accounts for some of the features of research we have reviewed: the omission of obviously important government policy variables in the specification of private behavior relations such as investment or money-absorption functions; and the comparative neglect of public finance, project evaluation, and studies which rigorously explain the limited successes and failures of public policies. It also explains the difficulties of communication between the administrative-political mind which simply believes in ordering "just" prices and quantities and ignores the consequences of private decisions, and the academic mind which neglects government policy variables. Both accuse each other of irrelevance and lack of realism with some justification.

Even before good theoretical models come up, research in applied economics can proceed in many neglected areas with a greater awareness of the need for rigor and realism. We can indicate a few of these areas. The list is only suggestive because (a) academic researchers must remain free to pursue any inquiries they want and (b) since there are many national and international research-sponsoring and financing agencies in India, there is no need for a single, small list.

#### Macroeconomics

The money-creation process, and money-multipliers.  
 The private (legal and illegal) money-lending market and its interaction with the organized money market.  
 The distributive effects of inflation.  
 Rural saving and investment behavior.  
 The economics of rural branch banking.  
 Models of the dynamics of price indices, general and sectoral.  
 Objective criteria for optimum monetary growth in a mixed planned system.

#### International Economics

Measurement of effective tariff and quota protection; and effective export subsidies.  
 Optimum tariff, quota and subsidy structures.  
 Continuous studies of internal and international cost and price differences in important sectors.

Finance

The overall progressiveness of taxation or taxation-expenditure systems.  
 The allocative effects of Central and State commodity taxation.  
 Optimum wealth taxation.  
 Taxation of capital gains.  
 The effects of corporate taxation on internal financing and portfolio decisions.  
 The optimum income-tax schedule.  
 Rationalization of agricultural taxation.  
 Optimum commodity taxation.  
 Development of objective federal transfer criteria.  
 Detailed studies of State and local taxation, expenditure and borrowing.  
 Interest rate policy of public sector borrowing and lending institutions.  
 Revenue forecasting.

Planning

Integration of estimated econometric models or parameters with optimizing planning models.  
 Improvement of submodels (equations) of production sectors, trade, government operations, factor shares, investment, consumption, price formation, financial flows, etc., in comprehensive econometric models in collaboration with specialists.  
 Detailed regional and sectoral manpower planning.  
 Optimum distributions of decisions between multiple planning levels, and coordinated decision rules for each level.  
 Relative efficiency of price and quantity instruments.  
 Multiplication of ex ante and ex post project evaluation studies on a large scale.

Labor

Correlations of the intensity of unemployment and underemployment in households with numerous individual, household and environmental characteristics.  
 Explanation of wage and salary levels, trends and differentials.  
 Rural and urban trade unionism of the unorganized poor and underemployed.  
 The economics of public sector mass housing for the poor.  
 Integration of full employment manpower planning with multipurpose regional rural development projects.  
 Evaluation and prospects of employment guarantee schemes.  
 Wage policy for rural works projects.  
 Rural consultancy services.  
 Optimum combinations of multiple techniques.  
 Testing of migration models.  
 Detailed studies of participation rates (labor supply behavior) of different labor force segments.

### Agriculture

- Evaluation of selected multipurpose regional rural development projects in respect of their productivity, distributive and employment effects.
- Measurement of the direct and indirect employment effects of technological change in agriculture with the input-output technique.
- Distribution and utilization of institutionally supplied credit and material inputs by farm size; and prospects of reservation of small farmer quotas.
- Empirical testing and improvement of models of interaction between farm and non-farm sectors.
- Continuous collection of physical input-output and financial return-cost data for regional farm samples stratified not only by size but by (a) water availability, (b) product-mix, and (c) level of technology.
- Continuous demand, supply and price projection work on all major agricultural commodities with better projection models designed for the purpose.
- Projection of the demand for and production of inputs (seed, irrigation, fertilizer, pesticides, livestock, equipment and power).
- Diffusion of innovations.
- Efficiency of input supply systems.
- Principles for the determination of product price support levels and input price subsidy levels.
- Evaluation of public sector agricultural projects.

### Industry

- Systems analysis of the present licensing system, its lag structure, information flows, and decision criteria. Proposals for rationalizing and speeding up the system. The characteristics of units which can be safely delicensed.
- Effective integration of monopoly control and licensing.
- Inventories of economic data (employment, output, investment and cost, value added, etc.) about alternative techniques and scales.
- Filling the vast gap in factual knowledge about the unorganized and household industry sector by means of large-scale survey.
- Filling the gap in factual knowledge about the service sector by large-scale surveys of professions.
- Evaluation of individual commodity control systems in terms of the achievement of their declared objectives.

### Regional Economics

- Disaggregation and communication of natural resource data for local project formulation.
- Empirical studies of private and public industrial location decisions.
- Economics of natural resource discovery.

Many of these topics will require interdisciplinary research. In fact, the modelling of a "mixed" reality necessarily implies the mixing of variables which "belong" to different disciplines. But the conditions of fruitful interdisciplinary research need to be carefully spelled out. There are many reasons why it has not flourished so far. Such research is bound to be more expensive than unidisciplinary research because more variables need to be observed. But finance is not the main obstacle because research support agencies have often expressed their willingness to support interdisciplinary research. Enthusiasm for such research is often blocked by departmental egos and jealousies. And when departments have still to prove their productivity and excellence in their own fields they hesitate to divert their energy to the uncharted field of interdisciplinary problem-solving.

But a purely intellectual reason also obstructs interdisciplinary work. The conceptual framework of the social sciences, other than economics, is nowhere near the level of logical-mathematical articulation, quantification, estimation, testing and predictive capacity which modern economics has reached. The economists and noneconomists do not find it easy to work together because of this acutely felt inequality of the state of their respective arts. They share no common or convergent idiom or theoretical framework, and no common methodology for proving and refuting propositions. And whereas it is easy for a well-trained contemporary economist to read up and digest the non-economic literature which seems to him to be still at a descriptive-taxonomic-speculative stage without much testable deductive theory, it is not correspondingly easy for the noneconomists to understand the work of contemporary economists. This communication gap naturally prevents collaboration.

In these circumstances, if interdisciplinary groups are commissioned to study something - say, a region - what emerges is an incoherent mixture of all kinds of facts, "factors" and judgments without scientific or operational content. The collections of essays by "area specialists" are good examples of such output. And it is difficult to distinguish them from good journalistic exercises.

But despite these difficulties, conditions for useful interdisciplinary scientific work can be created. Resource and organizational problems can be solved by universities and the research-financing agencies. The "status" problem can be solved by bringing out a number of national and international quality journals of interdisciplinary development research with which some eminent specialists of all social sciences are associated. But the most difficult problem is the "framework" problem. It seems to me that the solution to this problem lies in all disciplines being persuaded to own the OR framework for development research. The OR framework has a problem-solving orientation; and it has always been interdisciplinary. This is evidenced by the wide range of studies it has produced in the fields of military planning,

economic planning, space research, urban planning, futurism and environmentalism, and the variety of scientific tools it has willingly used: programming, statistics, econometrics, simulation, decision theory, inventory theory, queueing theory, factor analysis, network analysis and systems analysis. It admits of no restriction on the kinds of variables it might include in conceptualizing a situation or a system, regardless of the discipline they belong to, provided only that they are shown by the theory of any discipline, or by empirical tests, or by the informed judgments of participant agents, to have an important causal interaction with other variables of the system. At the same time OR is not journalism. It is, in the broadest sense, a form of generalized rationalism or problem-solving with all available scientific techniques.

Systems analysis and simulation in particular have increased the capacity of OR to analyze administrative systems. Continuous progress is also being made in the OR literature in the treatment of multi-objective criterion functions. And methods are being developed for identifying good quantitative indicators and proxies of the qualitative aspects of systems.

If, therefore, OR is widely accepted by social scientists as the common basis of multidisciplinary research and they are systematically trained in all the major techniques used in OR, such research will begin to flourish.

In connection with the training of social scientists, it is necessary to emphasize that a large proportion of university economists in India would welcome and benefit from regular refresher courses in theory, mathematics and econometrics. They are turning out a large volume of research output but some of it is unnecessarily weak in respect of theoretical specification, procedures of estimation and the derivation of valid inferences. This has been pointed out in many recent surveys of econometric work done in India. (See V. R. Panchmukhi 1973).

We have not said anything so far on the most propitious institutional arrangements for social science research. But it is clear from the Indian experience that a basic structure consisting of universities aided by a University Grants Commission, and research institutes and projects financed by a Social Science Research Council, is adequate to support the growth of meaningful social science research. Both the Commission and the Council should have adequate funds provided by the Government but they should be administered, as autonomous institutions, by distinguished academicians. The operation of the Social Science Research Council has been evaluated very recently by the Adisesiah Committee, and will be hopefully reformed in consequence.

The operation of advisory institutions - advisory panels of economists, planning boards and committees and commissions - is also in need of inquiry and reform. Many distinguished economists, engaged in technical research, often do not present their findings in a language intelligible to policymakers, and avoid taking positions on policy issues. The policymakers on the other hand do not take the advisory institutions seriously; important issues are not referred to them before decisions are taken; enough time, information and research assistance are not provided to them to enable them to offer informed judgments on the issues referred; and of course the advice given is often shelved or ignored. The evidence system of the old British-style committees has almost broken down. Perhaps the best approach to reforming the advisory systems will be to introduce procedural changes in the operation of Legislative committees so that they can have a hearings system resembling the U.S. Congressional system which supports good policy-oriented research on a large scale, and sustains an extremely effective dialogue between specialists and policymakers.

But a dialogue is all that the intellectuals and the powers can have. Their worlds, though related, must retain their separate identities.

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List of Abbreviations

- ASI - Agricultural Situation in India (monthly)
- GES - Constant Elasticity of Substitution
- CMI - Census of Manufacturing Industries
- DES - Directorate of Economics and Statistics, Ministry of Food and Agriculture
- ECAFE - United Nations Economic Commission for Asia & Far East
- EEC - European Economic Community
- EPW - Economic and Political Weekly
- GOI - Government of India
- HYV - High Yielding Varieties
- IAD - Intensive Area Development
- IAMR - Institute of Applied Manpower Research
- ICSSR - Indian Council of Social Science Research
- IER - Indian Economic Review
- IIFT - Indian Institute of Foreign Trade
- IJAE - Indian Journal of Agricultural Economics
- IUCD - Intra-uterine Contraceptive Device
- KVIC - Khadi and Village Industries Commission
- NCAER - National Council of Applied Economic Research
- NCERT - National Council of Education Research and Training
- NSS - National Sample Survey
- NPV - Net Present Value
- OECD - Organization for European Cooperation and Development
- OR - Operations Research
- PPD - Perspective Planning Division, Planning Commission, GOI
- RBIB - Reserve Bank of India Bulletin
- UNCTAD - United Nations Conference on Trade and Development



Social Science Research and Rural Development:

The Philippines Experience

by

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## INTRODUCTION

In the field of international development no one is currently "in" or "with it" unless he has something to say about income distribution, employment and growth, perhaps in that order. And it is amazing how much social scientists have to say about these, whether in conferences, in the literature or in after-dinner speeches. The men of the hour to whom their concerns are addressed are the 30 to 40 per cent of the total population whose mass poverty we want to eradicate. Gems of wisdom on the subject abound but only two of them will be cited. President McNamara in his famous UNCTAD address defined the development task as follows:

"Our clear duty for the remainder of this decade is to face up to mass poverty for what it really is, determine its dimensions, locate its whereabouts, set a limit beneath which we will not accept its continuance, and make our first priority a threshold of human dignity and decency which is achievable within a generation...What we need most is to get on with it."<sup>1/</sup>

On the other hand, D.R. Gadgil of the Indian Planning Commission said:

"The real problem in all progress toward egalitarianism is twofold. One is something which arises out of the process of development in an underdeveloped economy. The ordinary process of development in an underdeveloped economy results in strengthening the position of those who are already in strategic positions and making them richer. If a city is growing, the land values at the center of the metropolis grow without people doing anything about it. This is a natural process which happens everywhere. All through the economy if government expenditure on development makes the economy more active, then all those who hold strategic positions in the economy without necessarily doing anything themselves grow richer and more powerful. It happens everywhere unless you do something to counteract the effects. The other aspect is that when government aid and assistance comes in a uniform pattern, the people who are more knowledgeable, who have access to seats of power, who know about things and have the right approaches are bound to profit more from it than those who do not, so that the richer,

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<sup>1/</sup>R.S. McNamara, Address to the UNCTAD Meeting, April 24, 1972, Santiago, Chile.

the more substantial and the middle class profit more than the weaker sections. What we have to remember is that the weaker section is weak because the broad elements of general government policy do not touch it. If we want to bring about egalitarianism or at least not to accentuate inequalities during the development process, we have to take very special steps and these special steps are not easy to take."1/

The choice of these two messages was prompted by the fact that the first one conveys a sense of urgency and places a "now" dimension to the development task and the second one admits how difficult it is to get on with it.

Quite a contrast are the physical and biological scientists who rarely pontificate on the solutions to poverty but instead they get right on with it. As the IRRI Annual Report for 1972 unceremoniously reported:

"Research results have confirmed that zinc is becoming an important yield-limiting factor on large areas of soils well supplied with major plant nutrients. Soil chemists diagnosed zinc deficiency as the yield-limiting factor on more than 47,000 hectares in Agusan del Norte, Philippines which have long remained uncultivated. Dipping the seedlings in a 2 per cent suspension of zinc oxide in water before transplanting - a treatment costing only about \$1.00 per hectare has enabled farmers to obtain yields of four tons per hectare without any fertilizers."2/

The Philippines has its share of both the "hard" and the "soft" sciences all dedicated to the service of mankind. Because the green revolution has received more than passing attention from the social scientists, it is only fair to find out what the social scientists themselves have been up to as far as rural development is concerned. This paper presents in capsule the infrastructure for social science research in the Philippines, the relationship between researcher and policy maker and the links of local researchers with the external research communities. Finally, an attempt is made to outline substantive issues for research as well as for reexamination.

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1/D.R. Gadgil, "Approach to the Fourth Plan," Communicator, Vol. 5, No. 5, September 1969, pp. 16-17.

2/International Rice Research Institute Annual Report for 1972, College, Laguna, Philippines, p. 4.

I. THE INFRASTRUCTURE AND ENVIRONMENT  
FOR SOCIAL RESEARCH IN RURAL DEVELOPMENT

With the creation of the Office of the Presidential Assistant on Community Development (PACD) in 1956, the major rural development program of the country at that time was born. In 1958 research in rural development likewise found a reason for being in the Community Development Research Council (CDRC) which is an interdisciplinary body made up of ten professors from the University of the Philippines selected because of "demonstrated interest in empirical studies of social change." The Council's major functions have been to conduct or support research projects in the rural field. Because "the CDRC was originally established at the behest of the PACD, the Council was envisaged as an external, objective vehicle for evaluation of the agency's program. Moreover it was hoped that research on basic social problems would help direct the PACD in its formulation of policy, operations and training of personnel. The University, on the other hand, perceived an opportunity to obtain scarce funds for the support of research and to free faculty from the excessive teaching load so characteristic at that time."<sup>1/</sup> After eleven years of its existence, in 1968, the CDRC reported a total of 63 studies, 43 of which have been completed. Only 19 have been published and two were in the printing process at that time. For this period, PACD had provided a total amount of ₱1,025,440 to support its program. Among its publications are such subjects as: The Competence of Barrio Citizens to Conduct Self-Government, The Value of Rural Roads, The Farmer Said "No", The Resources, Levels of Living and Aspirations of Rural Households in Negros Oriental, Food Management Practices of Rural Homemakers, The Farm and Home Development Project, Private Voluntary Programs in Community Development, Bibliography of Land Reform, Training Program of PACD, Administration and Management of Barrio Funds, Social Effects of Donated Radios, etc. Although there was quite an attrition from the number of studies (63) conducted to those actually published (21) (about 30 per cent), the 1968 CDRC Report assessed their accomplishment as that of having "helped increase the awareness of both the government and the private sector of the essentiality of uplifting barrio life as a prerequisite to a stable economy." Secondly, it "provided the University the opportunity to pursue two of its major functions - research and community service - by stimulating the interest of the university faculty and graduate students in research work, thus promoting the development of the social sciences in the University." For whatever this general self-evaluation is worth, the CDRC started the academically respectable tradition of research in rural development. Unlike the experience in other countries, being engaged in action or development-oriented research has not hurt the Filipino social scientist at all.

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<sup>1/</sup>M.L. Barnett, "An Interdisciplinary Council for Social Research in Rural Development," The U.P. Research Digest, Vol. 4, No.2, April 1965, pp. 2-6.

Using Hollnsteiner's 1/ and Lynch's 2/ list of selected non-commercial social science and/or humanities research centers as a take-off point, a more expanded inventory of units engaged in research and other activities directly or indirectly related to rural development was drawn up. (See Appendix.) At present there are 30 institutes, centers, departments, etc. under the University of the Philippines System; at least 21 privately supported church-related academic institutions (18 Catholic and 3 Protestant) with some form of social science research unit doing some research in rural development; 8 government non-academic units supported by public funds and 6 non-academic privately supported agencies. Just by sheer size of involvement alone, the University of the Philippines clearly dominates the scene but the Ateneo de Manila University through its Institute of Philippine Culture and Departments of Sociology and Anthropology provides healthy competition for the social science limelight. The Institute of Philippine Culture, for example, has taken a major interest in development-oriented research on land reform, family planning, urban resettlement, integrated agricultural development, etc. The Center for Research and Communication also offers some push to the economists of the Institute of Economic Development and Research of the University of the Philippines. Outside the Greater Manila area, there are small but excellent centers, an outstanding example of which is the Research Institute for Mindanao Culture at Xavier University (Cagayan de Oro) under the leadership of Fr. Francis C. Madigan, who is well known for high quality research. The other notable places are Silliman University in Negros Oriental, University of San Carlos in Cebu, the Central Philippine University in Iloilo, the Notre Dame University in Cotabato, etc. The leadership role played by these institutions in their respective provincial locations can never be underestimated especially with the emerging policies toward regional development. Research in such places revolves around one or two top-notch social scientists who perform a heroic job not only in getting the research done but in training the junior staff members who quite often leave for greener pastures (richer centers in Greater Manila, if not abroad) as soon as they acquire their "wings".

Government agencies charged with the basic data gathering and statistical functions of the country, such as the Bureau of the Census and Statistics, the Department of Economic Research, Central Bank, and the Bureau of Agricultural Economics, are indispensable for rural development. Without the statistics that they generate it is impossible to define the magnitude and dimensions of the rural development problem and to assess the country's development performance. Furthermore without their data, the demographers and

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1/ Mary R. Hollnsteiner, *The State of Social Science in the Philippines*, PSSC Social Science Information, Vol. 1, No. 1, May 1973.

2/ F. Lynch, *Philippine Research on Malay Culture. The Viewpoint of a Participant Observer*, Paper written for UNESCO, Philippines, 1971.

macroeconomists are without their bread and butter. These three agencies plus the National Economic and Development Authority (NEDA), the National Manpower and Youth Council and the National Tax Research Center perform research functions and employ social scientists, mostly young economists with baccalaureate degrees and some units toward an M.S. The few Ph.D.'s in these government agencies usually occupy administrative positions and have little or no time to do research themselves. The School of Economics of the University of the Philippines which has close, although informal, professional relations with NEDA is often commissioned to conduct needed research, tapping the talents of a dozen or more top-level economists in their staff (including visiting professors). The research interest of non-academic privately supported agencies such as the Philippine Business for Social Progress, the Family Planning Organization of the Philippines, etc., is quite recent and came about because of their action programs. Research in their case is not pursued for its own sake but is regarded as a necessary tool for learning about their approaches and evaluating their impact. The Davao Action Information Center which is a project of the Davao Research and Planning Foundation is an interesting experiment on the use of research as "the unifying factor in establishing a community planning and action program" in a place undergoing rapid urbanization. The project includes a research phase which consists of periodic surveys to identify and describe the general indicators of growth and development of the city as well as an in-depth study of six priority problem areas. Once these findings are available, the evaluation-action phase uses these data as a basis for project planning and policy decision thereby ensuring the application of research findings to action programs. The city council, the different government agencies and private organizations which need up-dated information about the city's population, resources and priority problems are the direct beneficiaries of the project.<sup>1/</sup>

As mentioned earlier, the University of the Philippines, which has been designated as the national university of the Philippines, leads in the number of institutes and departments doing research relevant to rural development. This role will probably be expanded by a resolution of the Board of Regents reorganizing the University so that "through the concerted application of science, technology, and the behavioral sciences to the problems of society, it may serve as a more effective instrument of national development while maintaining its commitment to arts, letters and the humanities, as well as to the pursuit of truth and the highest standards of academic excellence."<sup>2/</sup> Despite the University's lead in the extent of commitment to development-oriented research, the actual amount of staff

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<sup>1/</sup> Philippine Business for Social Progress, 1971, Annual Report.

<sup>2/</sup> UPLB Horizon, September 1973, p. 2.

time devoted to such activities is hard to ascertain because there are academic and other responsibilities which have a prior or equal claim on their time. It is no exaggeration to say that the available research manpower is probably as competent as can be found anywhere in the world in their respective fields. Perhaps precisely because of this recognition, there are many competing demands placed on the competent staff by various national, regional, and international development programs. Competence, however, is sometimes diluted by overcommitment which may be reflected in missed deadlines; different versions of the same paper appearing under different covers and titles with new introductions; hurriedly-complied-with reports; substantial reliance on junior staff members who remain unknown; and overlapping assignments. Overcommitment, however, is not solely a function of demand for professional skills and services but also of low salaries, hence the pressure to take on additional projects for supplementary income. Added to that is the "have grant will travel" push which inevitably results in accepting more than what they should. Fortunately, country-hopping is becoming passé among those who have "arrived". They no longer feel the need to validate their professional status by the number of trips taken in one year. Hopefully, selective travel would increase research productivity of the senior researchers.

Another source of overcommitment arises from the University's definition of its function as "an instrument of national development". Different units of the University have their "technical assistance" links with corresponding government and private sector agencies whose work is along their respective fields. For example, the Local Government Center has some relationship with the Department of Local Government and Community Development (DLGCD); the School of Economics with the National Economic and Development Authority (NEDA); the Institute of Social Work and Community Development with the Department of Social Welfare; the University of the Philippines at Los Baños with the Department of Agriculture and Natural Resources, the National Food and Agriculture Council, the Bureau of Forestry Development, etc.; the Institute of Mass Communication with the media practitioners and communication units of various development programs; the Institute of Planning with the Regional Development Planning Centers; the Agricultural Credit and Cooperatives Institute with the Department of Rural Banks, the Agricultural Credit Administration, and the nationwide Rural Cooperatives Development Program, etc. In general, therefore, the University of the Philippines cannot be accused of being an uninvolved "ivory tower". It is not unusual for the University to respond to requests for development assistance in the form of technical expertise in various fields. As one news item said: "U.P. Comes to Aid of Palawan Town." This town is reported to have an infant mortality rate of 50 per cent; one-third of its rural population has tuberculosis and one-fourth has malaria. The School of Fisheries, Institute of Small Scale Industry and the Institute of Community Development worked with the Municipal Economic Development Council to develop a plan of action for the

place.<sup>1/</sup> The University as an agent of social change therefore makes it acceptable for an intellectual to be engaged in development-oriented rather than "pure" research without losing his standing in the academic community. On the contrary the professional prestige, research awards and travel grants accrue to those who are development-oriented. Needless to say, the funding agencies both within and outside the country have contributed immensely toward making this type of research attractive by defining their funding priorities in this direction.

Having created several institutes within the University, it was inevitable that a link between them would be deemed necessary. The National Development Research Center (NDRC) was established "to coordinate and integrate the contributions of the University of the Philippines to the formulation and implementation of plans and programs for national development; to encourage an holistic approach to the problems of national development by providing a center where social scientists can collaborate with others in multi-disciplinary research; and to promote the formulation of realistic programs and policies by closer dialogue and collaboration between key policy-makers and social scientists." To date, the NDRC has two research projects on social change and regional development, one of which is on the Bicol River Basin Development Program and the other is on the Upper Pampanga River Project which is a huge irrigation project financed from a World Bank loan. It should be mentioned that it is not unusual to find the names of the top-level social scientists listed as consultants, resource persons, committee members, chairmen, etc., of several institutes, councils, centers, programs or projects, hence there is an inflationary effect. The number of "warm bodies" are not that many when counted as such. Each one of them has a long list of development-oriented involvements.

Obviously the Philippines does not suffer from lack of institutes, centers, and agencies purportedly dedicated to research for rural development. As the Community Development Research Council said in its 1968 report: "This report is dedicated to the Rural Man whose welfare is the paramount interest and goal of community development." Funds come from a variety of sources but universities, even the University of the Philippines, have little money for research from their own budgets. In almost all instances, money comes for specific projects, not for institutional program support. Although the grants are made to the institution, they are usually earmarked for particular research projects of specific individuals. The credentials of these individuals are therefore very crucial in obtaining research money, and their performance is critical for paving the way for subsequent grants. Among the external sources of support are: UNDP, Ford and Rockefeller Foundations, Population Council, Agricultural Development Council, International Development Research Centre, Asia Foundation,

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<sup>1/</sup> The Carillon, Vol. 14, No. 12, August 1973, p. 8.

ILO World Employment Program, UNFPA, US AID, ECAFE, etc., and even the World Bank. Within the country, the major source is the National Science Development Board (NSDB), and in a much smaller way, the National Research Council of the Philippines, the Philippine Social Science Council and government as well as private agencies and foundations, such as the Philippine Business for Social Progress, which commission these centers and institutes to do research for them. Depending upon the size and urgency of the problem, these agencies are willing to provide substantial funding.

From the period 1959-1965 to 1966-1972, NSDB's support for social science research increased from a total of ₱471,130, or an average of ₱67,304 per year, for the first period to a total of ₱1,448,004, or an average of ₱206,858 per year, for the second period. The expenditures for the first period make up 3.9 per cent of the total NSDB appropriations for research and development. This went up to 6.7 per cent for the second period or a 207.4 per cent increase. This represents the highest percentage of increase among all the areas of research. The rationale behind the marked increase is the "growing awareness of the government of the essential role that social science research plays in laying out the fundamental basis for a dynamic formulation and implementation of national development plans. The Research and Development Program recognizes the desirability of an interdisciplinary approach to research and development. It also places significance on the role that social science can play in the smooth orchestration with the other sciences in achieving economic and social advancement. The thrust of the program for social science is to identify the social and cultural factors that hasten or retard the adoption of modern techniques in essential aspects of human endeavour. It also aims to attain a better understanding of the sources and tendencies of Filipino behavioral patterns from the sociological, political, economic and psychological points of view taking into account the varied aspects of Filipino culture which affect the modernization of society. Supported under the program are studies on methods of introducing innovations, population research, education in science, research methods, urbanization, Philippine studies and policy-oriented research."<sup>1/</sup>

The Social Science Division of the National Research Council of the Philippines in its March 1973 report indicated eight ongoing research projects in agricultural economics worth a total of ₱213,379. It is interesting to note that two which were approved in 1971 and 1972 had not been started; one was approved in 1970 but started only in 1972; one was approved in 1969 but started also in 1972. Three projects are still ongoing beyond the termination date but the balance of funds allocated is still unspent. One clue

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<sup>1/</sup> PSSC Social Science Information, "NSDB Increases Social Science Aid,"  
Vol. 1, No. 2, September 1973.

as to the possible explanation for this state of affairs lies in the minutes of the council meeting where someone had suggested that research grants should be opened to non-members since members were already overcommitted in their research work as they were able to tap other sources of funds. The experience of the Philippine Business for Social Progress with respect to its research contracted to academic institutions is an uneven one. Where a senior person has paid full attention to the task, the result is satisfactory and on time. In other cases, too many other jobs interfered with performance and deadlines. Research projects that are perpetually "in progress" year after year are a symptom of the same disease. Furthermore, there are collected data which have not been analyzed and analyzed data which have not been written up. To accomplish all these takes time and discipline, assuming that competence is present. Sometimes, a project gets deserted because something more attractive and lucrative came along and attention shifted to it.

For the continued growth and usefulness of these social science units, it is essential that a continuing staff development program be provided for. Unless this is done, the demand which already outweighs the supply will have debilitating effects on present programs and the practice of "robbing Peter to pay Paul" will be perpetuated. What is particularly disturbing is the loss of key staff-members in units offering graduate programs where the next generation of social scientists are being produced. It is just like killing "the goose that lays the golden egg" especially if it is a particular "goose" which has been trained to lay a particular "egg" (a field of specialization). Some units go through cycles of "depression" and "elation" as far as staff strength is concerned. When key professors leave either full time or part time, they sometimes have to start all over again, hence there must be a continuous stream of replenishment. The University of the Philippines has just granted salary adjustments to meet rising costs of living. This will help minimize the exodus. Another mitigating social mechanism is the employment of husband-wife faculty, which substantially increases family income. The University also allows its faculty members to go on leave for a certain period to accept other assignments abroad or within the country. This helps the financial situation but creates other problems because while the position is being kept for the professor, no one, not even an excellent substitute, could be appointed to his position on a regular basis. If the substitute is really good, he will not stick around. In the meantime, the professor might decide after a couple of years that he could not afford to work for anything less than an international salary.

Perhaps it is fair to say that among the leading social scientists in the country today, there is ample professional recognition in terms of involvement in exciting and challenging research and reasonable material rewards, even if these come as an occasional "windfall." But we need more of their quality to cope more ably

with the multiple demands. One feels guilty about a B research job when he is easily capable of an A performance. One likewise feels bad about being unable to respond to requests for more studies on meaningful development studies because there is neither time nor adequate trained personnel. Laing, for example, echoes this frustration with respect to population and family planning studies: "Efforts to attract highly qualified local research personnel to work fulltime is critical. Presently only one Filipino with extensive research training and experience is employed fulltime by the Population Commission for research and evaluation."<sup>1/</sup> (Parenthetically, they have just succeeded in hiring away one of our capable research associates who has had eight years of valuable experience in action-research.)

All the Regional Development Centers require development-oriented social scientists who will perform research, evaluation, and planning roles. As one administrator remarked: "One indication of their market value is I called up a number of them several times but they did not even bother to return the call". The graduate students in economics cannot find the time to finish their theses because they are already employed before their M.S. degree materializes. The Association of Colleges of Agriculture of the Philippines (ACAP), which has about 20 member institutions located in different parts of the country are very strategic places for rural development research but they do not have the staff to do the job. Their social science-trained faculty have all become administrators or have heavy teaching loads. A brand-new Ph.D. we produced has just been appointed Executive Vice-President of one of these Universities. Research funds alone, without staff development support, will not solve the problem. The middle-aged social scientist (there are no old ones) has to be retooled also if we want to prevent him from prematurely reaching his limits of incompetence. Otherwise what he cannot do he will not allow others to do. For the socialization of the budding social scientist and the continuing relevance of the middle-aged one, they both need constant exposure, if not immersion, in real life rural development problems. This exposure takes money for per diems and travel whether to the next village or to a neighbor country. Funds to bring about this exposure are hardly ever available except in connection with a research project, but this prior exposure is needed precisely to stimulate the researcher to potentially significant research problems.

Finally, there is a hierarchy within the social sciences. Hollnsteiner in her assessment of the state of social sciences in the Philippines concludes that:

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<sup>1/</sup> Philippines Daily Express, "Family Planning Acceptance Among Couples Has Been Most Encouraging, Says Expert," Nov. 1, 1973, p. 6.

"The highest star in the social science firmament is economics...The non-economic social scientist can only look at his solidly entrenched brothers with a certain degree of envy hoping that someday, he too, will join the ranks of the elect...Yet the educated public is coming to recognize what many economists themselves have admitted - that an economist's understanding of society encompasses only one part of the problem. His models apply best to the monetized sector of the economy but cope less ably with the non-market subsistence orientation of the numerically dominant peasant sector. Sophisticated analysis of price theory and international trade have little direct bearing on the empirical reality out there - the rural areas. Even in the market-dominated areas of the nation, cultural values, social behavior patterns, personal motivations, and developing ideologies impinge on economic behavior often enough to make their importance felt. Why do they do this or why don't they do that or how will they respond are questions raised by administrators trying to get at the bottom of this seeming discrepancy between models of 'economic man' and 'social man'. The realization is growing that the soft social sciences may have some of the keys after all to the problems of motivation and process in modernization."<sup>1/</sup>

Such a declaration of envy is worthy of sympathy, but as long as non-economic social scientists insist on a dichotomy between "social man" and "economic man" the economist will always be the highest star in the social science firmament, for economic behavior is social behavior. Furthermore, as long as the non-economists trace poverty to "aspects of peasant behavior which function in ways to maintain poverty-ridden status and which restrict the extent to which peasants are able to gain the better things in life which they see in the cities"<sup>2/</sup> the policy makers will always find this "shuttlebox of subsistence attitudes" a convenient excuse for not being able to alleviate poverty. After all the peasant is "subsistence-minded"!!

Quite a contrast is the economist who does not heap the burden of poverty on the farmer's mentality but rather defines manipulable variables which explain why farmers behave the way they do. Even a cursory examination of what the economists do will tell us why they are the "stars". They address themselves directly to policy questions and do not hedge in their conclusions. To illustrate,

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<sup>1/</sup> Mary R. Hollnsteiner, The State of Social Science in the Philippines, PSSC Social Science Information, Vol. I, No. 1, May 1973.

<sup>2/</sup> George M. Guthrie, "The Shuttlebox of Subsistence Attitudes," in Attitudes, Conflict, and Social Change, Academic Press, Inc., N.Y. and London, 1972, pp. 191-210.

almost 10 years ago, Ruttan *et. al.* in a comprehensive analysis of the inhibitory effects of environment on farmers' response to modern agricultural technology in the Philippines and Thailand concluded that:

"Both the yield increases of the last decade and the yield differences among the major rice-producing regions in the Philippines and Thailand primarily reflect variations in the environmental factors under which rice is grown rather than differences from variety planted or cultural practices. After the effect of the environmental factors are taken into account, there is little yield increase or yield differential left to be explained by such factors as new varieties, better cultural practices or more intensive use of technical inputs such as fertilizer and insecticides or by economic and social differences among regions and between Thailand and the Philippines." 1/

One major implication drawn by the authors is that the factors which permit a region to increase its yields to the levels currently being achieved in the higher yielding areas of each country are primarily outside the control of the individual farmer in the major rice-producing areas such as Central Luzon or Central Thailand. The modifications in the environment necessary to achieve effective water control and pest control will have to come primarily from public or semi-public agencies capable of organizing resources in a manner not available to the individual tenant or farm owner. 2/

Another analysis made on the possible impact of land reform on inequality is even more dramatic in its policy message:

"A rough estimate of the potential reduction in family income inequality from land reform was made. Various data indicate that the maximum proportion of total family income derived from all agricultural crop rentals is roughly 4 per cent. If this amount of income could be transferred away from the richest 20 per cent of urban families, to which the landowning class belongs, equally shared by the lower 40 per cent of rural families, to which rice and corn tenant farmers roughly belong, without any compensation from the gaining group to the losing group, then income inequality among urban families would be reduced (from a 1971 base) by an estimated 7 per cent

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1/ V. W. Ruttan, *et. al.*, Changes in Rice Production, Area and Yield in the Philippines and Thailand, Paper presented at the Annual Meeting of the Thailand Agricultural Economics Society, Bangkok, Thailand, July 10-12, 1965.

2/ V. W. Ruttan, *et. al.*, op. cit.

and would be reduced among rural families also by about 7 per cent. Furthermore, there would be a narrowing of the income gap between urban and rural families, implying that national income inequality would fall somewhat more by perhaps 8 per cent. There will be a drop in the Gini concentration ratio from .50 to .30."<sup>1/</sup>

The economists' tools for reducing a mass of rough estimates into a single "precise" figure is truly amazing. This figure is now treated as an incontrovertible fact which a policy maker can take into account either positively or negatively when he issues the next decree on land reform. Finally, economists are admirable in their self-sufficiency. They seldom, if ever, find use for anybody else. As one prominent man in the field admitted: "Although we now say that we need non-economists, when we get them in our midst, we don't know what in the world to do with them."

## II. THE SOCIAL SCIENTISTS' LINKS WITH THE EXTERNAL RESEARCH COMMUNITY

By and large, the Filipino social scientist engaged in rural development research is not professionally isolated. Different kinds of linkages relate him to the external research community, such as:

- (1) International conferences on rural development.
- (2) Visiting professor--graduate student tie-up with the latter conducting research in his home country under the supervision of the former.
- (3) Local professor--visiting graduate student tie-up with the former informally supervising the research being done by the student.
- (4) Research contracted or commissioned by international development agencies as in the case of the ILO World Employment Research Programme, the UNRISD Global 2 Study on the Impact of the New High-Yielding Cereal Grains, etc.
- (5) Visiting professors and researchers based in local institutions. It is significant to note that several social science research units, even the small ones, in the country have links with the external research community. For example, the Research Institute for Mindanao Culture has a joint project

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<sup>1/</sup>From a report prepared for the ILO Comprehensive Employment Mission to the Philippines, 1973.

with the University of North Carolina. St. Louis University has visiting professors from the University of Louvain; the Institute of Philippine Culture has a continuous stream of visiting researchers coming from different outside institutions; the University of the Philippines has links with several universities in the U.S. and elsewhere by way of visiting researchers, joint research projects, exchange professor arrangements, etc.

- (6) International development agency missions which put experts from the outside in touch with the local ones.
- (7) A reverse research consulting relationship with the local social scientist playing the role of adviser to a foreign-initiated research project.
- (8) Regional or international research networks focused on certain research problem areas, such as the Study of Changes in Rice Farming in Six Asian Countries organized by the International Rice Research Institute, the Study of Non-Formal Education in Southeast Asian Countries initiated by the SEARCA (Southeast Asian Regional Center for Graduate Study and Research in Agriculture), the various studies on rural development sponsored by the SEADAG; the research activities being undertaken by the Organization of Demographic Associates, and many others which are currently in progress or in the process of organization.

With regard to our relation to external research communities and visiting foreign experts, the issue of Western "cultural imperialism" is no longer a salient one. The defused student activist movement which has always taken a convenient anti-American stance is not an insignificant factor in this new climate. There was a time in the activist era when one almost had to be an Asian or an African to be acceptable to the academic community as a visiting expert. Now being pro-Filipino is no longer synonymous to being anti-American. Other factors which have contributed to the decline of Western cultural imperialism as an issue are:

- (1) We have our own contribution to the pool of international development experts, 65 of them working in 40 different countries with a number of UN agencies. This does not include the academicians and technocrats who are hired as short-term visiting professors or consultants. As experts in other countries, they are probably doing their share of "cultural imperialism" from the point of view of recipient countries. There are a few reports of lukewarm welcome and even occasional rejection. "The shoe is now moving to the other foot," so to speak.

- (2) We have had experiences with non-Western experts and most of us have come to the happy conclusion that insufferability is not a monopoly of the Westerner. Moreover, the middle-class non-Western expert could be more unpleasantly demanding than the middle-class Western academic with respect to facilities, privileges and services, but not necessarily more relevant.
- (3) In spite of all the good intentions and professed desire to invite experts from the LDC's rather than from the developed countries, it is a fact of life that there are not that many good social scientists from the LDC's who would be willing to leave exciting and important jobs in their own countries to spend any length of time away from home. In short, those whom we want we could not get.
- (4) Although the total number of trained social scientists has increased within the past 10 years, the current demand exceeds the supply. There has been some international brain drain via immigration or international marriage but domestic brain drain, the "pirating" of trained staff from one institution to another and the loss of researchers through elevation to administrative positions is a more serious one. Unbelievable though it may seem, we have a need for social science assistance from the outside. It must be recognized, however, that this is a different era altogether and the role of the forthcoming visiting researcher has to be a different one. At the moment there is not much use for advisers. There is also little patience for self-righteous social scientists whether black, white or brown whose only prescription for us is the commune while they themselves live in decadent capitalism. What is needed are "doer-colleagues" who will share in the tasks of research, evaluation, program implementation and teaching. The need is for colleagues who will make productive contributions (tangible ones) to our total social science research efforts while enhancing their own professional careers. Laing is even more demanding in his suggestion with respect to the role of foreign specialists who come to the Philippines to help evaluate family planning programs: "They should be encouraged to concentrate on assisting and training local persons rather than conducting research of their own."<sup>1/</sup>
- (5) The leading social scientists in the country have their share of national and international accolade and therefore have a sense of professional security which enables them to take on whoever deserves to be taken on. There is, at times, even a reverse arrogance.

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<sup>1/</sup> Philippines Daily Express, Nov. 1, 1973. op. cit.

- (6) The external funding agencies have been most sensitive to our sensitivities and have taken very significant steps to improve the climate for collaborative work between foreign and local social scientists. They take into account the recipient countries' interests when research grants are made. The Ford Foundation Southeast Asia Studies Program, for example, uses a Southeast Asian review committee to screen applicants for graduate training, research and teaching fellowships. ILO's World Employment Programme has consultant groups made up of specialists from both the LDC's and the developed countries to assist in formulating their research programs. Ford Foundation and IDRC include respected Asians in their Board of Trustees. On an informal basis, the local social scientists are often consulted by program officers with respect to subject matter, policies and procedures. If all of these fail, we can always "holler" and we do!!
- (7) The relationship of the social scientist to external funding and international development agencies is not one of helpless dependence and one-way agreement. Our priorities seem to have a way of converging perhaps because all of us have profited from our mutual experiences in development. Furthermore we have discovered that we need each other and we can all survive only if we co-exist.
- (8) Some important precedents have been accomplished which mitigate the strain arising from the huge gap between the foreign and the local experts in matters of professional rewards and privileges. In the past, one could not be hired as an expert in his own country. It is the height of irony that he had to go to country X about which he knows nothing in order to qualify as an "expert." At present, at least one Foundation has hired local social scientists as program specialists. The ILO Comprehensive Employment Strategy Mission included four local social scientists and used several consultants. But even more amazing is the willingness of certain international development agencies to pay honoraria or whatever-it-is-called to their local colleagues charged against dollar costs of the project. Although this does not equalize salaries, it goes a long way toward getting there. To illustrate what this means, the budget for a certain project worth more than half a million dollars includes 104 man-months of foreign expert assistance for a three-year period which amounts to 46 per cent of the total budget. On the other hand, the costs of local personnel made up of 507 man-months of senior local consultants and 576 of supporting services add up to only 25 per cent of the budget. When some objection to the payment of local consultants was raised at higher quarters, the evaluation team which assessed the project underscored the fact that the professional staff members of the project

are not without alternatives. Because salaries are low, they are expected to accept consultancies, honoraria, etc., in order to survive. Simply stated, if the project does not pay, it will lose its competent staff. Local costs for personnel therefore undergird the retention of competent staff. Needless to say, the local project leaders have had to fight a tough battle before such arrangements were approved by the international development agencies concerned. The biggest argument in their favor was the realization by the latter that the staff would not be idle without the project. They might be poor but not that poor.

- (9) The experience of the past years has also taught us valuable lessons on how to cope with the visiting expert's occasional negative qualities: his data-exporting predilections, his academic and non-academic arrogance and his latent predisposition to exploit the local situation for his own ends. There are mechanisms for passive resistance, active resistance or no resistance, but no cooperation. In fairness to the visiting social scientists, most everyone has responded admirably to the changing expectations. Perhaps now, we can "live together" as in Grimm's fairy tales "happily ever after."

There are emerging trends, however, which offer even more exciting opportunities for growing together research-wise but with some potential for a resurgence of the "old wound" unless handled with care. At the moment the most attractive strategy for linking with external research communities is the regional or international research network. It is glamorous for a number of reasons:

- (1) In many ways, both subtle and direct, participants learn about each other's country and problems thus contributing to the reality of being an Asian or a Southeast Asian.
- (2) There are both substantive and methodological gains on the part of participants in a cumulative and simultaneously regional way.
- (3) Because of earlier problems with data exporting in many LDC's today, quite often the only way a foreign researcher could test his model in a range of situations is to involve the "natives" themselves in the data-gathering enterprise.
- (4) It provides local researchers an opportunity to associate with some of the established personalities in international development. At the same time, it puts pressure on them to conduct research both in a cooperative as well as competitive fashion thus probably upgrading quality.

- (5) Foreign travel which is built into the operations of the network is not easy to resist.

Four general approaches to the research network are currently in use:

- (1) Getting together social scientists from different countries who independently of each other happen to be working on the same general research problem area. An exchange and sharing of research experiences is enhanced by this network.
- (2) Organizing social scientists from different countries to investigate a research problem which somebody somewhere has conceptualized and designed. A common methodology is frequently imposed and comparability of data is a major consideration. The function of the participants is to provide data from their location which will test the model, so to speak. All the incentives are offered to attract the best possible participants from each country. In some instances, a minimum of comparable information is required and beyond that the researcher has his "degree of freedom" in what he wants to include.
- (3) Defining a research problem with the collaboration of prospective participants who eventually agree on the subject matter of investigation and the "rules of the game" for carrying out and publishing the research results.
- (4) Commissioning researchers from different countries to study a certain problem area defined by an institution or agency. The major concern is the research output. The network is provided by the agency which commissions the research. The participants may or may not meet each other. This is often strictly a contractual job with specified terms of reference agreed upon for a certain price. In delineating what will be researched, the agencies usually consult respected scholars from the LDC's as well as the developed countries.

But even something as seductive as the network has its problems. Because the world is unequally developed, research capacity is also unequally distributed. Sometimes a network has to use the lowest common denominator in order to have a common working base for operations and expectations of output. Networks organized primarily for the purpose of research output could commit valuable research time to something the country may not need but which is carried out because the incentives offered are irresistible to the individual researcher. Furthermore, if being part of a network becomes the sine qua non for approving and funding a research project, then the network might lead to a resurgence of the old accusation on academic imperialism. However, if the research

problem is something of equally high priority to the participating countries, the network could be a mutually reinforcing short-cut approach to finding answers to rural development problems; (except that priorities can be induced, if not seduced). The critical question therefore is: Who decides the subject and object of the research network? In the ultimate analysis, who gains most, the organizers or the participants? There is no doubt that networks have a training value especially when the more junior staffmembers are encouraged to participate in their own capacity. But a network cannot be continuously justified on its training value alone. Sooner or later, a research output will be expected by the organizers, and of course by the funding agency.

Another important problem pertains to the quality of the output. The organizers will understandably be preoccupied with international research standards which are often taken to mean, Western Standards. If this is insisted upon, only the senior, well-established researchers will be invited into the network. Depending upon the research problem being studied, the network could lure the country's already overcommitted top-level talents away from its own priorities. And if the funding agencies decide that network projects have a better "batting average" than an individual project, then the diversion of scarce talent can be potentially serious. Rural development has many location-specific characteristics and therefore the international research network ought to be pursued only as one of the strategies and perhaps not even the most predominant one. Probably a loose network provided as a forum for discussing and learning from each other's research in rural development would be of greater value than a tight and conceptually closed linkage. Finally, a network is only as strong as its weakest link. Therefore unless and until the weakest link is strengthened, a network is only half a network and the strong has built-in advantages over the weak. In the spirit of Filipino optimism, all these need not happen if we have profited from past experience and learned how to mitigate the minuses and to accentuate the pluses in the new venture.

### III. THE DIALOGUE BETWEEN RESEARCH, POLICY AND ACTION

Within the context of rural development, research for intellectual curiosity's sake is neither prestigious nor fundable. This does not mean that such pursuits are non-existent but whatever exists has been most likely proposed and approved under a "development-cover". Even a rural development umbrella, however, is still so encompassing that it is hard to determine what it excludes. In assessing the relationship of development research to policy, four questions seem to be pertinent: Whose development? Which policy?

What research? And how are priorities determined with respect to the first three? At any rate the greatest challenge to the social scientist lies in the creative translation of practical problems into researchable ones which will yield solutions or answers to the practical questions which have been asked by "developers". Actually there is a chicken and egg problem in development research. The latter helps define the magnitude and dimensions of rural development problems but a definition of these problems also lays the boundaries for research. For example, perhaps political sense and social pressure has brought about a land reform program; but having decreed the Emancipation of Tenants from the Bondage of the Soil, there is an urgent need to determine size of holdings and who holds them. In the process of program implementation, it was found that a majority of the landowners are small owners. Having made a previous pronouncement that development of a strong middle class is a desirable national goal, a possible conflict between the stated policy and the reality was immediately perceived. A study is now underway to ascertain who the small landowners are in order to have a basis for changing or pursuing previous policy.

Another more dramatic example is the decision made earlier that water in the new irrigation systems would be distributed on a rotational scheme. Therefore the physical infrastructure was constructed to achieve this end. Given this state of affairs, a research project comparing farmers' organization for water distribution under the traditional continuous irrigation and under the rotational scheme would not be very helpful. As a matter of fact, it will be considered irrelevant. What would be more useful is research on variables which impinge on the effective implementation of the rotational scheme. There are six general types of rural development-oriented research:

- (1) Research which indicates or defines the development problem and suggests what policies might be needed to solve the problem. For example: the Philippines has 52,710 registered nurses, of which 48 per cent are practicing in the country and 35 per cent are working abroad. Of those practicing locally, 73 per cent are within the Greater Manila Area and other cities. In the meantime 33.3 per cent of registered livebirths in 1971 were attended to by local "granny midwives". The Food and Nutrition Research Center survey showed that 27.5 per cent of the population are normal nutrition-wise; 69.7 per cent are undernourished (45.7 per cent suffer from first degree malnutrition; 20.5 per cent, second degree and 3.5 per cent are critically malnourished); 2.8 per cent are overweight.
- (2) Research that arises out of problems associated with implementation of policy such as that cited earlier with respect to land reform.

- (3) Research that is stimulated by policy consequences, e.g., the multitude of studies generated by the "green revolution".
- (4) Action-research which includes intensive studies of particular approaches to rural development, case studies; pilot projects, prototype, "experiments" or social laboratory.
- (5) Evaluation studies of rural development programs.
- (6) Studies which integrate results of researches which focus on particular development problem areas.

Because the Philippines has a proliferation of social science research centers, most of which deal with some aspect of rural development either in a big or a small way, research integration projects are almost indispensable not only for determining what is known and what else needs to be known but also as a "collective" device for reaching decision-makers. Equally important is their potential usefulness to teachers who seldom have access to a wide range of local research-based materials for teaching. What comes handy to them are American books. Since social scientists do not perish if they do not publish, research results tend to remain in typescript, mimeographed form or in graduate student theses, most of which are circulated only in limited numbers, if at all. In the meantime, considerable investments have been made in every research undertaking, reports of which are sometimes known only to the researchers and the funding agencies. The latter may not even have the time nor the inclination to read them. The need for pooling together, codifying, analyzing, integrating and interpreting available data and research results is evident in the priority concerns of the major social science outfits but little along this line has been accomplished.

In order to be more relevant for policy-making and action, the integration of research findings should be done not by discipline but by development problem area, such as: agrarian reform, credit, impact of agricultural innovations, family planning, non-formal education, etc. An annotated bibliography, although informative and useful, is not as valuable for identifying gaps in knowledge and as a basis for research priorities as an actual analysis and synthesis of research findings many of which might be conflicting. Two examples of research integration projects are W.F. Stinner's, "Levels, Trends and Differentials in Philippine Nuptiality, Fertility, and Family Size Ideals and Preferences: An Integrated Summary of Research conducted under USAID Contract" which was done for the Population Institute and "All in a Grain of Rice: A Review of Philippine Studies on the Social and Economic Implications of the New Rice Technology". The latter which was done at Los Baños includes a synthesis of researches in land reform, employment and income distribution, diffusion and adoption, credit, cooperatives and other organizational components, the rice farmer, and agricultural extension services, all of which have some relation to rice.

Despite the demand for it, the research integration job is not a glamorous one. It is a long and tedious process and no matter how creatively one goes about his task, his study is viewed as a mere compilation of other people's work. The attraction is still toward the opportunity to do "original" research projects, the whole of which falls under one's own byline. Moreover when other researchers cite data from this integrative piece, they only mention the original source, and rarely, if ever, do they find the need to footnote this secondary source which has brought the hard-to-obtain research results to their doorstep.

Despite frustrating attempts to secure support for this kind of work, an occasional bright spot appears in the latent interest which one or two funding agencies have shown. What is curious, however, is that one agency might support the research integrator to do the job but will not bother about its publication. Another agency would argue that they cannot support such a project because they are not smart enough to predict how good a job the person can accomplish. But as soon as the manuscript is completed, they are eager to pay for its reproduction. Still another agency would support a research integration project because they are interested only in using the research integrator to bring together many fugitive materials which they themselves could never get. He is even made to understand that they have no obligation to publish the manuscript; neither is the research integrator free to use the materials after they have been submitted. Somebody else would use these materials which will be gathered by the research integrator. Fortunately, contact with more than one funding agency provides some alternatives. One does not have to do what he does not want to do.

In the literature of development, one of the most used, if not abused, words is priority. Development research is no exception to this. Objectively speaking, there are at least four types of priorities for development research:

- (1) Systematically determined priorities based on an analysis of rural development problems in the light of what is considered desirable as national goals. Coupled with this is a synthesis of past and current research findings which will point out what the necessary "unknowns" are.
- (2) Natural inclination priorities (a euphemism for vested interests) dictated by the importance of whatever is germane to one's particular field of study or apropos to one's professional advancement.
- (3) Opportunistic priorities determined by the very pragmatic consideration embodied in "Tell me what your angle is as a funding agency, and I'll come through with an appropriate proposal." Officials of funding agencies wittingly

or unwittingly provoke this type of priority.

- (4) Induced priorities set by someone who possesses knowledge or money or both and exercises persuasive powers over researchers. This is not at all bad if he happens to be very wise and very rich!!!

Because development research is supposed to contribute to the development process, the nature of the links between research and policy is always a pertinent question. In the Philippines these links, whether tight or loose, take several forms:

- (1) Personal, informal and/or professional interaction, advising or consulting which takes place on an ad hoc basis between researcher and policy maker.
- (2) Seminars, workshops, conferences organized by development agencies to which social scientists are invited to present papers, serve as resource persons, etc.
- (3) Research commissioned by development agencies. Hopefully, research results are at least read, if not used.
- (4) Dialogues between social scientists, policy makers and program administrators at the latter's invitation.
- (5) Social scientists themselves become policy makers or perform two roles concurrently.
- (6) Training programs for action workers and development administrators where research results are used as training materials.
- (7) Social science graduate courses where students come from rural development agencies. Upon return to their respective agencies, they almost always occupy key positions.
- (8) Social science publications. Because they are quite often addressed to other social scientists they may only be of marginal utility to the policy maker.
- (9) Social scientists working in development agencies in a research and evaluation role.
- (10) Research communicated via an intermediary. Where there is a residual colonial mentality in the policy maker, the researcher passes on his research findings to the foreign expert who then gives his advice with or without mentioning the source. It is not uncommon, however, for the foreigner to build up the local researcher in the eyes of the policy maker by putting in a good word for the research he has done.

One or a combination of several of these ways of reaching policy-makers and developers may be found in the different institutions engaged in development research. These avenues are utilized not only for domestic but also for regional or international linkages. Some of these methods have more direct and more immediate impact on policy than others but formality of arrangement is no guarantee of effect. The "whispering" linkage, depending upon who is whispering to whom, works wonders in many ways.

Two examples of study series which have been the basis for major development policies are those on population and education. The former have been responsible for generating much of the basic facts and figures without which it would not have been possible to argue that we have a population problem. A series of conferences on the subject provided a forum for academicians, practitioners, and policy makers including church authorities. With intensive cooperation from the media, all the research papers have contributed immensely to the definition of the population problem and its implications. They helped objectify the issues involved and to banish the undercover and sensitive nature of the proposed solutions to the problem. Eventually a population policy was arrived at and a population program was evolved. In the case of the studies conducted by the Presidential Commission to Survey Education, their recommendations on the basis of survey findings were adopted for implementation almost in toto via a Presidential Decree.

Development policy, by the way, is more readily amenable to influence by research results because only a few key decision-makers need to understand and be convinced for a policy to materialize. Furthermore, development policy usually comes in an oral declaration and/or a written document which serves as the framework for succeeding actions. What deserves as much, if not more, research investment is the dynamics of getting things done, the process of implementing whatever policies have been adopted. It is in this area where the services of the Filipino social scientist seem to be in greater demand but it is probably in this area also where he is most ill-equipped, research-wise. He is asked to participate in programs for rural health, credit, cooperatives, irrigation, agricultural innovations, family planning, agricultural education, applied nutrition, employment, rural-urban migration, regional development, etc. The extent to which the policy maker or program administrator takes into account the research results as interpreted to him by the social scientist is difficult to determine. A most optimistic assessment would be to say that he is almost always consulted on any major development program. For this task, action research has much to offer to the researcher's repertoire of insights on the development process. Unlike the usual research project, action-research, as the name implies, combines research and action. It documents and analyzes the process by which a particular rural development strategy is introduced, accepted, rejected or modified

and its impact on the object of development. Dozens of projects can tell us the before and after circumstances of a development situation but what takes place in between is rarely studied. In other words we need to know much more about the nature of the "stimulus" or the strategy itself which brings about change and if it does not, why not.

With an action-research project, the research input required to satisfy project objectives is substantial, intensive, dynamic, and continuous. Let us take the case of a multiple cropping project which has the following objectives, all of which call for a major social research investment:

- (1) To develop a pattern of introducing multiple cropping to farmers for adoption.
- (2) To study the processes involved in the introduction and adoption of multiple cropping.
- (3) To identify the technological, educational, sociological and economic problems in the introduction and adoption of multiple cropping.
- (4) To determine the economic and social development impact of multiple cropping.<sup>1/</sup>

This expected pattern of introducing multiple cropping is really a kind of social technology which is an integral part of the agronomic package which makes up multiple cropping. Without the necessary research input, only the direct participants in the project will learn from it and therefore the whole *raison d'être* for the pilot project is defeated and lessons gained from the multiple cropping strategy cannot be effectively shared.

Action-research by its very nature leads to a lot of role confusions and conflicts because research action and evaluation are simultaneously carried out.<sup>2/</sup> The situation is complicated

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<sup>1/</sup> Arturo A. Gomez, "Introduction and Impact of Multiple Cropping in Selected Communities in the Philippines," (Paper prepared for a Seminar on Multiple Cropping Diversification in Taiwan and Its Relevance to Southeast Asian Countries) held in Taiwan, Oct. 29 to Nov. 4, 1973.

<sup>2/</sup> G.T. Castillo, "Man With Many Faces: The Researcher in a Program of Planned Change," A/D/C Paper No. 1, April 1964, Agricultural Development Council, Inc. N.Y. and "Research and the Action Program," (Paper on Survey Research Methodology), The Agricultural Development Council, Singapore and New York, Dec. 1972.

even more if a university administrator also happens to be project leader, researcher and evaluator at the same time. He will find it difficult to separate an administrative report, which justifies project support, from the research report, which documents and analyzes the various dimensions of the action program which is the object of study. For example, a report of accomplishments which includes the number of visitors and VIP's who came to see and were impressed by the project is a public relations message, not a research report. To illustrate further, if an action-research project were to report the case of 80 small farmers who were able to obtain loans totalling ₱150,000 under an integrated farm-financing program, this would not be a research accomplishment. The object of the research is to analyze the process by which farmers came to borrow from the bank for multi-production purposes. There are at least three social phenomena worth investigating in this regard: (1) banks do not usually lend to small farmers without collateral, etc. (2) small farmers in the area are usually oriented only toward rice, and (3) the emergence of an integrated farm production and financing scheme is a very modern phenomenon which entails a great deal of risk and sophistication. Certain social mechanisms must have been introduced to make this development possible. An identification, description and analysis of such mechanisms would be a research achievement.<sup>1/</sup> The problems of combined roles in one project are not insurmountable, but the research role has to be exercised with the utmost professionalism. Despite inherent difficulties, these types of projects are invaluable in the study of rural development strategies and the intricacies of the development process. One other problem in carrying out such research is the relatively long-term commitment (at least 3 to 5 years) which is required. Within this period the project is subject to personnel turnover, natural calamities, major political changes and competing alternatives for the researcher's time. These drawbacks, however, can be assets in the sense that the project has a chance to respond to natural events which normally occur in the lifespan of a development program.

Having discussed the myriad ramifications of development research and its actual and potential links with policy, an attempt will be made to concretize some of the generalities by describing briefly six different settings for rural development research. Four were chosen for their major involvement in this type of work; one for the unique way in which research is combined with training and other functions; and a sixth one, for both a direct research role and an indirect role via the training of potential development researchers.

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<sup>1/</sup>This illustration was stimulated by a discussion with Federico Cruz, farm management technician of the UPLB-SEARCA Social Laboratory at Pila, Laguna, December 19, 1973.

## (1) The University of the Philippines at Los Baños (UPLB)

Besides the fact that 13 social science units (See Appendix) are located in this community, the mandate of the UPLB for involvement in rural development is spelled out in the provisions of Presidential Decree No. 58 which granted it full and complete autonomy, excerpts from which are cited:

"Whereas, the Government is strongly committed to the proposition that agricultural and rural development should be achieved as a foundation for industrialization and social and economic progress;

Whereas, the entire country has been proclaimed a land reform area in order to emancipate tenant farmers from the bondage of landlordism as a prerequisite to the development of a strong and viable economy;

Whereas, in order to effectively implement the land reform proclamation, there is urgent need for a highly competent center of training to turn out the needed manpower, and to undertake relevant research and extension services in agriculture, agrarian reform, agricultural engineering, agribusiness, agricultural credit and cooperatives, forestry and related sciences and technologies;

Whereas, it is necessary to establish an agricultural center that will effectively mobilize and totally, realistically and directly apply its academic and technical expertise and physical resources to achieve the purpose of the New Society."

In order to respond more effectively to this mandate, the University was reorganized to include a University Research Council which shall develop an integrated research program and promote multidisciplinary collaboration in the solution of developmental problems.<sup>1/</sup> A University Extension Center was also created. It is believed that "the new extension set-up will be more effective in attaining the University's ultimate aim of developing the Filipino into a self-reliant, resourceful, productive, and well-adjusted citizen who will contribute more responsibly and effectively in national development;...to prepare and disseminate research findings for application to relevant social action." The Agrarian Reform Institute which is barely two years old regards itself as a "dynamic agent of change" for it has a "mass orientation." Its functions are to undertake studies on agrarian reform and its administration and recommend to the Department of Agrarian Reform plans and programs for approval and integration into its operation as a result of these

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<sup>1/</sup>UPLB Horizon, Vol. I, No. 1, September, 1973.

researches; evaluate programs; provide professional assistance to pilot land reform projects and areas and to operate as a center for exchange of information on agrarian reform.<sup>1/</sup>

Number of Published and Unpublished Studies\* by Unit/Department  
at the University of the Philippines at Los Baños, 1961-1972

Unit/Dept.	1961-1965		1966-1972		Total	
	Pub- lished	Unpub- lished	Pub- lished	Unpub- lished	Pub- lished	Unpub- lished
1. Ag. Credit and Coopera- tive Institute	2	5	-	2	2	7
2. Community Development Center	-	14	-	-	-	14
3. Dairy Training and Re- search Institute	-	-	3	28	3	28
4. IRRI-Ag. Econ.	14	12	40	63	54	75
5. Dept. of Agricultural Communications	-	14	5	32	5	46
6. Dept. of Agriucltural Economics	15	46	69	170	84	216
7. Dept. of Agricultural Education	23	42	46	71	69	113
8. Farm and Home Development Office	6	34	13	30	19	64
9. Home Technology	3	9	2	11	5	20
10. College of Forestry	6	12	2	8	8	20
Total	69	188	180	415	249	603

\*Includes only those classified as social in nature.

Source: Report of the Committee On Development Studies, U.P. at  
Los Banos, June 30, 1972.

<sup>1/</sup>UPLB Horizon, Vol. I, No. 3, Nov. 1973.

Although UPLB has just been reorganized, its social research activities date back to pre-war days. However, research productivity increased considerably within the past 10 years (1961-1972) as the accompanying table reveals. The Department of Agricultural Economics tops the list of both published and unpublished materials. The International Rice Research Institute which is noted for its biological science research is likewise very productive in agricultural economics. The Department of Agricultural Education has a wider range of rural development research interest than the name of the department suggests, although almost 60 per cent of its published work belongs to one staff member. The lag between the unpublished and published materials stems from four reasons: (1) Quite a few of them are undergraduate and graduate theses, the authors of which rarely bother to prepare them for publication. (2) Overcommitment of faculty members leaves them little time to polish a research report for publication because a mimeographed version is already in demand for immediate use. They therefore move on to the next report in order to meet the next deadline. (3) The usefulness of research as a basis for expertise in relating to policy makers and development programs does not depend on whether or not the results are published. As soon as the research is completed, the findings are already known to the researcher and therefore he could already share it with someone who is interested. Quite often development agencies cannot afford to wait for the final printed version which almost always takes a long time. As a matter of fact, the preparation of a research paper for a journal article could serve as a handicap to its usefulness for policy because academic jargon gets in the way of comprehension. Ideally, a researcher should be able to write two versions of the same research report - one for academia, and one for developers. Given the high incidence of unpublished research, the earlier argument for research integration becomes even more apropos. (4) Funds for research are often easier to obtain than funds for publication.

Perhaps it is no exaggeration to say that UPLB by the very nature of its mandate, resources, and facilities, is more equipped to engage in action-research than any other institution in the country. Among its many projects along this line are: studies on farm and home development, alternative extension approaches, cooperative approach to rural development, social laboratory, barrio development school, out-of-school youth training program, adult farmer education, nursery school, agro-forestry development, intensified livestock and poultry production, multiple cropping, intensified corn production program, Bicol River Basin Development Program, farmers' organizations, etc. These projects, some of which started as early as the 1950's, are a special feature of the UPLB involvement in rural development research. They have been essential and instrumental for the development orientation and socialization not only of the social scientists but the physical and biological scientists as well. Without them these academicians

would still be teaching and conceptualizing rural phenomena on the basis of old notes from New York State, Wisconsin, etc. Furthermore, they have been effective in helping everyone come down from the ivory tower to the ground. Now, it is the "in" place to be (on the ground).

(2) The Development Academy of the Philippines (DAP)

The Development Academy which was established in May, 1973, is a joint project of the Central Bank, the Social Security System, the Government Service Insurance System, the Development Bank of the Philippines, the Philippine National Bank and the National Economic Development Authority. It has an initial endowment fund of ₱15 million and initial annual operational funds of ₱500,000 from each of the five banking and insurance systems. The professional staff consists of 16 Fellows from different social science disciplines and its president. DAP's information kit describes itself as a "center of learning, a place where the best minds of the country can meet, exchange and generate new ideas and formulate ways of translating these abstract ideas to concrete action. It is the school for the country's decision-makers and the welders of the country's economy and ultimately its destiny. The Academy is designed for versatility wedded to purpose. Versatility consists of the Academy's undertaking a rich diversity of programs responding to a range of specific tasks. Purpose lies in servicing of development which invests these programs with direction and relevance. The DAP's total commitment is to development which in its view consists of strengthening the nation's capacity to produce more wealth, the efficient use of this capacity and the distribution of wealth for the people's welfare." Its program consists of research, training and a regional role. The research program is designed toward an understanding of development programs and the outputs from research projects would provide background materials which can be used for training. It aims to have a continuing dialogue with the academic community, leaders of finance, industry and agriculture. The current research program includes nine major projects:

- (1) Medium and small-scale industry coordinated action program which expects to prepare 100 industry profiles. An integrated system approach is being applied.
- (2) Development policy conference to assess sectoral performance vis-a-vis sectoral targets as specified in development plans. In-depth studies will be utilized for this purpose.
- (3) Project management information system using a conceptual systems model for an integrated area development model.
- (4) National Electrification Administration - National Irrigation Administration and DAP Irrigation Program, the aim of which is

to increase the production of rice and other crops of farmers in the project area by providing irrigation and other complementary agricultural services. It seeks to develop a model upon which all other pump irrigation projects in the country may be based.

- (5) Research information resource library which aims to effectively and efficiently provide the critical information needs of specific development agencies. It will establish a national clearing house of development information servicing both local and foreign institutions.
- (6) Intermediate technology for rural transformation will identify the relevant sources of information, both local and international, which can be tapped for the utilization of appropriate technology suited to the needs of rural areas. As they put it: "While policy measures on a macro-level emphasize labor-utilizing strategies, their translation into effective programs on the project level particularly for the rural areas is sorely lacking. There is need for a deliberately planned strategy of technological diffusion and innovation especially for its demonstration effect value.
- (7) Social indicators project includes a comprehensive review of the literature on social indicators, defining those relevant to policy planning in the Philippine context and the operationalization of suitable development indicators on a provincial and regional level for government programs regarding health, income and consumption, employment, wealth and productive capital, environment, public safety and practice, political and social opportunity.
- (8) Regional development project seeks to assess the various efforts and plans aimed at regional development through an in-depth comparative analysis of their individual development strategies.
- (9) Task Force on Human Settlements includes survey analysis, evaluation of all existing plans, reports, studies involving human settlements followed by a conceptualization and development of models designed to integrate the discovered components of an ideal human settlement. Four basic studies on population for the Manila Bay area, industry and commerce, environment and housing are being undertaken. The expected overall output is a set of policy and program recommendations backed up by strategies for implementation.

Because the Academy is very young, it is too early to judge how successful they will be in realizing the outputs expected from their research program. Their Fellows are all bright, young, enterprising and committed to the goals they have set for the Academy.

Even if for this reason alone, the place bears watching especially because it is an all-Filipino enterprise both in expertise and in financing.

(3) The Philippine Business for Social Progress (PBSP)

On December 16, 1970 representatives of 50 business companies created the PBSP, to which corporate members contribute annually one per cent of net profits before taxes. Sixty per cent of the contributions are handled directly by PBSP and 40 per cent by the member company for any similar program of their own. As of March 31, 1972 a total of about ₱6 million have been contributed by 148 members for 51 social development projects all over the country which include applied nutrition, social condominium housing, irrigation, small-scale industry, cooperative training, swine breeding, integrated rural development, land reform, fisheries, skills training, urban resettlement, credit, cottage garment industry, small business management, rehabilitation program for the disabled and blind, water power development, urban social development, social laboratory, action information center, etc. From their statement of the expected returns from this program one sees both enlightened self-interest as well as a social conscience expressed in the social responsibility of the business community to Philippine socio-economic development. The latter is to be achieved by "mobilizing the scattered resources and efforts of private enterprise in social action into a unified social development program to be administered in a scientific manner." The PBSP approach commits not only funds but also the managerial and technological resources of the business sector. In terms of enlightened self-interest, "membership in the PBSP is a long-term investment in solid economic progress because such a program will further stimulate production by creating a greater purchasing capacity among the needy, thus broadening the market to an ultimate mass market for consumer goods. It will also ensure the development of human resources which will supply industries with skilled manpower, who will be more productive because they are highly motivated. Contribution to the PBSP can be viewed as a kind of self-insurance because it is a contribution to social peace."

The unique feature of PBSP-supported action projects is the research and evaluation which goes with every project. As they describe the rationale for this approach: "Despite the years of competent and dedicated work by practitioners in social development in the Philippines, there remains a serious deficiency of scientific documentation and validation of this work. For lack of data, and of communication and organization, practitioners continue to waste resources on methods or systems that have been tried and found inadequate in one way or another but never reported or evaluated. PBSP's research and development approach hopes to increase the fund of scientific data on social development by reporting and documenting each of their projects in a systematic manner." From this approach

they expect to emerge with an "array of social development approaches for future use and reference, all carefully evaluated and documented, each with its own identified special strengths and specific uses, as well as weak spots and inadequacies. So that all social development institutions, public and private, in the Philippines and in neighboring Asian countries would have a range of experience in the utilization of tools and techniques which may be considered for application to the many social development situations that may similarly arise."

To implement their aim of arriving at a documented social technology, PBSP has a small research and evaluation unit, and a research advisory group made up of social scientists from different disciplines. Major research undertakings are contracted to academic and commercial research centers. About three to five per cent of project costs is allocated for this purpose. One bottleneck which PBSP has encountered in the pursuit of their research objective is once again their inability to find enough competent and experienced researchers who could meet the considerable and difficult demands of action-research. The businessmen's willingness to support these research and evaluation ventures is admirable but their research output expectations will have to be satisfied sooner or later.

(4) The Philippine Council for Agricultural Research (PCAR)

Having defined "the weaknesses of agricultural research in the Philippines as rooted in the lack of planning and coordination at the national level" <sup>1/</sup>, the Executive Panel to Develop a National Agricultural Research System recommended the creation of PCAR which became a reality on November 10, 1972 through Presidential Decree No. 48. The PCAR is designed primarily "to provide a systematic approach to the planning, coordination, direction and conduct of the national agricultural research program." Among its objectives are to define the goals, purpose and scope of research in agriculture, forestry and fisheries; to develop a national research program based on a multidisciplinary, interagency and systems approach; to establish a system of research priorities; to develop full communication among workers in research, extension education and national development; to initiate the establishment of a repository of research information and to provide for appropriate incentives to keep competent research scientists in the system. All government revenues earmarked for research in agriculture, forestry and fisheries, whether coming from regular budget appropriations or tax levies on specific commodities or from the National Science Development Board, shall be programmed by PCAR. Beginning July 1, 1973 no funds earmarked for

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<sup>1/</sup> Romeo A. Obordo, "The PCAR: A New Direction in Agricultural Research Management in the Philippines," Paper prepared for the Seminar-Workshop on Institutionalizing Research Management in Asia, College, Laguna, December 10-19, 1973.

agricultural research shall be released by the Budget Commission or other funding agencies for research activities in PCAR research centers and stations, universities, colleges and other research agencies unless these are integrated as a part of the national agricultural research program. For the Fiscal Year 1973-74, a total of ₱53.8 million was earmarked for agricultural research.

PCAR has six divisions: crops, fisheries, forestry, livestock, soil and water and socio-economics. There are 29 commodity research teams with specializations in the different aspects of each commodity, including a rural sociologist and an agricultural economist. The Socio-Economics Division has two research teams - one in applied rural sociology and another one in macro-economics. The members of the first team represent agricultural education, community development, extension, agrarian reform, communication, agri-business, sociology, economics, credit and cooperatives and development sociology. The second team on macro-economics represents prices, resource economics, agrarian reform, statistics, development economics, credit and cooperatives, sociology, employment, business marketing and trade.

After two workshops held separately by the two socio-economics research teams, the major research areas considered as priority for applied rural sociology are: benchmark studies, evaluation studies, agricultural education, patterns of rural development, socio-cultural studies, agrarian reform studies, agri-business studies, credit, cooperatives, and farmers' organization and communication studies. Within each research area are more specific projects.

The Macro-Economics Research Workshop included administrators and policy makers in key positions who presented their views on current government programs and priorities. Subsequent discussions yielded nine priority research areas which constitute the national program for macro-economics research: improvement of the data base for macro-economic research pertinent to agriculture; desirable future resource allocation among agricultural commodities; design and implementation of rural credit schemes; design and implementation of land reform policy; the role of rural cooperatives in development; present and potential effects of government intervention on markets for agricultural products and inputs; policies for the improvement of employment and labor incomes in the rural sector; policies regarding agricultural taxation; and broad rural development strategy. 1/

Both teams regarded research to strengthen the data base for planning and policy formation as indispensable. The need for data on resources not only at the national but also at the regional and provincial and even lower levels is recognized. They also agreed

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1/ First National Agriculture System Research Congress, Socio-Economics Research Division, Workshop Session No. 18, Applied Rural Sociology, University of the Philippines at Los Baños, Feb. 12-17, 1973 and PCAR Monitor, Vol. I, No. 6, November 1973.

that agrarian reform is an important field of study for both sociology and economics. For the year 1973-74, these two teams have about ₱2 million for research. Although some research projects have been approved for funding, the Research Director of the Socio-Economics Division thinks that efforts should be directed toward stimulating more research proposals within the priority areas. Shortage of social science research manpower appears to be a problem again.

With PCAR barely a year old, some questions about its powers have been raised. As someone from a major government research agency put it: "PCAR is now an ubiquitous influence in current agricultural research activities ... Besides government appropriations, the source of funds for PCAR operations includes commodity tax levies, special funds, foreign aid and private sector contributions. These funds are to be programmed by PCAR alone ... While the PCAR is setting up its mechanism in order to establish a comprehensive and relevant national research program, there must be not only an evaluation of research projects in the different agricultural organizations but also a frank self-evaluation of PCAR at this stage to ascertain the steps to be taken to improve its potential, particularly in establishing a direct line of authority that is devoid of operational complexities. Perhaps it is not improper to suggest that PCAR evaluation be conducted by a group of academicians with expertise in administration organization. For only then can the financial resources be made to flow efficiently, smoothly, and continuously without unnecessary constraint on the conduct of agricultural research." 1/

The beneficial effects of centralized research direction via the power to withhold fund releases unless project proposals are approved by a technical review panel can come in the form of pressures on the researchers and research agencies to really do significant research instead of simply spending research money. However, to be charged with the task of defining and directing an entire national research program is no mean job. It requires infinite wisdom, tremendous responsibility, and willingness to take risks.

There are two types of risks which funding agencies and project evaluators have to take with respect to research proposals. The first refers to the risk of funding a project which may not be worthwhile. The second refers to the risk of rejecting a project which may have significant results. Since research is a creative enterprise with a great deal of "unknowns", the second type of risk must be avoided or kept at a minimum. This allows for the possible lack of perfect knowledge on the part of project evaluators and gives the benefit of the doubt to the researcher. For who could predict exactly what

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1/ J. C. Alix, "Research Management in Agriculture: The Philippines," (Position paper prepared for the SEARCA Seminar-Workshop on Institutionalizing Research Management in Asia, College, Laguna, December 10-19, 1973). Mr. Alix is Assistant Director of the Bureau of Agricultural Economics.

results a research project would yield? Sometimes the questions which project evaluators of funding agencies (whether local or external) ask of a project proponent are such that if the latter knew the answers, he would not need to do the research. A curious situation exists at the moment wherein money may be available and waiting to be tapped somewhere while important research projects may remain unfunded. This situation can be traced to lack of sophistication in the "art of grantmanship"; inability or unwillingness to go through the bureaucratic requirements and procedures in preparing a proposal, submitting it for review and then waiting forever for the response; indifference of funding agencies to small projects; and availability of alternative financial sources for competent researchers. Hopefully, access to these alternatives will be kept open and not "captured" for the sake of coordination and national programming. Otherwise, it would be difficult to keep competent researchers and bureaucratic complexities in the same system.

(5) The U.P. Insititute of Mass Communications (IMC) Family Planning Communication Project

This project is executed by IMC, sponsored and directed by the Population Commission and assisted financially and techr cally by UNESCO and UNFPA. The first two years were devoted to a collation of Philippine and Asian data relevant to population and family planning; analysis of survey, experiment and other types of research data to generate guidelines for the development of prototype communication materials; synthesis of data in accordance with training goals and needs; development of research designs to evolve innovative approaches in family planning communication; pretesting of prototype communication materials; conducting of laboratory and field experiments to test communication approaches and evaluation of the project's training activities. In this project, research and evaluation occuppies a very central role in the sense that family planning communication materials are developed on the basis of research. Training curricula and training materials also have their roots in research. The fusion of these three components - research, materials development, and training - owes as much to creative organizational leadership of IMC as it does to the effective demands placed on the Institute by policy-makers and promoters of family planning. Although three working committees have been set up to plan and implement these three types of activities, staff members contribute to two or three of these functions with greater time spent on one component in which they specialize.

Among the by-products of this project are: field-tested mass media materials such as leaflets, posters, etc.; research-based guidelines for content and presentation of such materials; case studies and analyses of family planning communication strategies; training materials produced directly from research findings using both primary and secondary data; and reports on research experiences with a focus on the methodological problems and adaptations made on the basis of field work.

What is significant about this project is not just the fusion of research, development of materials and training, but also the multiple agency (local and foreign) involvement in it. The IMC is an academic institution, while the Population Commission is responsible for developing and coordinating the country's population program. Technical assistance comes from UNESCO, but funds are provided by UNFPA. Whatever by-products are generated from the project are immediately used in several ways for the family planning communication programs of different agencies. This whole approach is also being introduced in other countries and the experiences gained are being shared. The transferability of the experience to other development communication projects such as in agrarian reform, introduction of agricultural innovations, nutrition education, etc., is being explored.

(6) The Philippine Social Science Council (PSSC)

The PSSC is a "non-stock, non-profit, private organization which intends above all to consolidate the resources of local social scientists by providing the leadership likely to enhance their impact on the development of the nation and to formulate appropriate policies, programs and projects to achieve this end." One of its functions is "to develop, stimulate, support or encourage social science research projects of a cooperative and multi-disciplinary nature that tend to meet national problems especially those which will illuminate both the basic and applied aspects." It has seven social science association members, and the Executive Board represents 12 disciplines of demography, anthropology, statistics, history, sociology, political science, public administration, economics, mass communication, linguistics and social work. Three of its programs have implications for rural development research. These are the (1) research integration program for researches intended to analyze and synthesize existing social studies with the end in view of pointing out important gaps and provide directions for further research. These studies may be on land reform, internal migration, impact of new agricultural technology, etc. (2) the major social science development-oriented research and (3) the Summer Training Program in survey research methodology which in 1973 has involved junior staff members from 15 universities located outside Greater Manila. PSSC hopes to see a federation of university-based research centers competent enough to undertake professionally acceptable studies for themselves and for contracting agencies. So far, the first two programs have not as yet made any grant for a research project directly related to rural development, but at least the intention is there. Just like the other social science councils, PSSC would like to receive more proposals for these two programs. For the present their financial support comes from NSDB, the Asia Foundation, Ford Foundation, and the Fund for Assistance to Private Education.

An examination of the avowed reasons for the existence of the different social centers, councils, etc., leads one to the conclusion that: everyone wants to be multidisciplinary, inter-agency, integrated, coordinated, programmed systems-oriented and relevant to national development. The concept of pilot project, prototype, and model is also evident among those engaged in action-research. PSSC is organized on the basis of disciplines, while PCAR focuses on commodities with each commodity research team including a social science component. It is interesting to speculate on which of these two approaches could lead to research which yields results more relevant to policy. The overwhelming concern for multidisciplinary research might be misplaced for, if one were to focus on the problem rather than on the academic discipline, every development problem is by its nature a multidisciplinary phenomenon. After all, the boundary between academic disciplines is an artificial one, created by academicians and imposed on the phenomena we are looking at. Many so-called multidisciplinary research projects are in reality simultaneous research problems within the same research project pursued parallel to each other, and they may or may not meet within the covers of one research report. As somebody said: "the best type of interdisciplinary thinking is one which takes place within the same skull."

In real life, for instance, the dichotomy between technical and social factors is less of a dichotomy and more of a unity. Every physical or biological piece of technology such as irrigation, new rice seeds, power tiller, etc., is developed within an organizational framework and introduced into a socio-cultural physical environment. The research which led to the development of new rice seeds is a social activity carried out within a particular social setting. The social organization in which they were developed is as much a part of the new seeds as its eating quality or tillering capacity. Although little has been said about it, there was some form of social technology which accompanied the introduction of the new rice seeds. Two examples of this social technology are: (a) Changing the change agent, a system whereby the extension workers were asked to go through the entire process of rice production from land preparation to harvesting. This not only provided them the necessary knowledge and skills and the self-confidence needed to enable them to face experienced farmers, but it also helped to develop a new reference group in terms of work norms; (b) Applied research plots in farmers' fields introduced the concept of experimentation to the farmers while at the same time reducing the extension worker's risk of making the wrong recommendation as to which combination of seeds, fertilizers, etc., would work best in the farmers' particular situations. Risk reduction measures were in fact taken at all levels by all participants in the rice production program - the researcher, and the national officials included.

The development-oriented social scientist is therefore a special breed of researcher. He has to be interdisciplinary within himself. He must comprehend and appreciate the basic ABC's of rice production if the subject matter of research is the impact of new rice technology. Otherwise, his social science training could lead him to the wrong conclusions. Unwillingness to apply fertilizer could be interpreted as traditionalism when in fact, it is rational non-adoption because of the nature of the rice variety being used and the soil in which it is planted. Agronomists, for example, who have been working on the development of improved cultural practices for the production of coconuts, abaca, etc., come to the social scientist to ask: "Who is the coconut farmer? Who is the abaca farmer?" If they are to introduce innovations in their production systems, they need to know who their clientele is. They also want to know how many such farmers there are, where they are and what kind of people will benefit from whatever new agricultural technology they develop. The social scientist can respond much better to these questions if he knows enough about coconut production, abaca production, etc.

Finally, the use of research results as a basis for deciding policy is not without its hazards on both sides. It is amazing how much of our analysis and definition of development problems is dependent on the statistics that we use. When the data base is in question, whether in terms of methodology or integrity of the statistical system, one cannot escape bedeviling doubts as to whether the development situation is what it is in actual fact or whether it is just the statistics which have made it so. For example, the Bureau of Census and Statistics reports the 1970 population figure as 36,684,486. The Population Institute disagrees and says that the total is 37.8 million: The difference of more than a million represents only 3 per cent of the BCS figures, but 1,115,524 Filipinos means a difference in our computed rate of population growth which in turn affects family planning targets, food production requirements, etc. Data on rice production are another source of policy headaches. As Crisostomo and Barker cited: "The estimates of the Bureau of Agricultural Economics are frequently criticized by policy makers and others who have attempted to use these as a basis for determining for a given year the level of national self-sufficiency and import and export requirements. The futility of this exercise is discussed in Mahar Mangahas, Efficient Forecasting and Philippine Rice Import/Export Policy (Paper presented at the Seminar on Consumption and Marketing of Rice in the Philippines, IRRI, December 5-6, 1969.)" On the other hand, they say that: "A separate set of government statistics on rice released by the National Food and Agriculture Council is at variance with these findings. In October 1971, they reported that the yields of high-yielding varieties continued to average about double those of the traditional varieties. These estimates are not based upon normal statistical sampling procedures. Furthermore, if these estimates were accurate, it would follow that the Philippines would be exporting large surpluses of

rice instead of importing in 1971. We have thus chosen to ignore the NFAC figures in our analysis." <sup>1/</sup>

On the other side of the coin, a well-known social scientist once said: "Sometimes the policy maker uses research the way a drunkard uses a lamp post, for support rather than for illumination."

#### IV. SOME SUBSTANTIVE ISSUES FOR RESEARCH AND REEXAMINATION

Extensive exposure to rural development in the Philippines tells us that certain research approaches deserve to be applied more and certain substantive areas have either been ignored or insufficiently noticed. Then, too, one acquires misgivings about the assumptions we make in rural development and nagging second thoughts disturb the professional and social conscience. There are at least nine issues for consideration:

(1) The need for a planned complementarity between the micro and macro level research and a more problem-focused research orientation which fuses rather than bifurcates the physical, biological and social aspects of a development problem. An excellent illustration of what this means is provided by Sodusta's research on Manpower and Wet Rice Cultivation in Jamoyawon, Siargao Island, Northeastern Mindanao, 1973.

One of the fears generated by the advent of the new rice technology is that labor-saving devices would come into greater use and human labor would be displaced. When we reckon the size of our labor force in aggregate terms, we arrive at the conclusion that we are a labor-surplus economy. Setting aside this macro-level view, Sodusta proceeded to examine in depth the manpower situation in rice production as it is practiced in a very poor community. Using intensive time and motion study for a close-up view of actual work performance, she concluded that labor resources for rice production is inadequate due to the shortage of "qualified" skilled workers. Three major reasons account for this shortage: (1) a high percentage of children among the village population; (2) available labor force is engaged in a variety of household and other non-productive activities but which are essential to everyday life; and (3) majority of the labor force are undernourished or malnourished. Because life must go on for the peasant, adjustments to the labor and energy shortage in rice production are made in the following manner: (1) four or five carabaos are used for land preparation but of all the activities engaged in by the peasant, caring for the carabao occupied most of his time; (2) the different stages in land preparation are combined, shortened, or omitted in order to save time and labor; and (3) to cope with the energy requirement in land preparation considering calorie-protein deficiency, the peasant spends much time sitting idly between

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<sup>1/</sup> C. Crisostomo and R. Barker, "Growth in Philippine Agricultural and Rice Productivity: The Impact of the HYVs", Saturday Seminar, Agricultural Economics Department, IRRI, November 27, 1971.

tillage activities and taking a nap or resting. After this intensive time and motion analysis, Sodusta expressed misgivings about the present preoccupation with multiple-cropping as an answer to what is defined aggregatively as a labor surplus problem. As she suggests: "the numerical size of the household is a poor indicator of the strength of the household." In the same vein, the size of the labor force is an inadequate indicator of effective manpower for production. We may have a labor surplus but human energy shortage.

(2) Equal research attention to the structural-fiscal measures for income distribution and employment as well as to the traditional cultural means for bringing these about.

Rural communities have traditional practices for work sharing, harvest distribution, income transfers and social levelling. About 20 per cent of Filipino households depend on family assistance, gifts, etc., as one source of income. With more efficient and effective tax measures, a higher percentage of family income will be transferred to the bureaucracy which, in turn, will redistribute these via social security, old age pension, scholarships, medicare, funeral benefits, etc. There is nothing wrong with these measures except that a government no matter how benevolent is impersonal and can only show how much it cares via the IBM. We need to think of ways to mitigate the Western system whereby an old man receives a welfare check and food stamps but no love and affection. Our family social security and old age insurance system might have little money but provides a lot of love. Can we combine the desirable features of the Western and the Filipino system? What creative fiscal devices can we try out to achieve this end?

(3) As much concern for currently landless and potentially landless agricultural labor as for tenant-cultivators. At the moment there is little or nothing known about them which will enable us to promote their welfare. The land reform program is practically silent about them. We do not even have an estimate of how many they are and labor laws hardly touch them.

(4) Studies of the impact of reduced family size on traditional functions performed by the family. Can the economists calculate how many children we can afford to have in order to achieve growth without undermining the basic character of the Filipino family? Shall we "stop at three" or "quit at five"? Nobody knows. We simply want to have fewer children. If we have three instead of 6.8 children, to what extent do we have to put up old folks homes, lay out "head shrinkers couches", and set up singles bars? In short, if at all possible, we want to "have our cake and eat it too".

(5) Reexamination of "dead-ends" we have conceded to our young people.

Rural youth as a development problem area is suggested by the following: (a) From 5.37 million in 1965, the youth aged 14-21 increased to 6.39 million in 1970, a net increment of 1.02 million or an annual growth rate of 3.0 per cent. This age group comprises 16.9 per cent of the total population. About 4.2 million or 65.6 per cent of these young people are located in the rural areas. Only minor migrations to the urban areas occur within this age group. (b) Of the total youth employment in 1967, around 1.013 million or 63 per cent are engaged in agricultural work. The rest are in industry, trade, commerce and services. About 1.1 million or 69.5 per cent of employed youth in 1967 were in rural areas. (c) There is a noticeable shift of youth employment from non-agriculture to agriculture. In 1967, the youth engaged in agricultural work was 57.6 per cent; in 1966, this increased to 59.4 per cent and then to 64 per cent in 1967. However, these young people are usually engaged in minor routine farm jobs. (d) About 300,000 young people composed the body of unemployed youth in 1967. Nearly half of them are females, and there is an increasing proportion of unemployed youth who are located in the rural areas from 60.8 per cent in 1965 to 62.8 per cent in 1966 to 64.0 per cent in 1967. 1/

Because of our preoccupation with the problem of the educated unemployed, we thought of limiting entry to the University. This is the one last hope of rural people for upward social mobility for their children. In the meantime we are also told that absorption rate of vocational school graduates is very low. We likewise want to discourage rural youth from migrating to the city. What alternatives do we have for them? There are worse things in the world than being educated and unemployed and one of them is being uneducated and unemployed in a poor village even if the fresh air is free.

(6) More research involvement in the various rural development strategies which are being vigorously pursued.

The Philippines as a great exporter of medical personnel is trying creative ways of using granny midwives, auxiliary health workers, lay motivators and paramedics in delivering health care and family planning services to the rural areas. Even the village citizens' assemblies are involved in the medical training program. Pilot projects in applied nutrition have also been embarked upon. The new Rural Cooperative Development Strategy which has been evolved after a long history of recurrent failures in the cooperative movement looks promising. All of these represent innovative approaches to rural development. They are very exciting and fertile fields for social

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1/ Victoria E. Raffaele, "A Study of Manpower and Employment of Youth in the Philippines", Development Forum, Vol. 2, No. 3, June, 1970, pp. 19-33.

science research. At the moment especially for the rural health and applied nutrition projects, the social research input is rather tiny, not because we have not been sought by project leaders but more because we do not have enough research manpower.

But the more important task is to determine who are the participants and beneficiaries of rural development programs. While the international development experts pay homage to the poorest 30 or 40 per cent when rural development programs are planned, there is some reluctance to put in the resources, the time and the manpower to reach them. Helping the poor is not only a very expensive proposition; it is also a very special proposition which requires special projects. The latter are not easy to "sell" because they are special and therefore are not duplicable. To what extent do we end up assisting those who are a little bit better off, relatively speaking?

(7) Looking at the possibility not only of agrarian reform but also of urban land reform in order to achieve an equality of sacrifice between the agricultural and non-agricultural landowners. Urban slum dwellers and squatters huddled in a small piece of land bother the planners but somehow it is not considered undesirable for middle and upper class families to move to the city and take up as much space as they do. Rural squatters, on the other hand, have attracted little attention. What would be the impact of land reform on them?

(8) Social Indicators and Quality of Life.

With some of the misgivings about GNP and per capita income, the development of social indicators is now becoming fashionable. Even the banks want a quantification of social benefits from their new integrated development projects. Indices such as number of hospital beds, doctors, nurses, etc. per 10,000 population; kilometer of roads, average electrical energy consumption, etc., can be as misleading as GNP and per capita income. To be "socially" meaningful, such indices should convey the distributional aspects of the benefits. For example, the national average per capita consumption of electrical energy in the Philippines is 177 KWH. This may be low or high relative to indices in other countries. A development policy-relevant indicator of electrical energy consumption will contain the following additional information: Luzon has a per capita consumption of 300 KWH; Mindanao 70 KWH; and the Visayas 27 KWH. Of the 66 provincial area groupings, only 12 show a per capita consumption rate exceeding 100 KWH, five areas have a consumption rate of between 50 and 100 KWH and the rest have a consumption rate between 0 and 50 KWH. During the fiscal year 1971-72, about 22.5 per cent of the total population were served with electricity. Of this, 50.7 reside in Greater Manila; 19.4 per cent in chartered cities and 29.9 per cent in rural areas.

Fifteen out of 16 families in the countryside do not receive the benefits of electricity. 1/

What social indicators are relevant should depend upon what is considered desirable by a particular society. For example, participation of women in the social, economic and political life of the country and the ability of the family to continue fulfilling its social solidarity functions may be more important to some than to others. On both aspects, the Philippines would rank high.

A recent and noticeable trend has shown us that we have now gone full circle in our concept of rural development. In the early 1950's we thought community development should take an holistic approach and so we had fourfold or manifold programs and multipurpose workers. Then we went to the other extreme of promoting single commodity production programs and currently, we are agog about integrated rural development. No one can disagree with the proposition that "the objective of rural development and welfare is the improvement of the quality of life of the farmer in its totality, more than just increasing productivity" 2/, but can we improve the quality of his life without increasing productivity. The food problem is still very much with us, and we are a long way from solving it. If we forget this, the lowest 40 percent whose welfare we want to enhance will suffer most.

Thoughts on the quality of life of the Southeast Asian border on the poetic and the romantic. As Puey Ungphakorn puts it: "Healthy air, beautiful scenery, clear streams and the like are free in the countryside: this environment which helps to compensate for the farmers' low income must be preserved for them and their progeny." 3/ In our preoccupation with the preservation of the environment, we tend to forget that there are two types of pollution - one which accompanies poverty and another which comes as a by-product of affluence. If the choice is between these two evils, one wonders if the farmer will opt for the healthy air and the beautiful scenery. It is rather poor compensation for what he hasn't got.

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1/ Statistics compiled by the Power Development Council in "Working Toward Total Electrification", by Rodolfo Fernandez, Times Journal, November 5, 1973.

2/ A. T. Morales, Teacher Education in the New Society, Philippines Sunday Express, December 16, 1973, p. 31.

3/ Puey Ungphakorn, "Thoughts on Southeast Asia's Development for 1980". Paper prepared for the 10th Panel Seminar on Southeast Asia's Development Goals in 1980, SEADAG, Asia House, June 8-9, 1973, New York City.

Turning to another aspect of development, if we want a better life for the farmer, why do we want labor-intensive technology? Isn't this what he has had all along? Otherwise, why would we have inverted labor-saving devices?

(9) The complexities of rural development and the humility required of the social scientist.

Almost two decades ago, rural development looked relatively simple. Our late President Ramon Magsaysay stated in his famous pronouncement in 1956 that "the little man is entitled to a little more food in his stomach, a little more clothes on his back and a little more roof over his head." Nowadays what we want for the villager has been considerably expanded to include literacy; a fulltime job which uses labor-intensive intermediate technology (with no disguised unemployment or underemployment); a God-given acre of land he can call his own; an infrastructure of facilities and services such as electricity, roads, communication, low-cost credit, health and medical services; not only higher income but more equitably distributed income. We envision high quality, development-relevant schooling for his children; decent housing; long, healthy adequately-nourished life with only two instead of 6.8 children living in a pollution-free environment. In addition we prefer that he remain in the village in order not to crowd our cities. Finally, we want him and his villagemates to get organized to solve their own problems in the true spirit of self-help.

With all these complexities which have been introduced into the concept of rural development, it is inevitable that the old-time missionary zeal would no longer suffice. We have almost convinced policy makers that we need social science to fathom the imponderables and to lead us to the "paths of righteousness" in development. In the meantime we have created full employment for ourselves.

The Filipino social scientist has come of age. He is respected and he is asked. He discovered his Asian neighbors without giving up his Western friends. For his long-term survival, he should not bungle this golden opportunity by overselling what he cannot deliver.

APPENDIX

University of the Philippines  
Diliman Campus

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1. Asian Labor Education Center
2. Community Development Research Council
3. Department of Anthropology
4. Department of Sociology
5. Institute of Economic Development and Research
6. Institute of Mass Communication
7. Institute of Planning
8. Institute of Social Work and Community Development
9. Philippine Center for Advanced Studies
10. National Development Research Center
11. Social Science Research Council
12. National Research Council of the Philippines
13. Institute for Small-Scale Industries, Manila Campus
14. College of Public Administration
15. Local Government Center
16. Population Institute
17. Family Planning Evaluation Office, Population Institute
18. Statistical Center

University of the Philippines  
at Los Baños

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19. Agrarian Reform Institute
20. Department of Agricultural Education
21. Department of Agricultural Communication
22. University Extension Center
23. Department of Home Technology
24. Agricultural Credit and Cooperatives Institute (ACCI)
25. Community Development Center
26. Department of Forestry Extension
27. Department of Social Sciences
28. Southeast Asia Regional Center for Research and Graduate Study in Agriculture (SEARCA)
29. International Rice Research Institute
30. Philippine Council for Agricultural Research
31. Department of Agricultural Economics

Privately Supported Academic Institutions

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| 1. Social Science Research Unit<br>Central Philippine University<br>(Iloilo) | 12. Office for Social Research<br>University of San Carlos                           |
| 2. Department of Economics<br>University of San Carlos<br>(Cebu City)        | 13. Asian Social Institute<br>(Manila)   |
| 3. Research Center<br>Immaculate Concepcion College<br>(Ozamis City)         | 14. Center for Research and<br>Communication (Manila)                                |
| 4. Institute of Social Order (Manila)  | 15. Coordinated Investigation of<br>Sulu Culture<br>Notre Dame of Jolo College       |
| 5. Socio-Economic Research Center<br>Notre Dame University (Cotabato)        | 16. Department of Anthropology<br>University of San Carlos                           |
| 6. Research Center, Philippine<br>Christian College (Manila)                 | 17. Research Institute for Mindanao<br>Culture<br>Xavier University (Cagayan de Oro) |
| 7. Research and Development Center<br>Centro Escolar University (Manila)     | 18. Institute of Philippine Culture<br>Ateneo de Manila                              |
| 8. Research Office<br>Ateneo de Davao  | 19. Department of Sociology and<br>Anthropology<br>Ateneo de Manila                  |
| 9. Office for Research<br>St. Paul College (Tuguegarao)                      | 20. Graduate School of Arts and Sciences<br>Saint Louis University<br>(Baguio City)  |
| 10. Silliman University Social Science<br>Research (Dumaguete)               |  |
| 11. University Research Center<br>Silliman University                        |  |

Government and Other Agencies  
Supported by Public Funds

1. Bureau of the Census and Statistics
2. Bureau of Agricultural Economics, Department of Agriculture and  
Natural Resources
3. National Economic and Development Authority
4. National Manpower and Youth Council
5. National Tax Research Center
6. Department of Economic Research, Central Bank
7. National Science Development Board
8. Development Academy of the Philippines

Non-Academic Privately Supported Agencies

1. Philippine Business for Social Progress
2. National Secretariat for Social Action
3. International Institute for Rural Reconstruction
4. Philippine Rural Reconstruction Movement
5. Program Planning Research Evaluation Office, Family Planning  
Organization of the Philippines
6. Davao Action Information Center

Social Science Research in Latin America

by

Rolf J. Luders  
Organization of American States

## INTRODUCTION

This paper discusses the general status of the social sciences in Latin America, the link between research and policy formulation in the area, and the relations between local and foreign research institutes and researchers.

The reader should be cautioned about the fact that the paper was written under severe time and space limitations, and some, although certainly not all, of its drawbacks reflect this fact. It is supposed to cover all the social sciences, but it was written by a person familiar primarily with economics, and the paper shows it. Moreover, no effort was made to decide which disciplines to include in "social sciences". The term is used rather loosely, since the basic information available did not always allow us to distinguish which disciplines were referred to. It was also to cover all of Latin America, but the author is well informed about the status of the social sciences in only a few countries, and there was not enough time to gather comprehensive information for the whole region. In summary, the author has tried to cover as objectively as possible, all of the social sciences for all of Latin America, but, in spite of the best efforts made, it is very often impressionistic, and has a definite bias to draw its conclusions from what happened in economics in a few countries, especially Chile.

A very important aspect in the evolution of Latin American social sciences is that research institutes tend to be sharply divided on the basis of methodological and ideological differences--the clearest line being drawn between those institutes which are Marxist and those which are not. Moreover, competition between institutes (ideologies) explains, at least in part, the relatively rapid development of these sciences in Latin America. However, this facet is not dealt with explicitly in the paper because, on the one hand, ideologies get reflected into research topics and, on the other, topics tend to determine the methodologies to be used. Therefore, in the author's opinion, a correct interpretation of research priorities encompasses these aspects.

The topics discussed in this paper were treated with considerably more depth and coverage, although the material is already somewhat dated in two publications derived from a six-week Seminar and a three day Conference, sponsored by the Joint Committee on Latin American Studies (1963) and the Social Science Research Council (1965), respectively: Social Science Research on Latin America, edited by

Charles Wagley (Columbia University Press, 1964), and Social Science in Latin America, edited by Manuel Diogenes, Junior and Bryce Wood (Columbia University Press, 1967). Undoubtedly, since 1965 research in the social sciences has made considerable progress in Latin America. Economics, already then a relatively established profession, has matured considerably as reflected in the research topic choice and the volume of output. The other social sciences, especially sociology and political science, have increased their professional ranks.

In spite of this progress, most social science researchers are not satisfied with the impact their research is having on policy makers. This feeling exists, although observers of Latin America would agree, I believe, that the social sciences per se have had considerable impact on policy making in Latin America. However, this influence was not the result of research findings, but of imported ideologies. There are perhaps only two important exceptions to this general statement, namely the impact of the Economic Commission's for Latin America work during the 50's and early 60's, and the influence of research on the demand for money and Central Bank policies. This paper suggests several reasons for the up to now limited impact of social science research on policy decisions in Latin America, and predicts a considerable change in this relationship in the future.

The influence of foreign social science institutes and researchers on the training of professionals in the area is highlighted. The paper deals with the costs and benefits of this professional development process and suggests one alternative for attaining it more effectively in the future. The paper postulates further that research done by foreigners--which in many instances has been irrelevant for Latin Americans--might be a very important instrument in fulfilling future research needs in some subjects which probably will not be covered by local researchers because of national political constraints.

Foreign human and financial resources have in the past, and can also in the future, play an important role in allowing local independent research institutes to have a wider topic coverage. Foreign experts have and probably will influence public policy and research orientation of institutes.

The paper clearly illustrates the overwhelming preponderance of research in economics, and within this discipline of the areas concerned with growth, inflation and trade. It also shows the general lack of interdisciplinary social studies in Latin America, with the possible exception of urban studies. Very little is being done in rural development, although considerable work can be found in the area of agricultural economics, and some in rural sociology.

Finally it is suggested that the region needs to further develop "loose" research networks. Regular professional meetings organized to discuss, on the basis of advanced or completed work, research methodologies and techniques should be institutionalised as soon as possible.

### I. THE STATE OF THE SOCIAL SCIENCES IN LATIN AMERICA

It is obviously an impossible task to provide in a few pages even a moderately complete description of the social science research institutions in Latin America, their staff and topics of research. Fortunately, the Social Science Research Council of Latin America (CLACSO) published in 1968 and again in 1969 the Directorio de Centros Latinamericanos de Investigacion en Ciencias Sociales, a publication that listed the 25 principal independent social science research institutes in Latin America, including information on their financing, staff, training and research. Moreover, during 1973 the Development Centre of the Organisation for Economic Co-operation and Development published as a LIAISON BULLETIN a Directory of Development Research and Training Institutes in Latin America, containing very similar information for a somewhat larger group of independent institutes as well as international organisations. There is, unfortunately, no similar information available for research done by the governments.

These publications, in addition to our own impressions about the institutions whose description is missing, have provided the basic material for the next subsections. We will make a few general comments describing the institutions, their financing, some characteristics about their staff and their research priorities. We will end this section with some comments on the independence of the social scientist in Latin America to evaluate policy issues.

#### Research Institutes

In this part we will (a) describe the quality of social science research institutes in Latin America and their character and regional distribution, (b) illustrate their growth and methods of financing and (c) show the lack of institutes doing interdisciplinary work as well as the need to create additional research networks.

The total number of institutes (government, international organisations and independent) doing some sort of social science research in Latin America must number several hundred. There are alone over one hundred and twenty schools of economics and business

administration, and all of them are engaged--some more, some less-- in research activities. If, however, we apply international standards to judge the quality of the research done in these institutes, the total number drops substantially. The LIAISON BULLETIN referred to above describes 39 independent institutes and 10 international organisations. There are, I am certain, several very "good" institutes missing in the publication, in addition to all one hundred percent government-controlled research organisations, some of which also provide us with excellent research. Recognising that there is an obvious value judgment involved, I would, however, doubt very much if any one could argue that in Latin America there are more than fifty or sixty "good" social science research institutes, besides a few international and government organisations.

Those institutes classified here as independent belong really into at least three, quite distinct, categories: those attached to the universities, those organised as independent private institutes, and those that belong to the governments but are autonomous. Of these three categories, the university institutes are in Latin America by far the largest. The independent private institute was only created, as a rule, when conditions at the universities did not allow an "adequate" and "free" development of the social sciences. They have, therefore, in general a political--in the good sense of the word--origin.

Social science research institutes, including international and government organisations are in Latin America generally very new. The vast majority were created during the last 15 years, and only very few date back to the 1940's or earlier. This, as we will see later, is important with respect to research topic selection.

The limited information available also seems to indicate that most social science research and training institutes depend very heavily on government financing. This is quite obvious in the case of government-owned and international organisations, but it turns out to be also true for those institutes attached to universities, both private and public. Even some private independent institutes finance themselves heavily on the basis of research contracts with the public sector. Foreign resources (foundations, AID and others), as well as private contributions and contracts, appear as less important.

However, the information given above on financing of these institutes is aggregated both for training and research. My own experience and observations make me believe that foreign resources and private and public sector contracts provide a substantial proportion of the resources available for serious research. Moreover, the diversity of financial possibilities provided for by different

sources of funds (government, local private, different foreign sources) has allowed the independent research institute to select research topics quite freely. The institute might, for instance, finance (a) studies on foreign investment with government funds and (b) research on some aspects of bureaucracies with foreign resources. This freedom has turned out to be a very important aspect in the development of the social sciences in Latin America and it might well be, as we will see in one of the next subsections, that over the next years its importance will even grow.

Most independent institutes are uni-disciplinary, and most of these are in the area of economics. There are only a few institutes which label themselves "social science research institutes" and whose staff is multidisciplinary. Interdisciplinary work at independent institutes is almost exclusively done in a few urban studies and public administration institutes. Interdisciplinary work on rural development at a significant scale could only be found in one independent institute and one international organisation, although I know of several government organisations engaged in it. In spite of this fact, several institutes are devoting important resources to do work in agricultural economics and rural sociology.

A few words should be said about the location and inter-relationship of social science research institutes in Latin America. Of the 39 independent institutes listed in the LIAISON BULLETIN, 12 were located in Argentina, 5 each in Brazil and Chile, 4 in Colombia, 3 each in Mexico and Venezuela, 2 each in Costa Rica and Paraguay, and one each in Ecuador, Jamaica and Peru. Although this list is not a complete list of all "good" social science research institutes of the region, it is my impression that the resulting geographic distribution is--in a rough way--relatively representative, although overstating relatively the contribution of the Argentine institutes, and understating the one of the Brazilian organisations.

It might surprise some to know that, to my knowledge, the formal contact of social scientist is perhaps more developed at the international level than at the national one. There is, as far as I know, in all of Latin America only one active national professional organisation in the social sciences, and that is the Argentine Economic Association. In some countries, Chile and Argentina among them, economists have held in the past "meetings" similar to the ones of the American Economic Association, but only in Argentina are they organized on a regular basis. No profession-wide journals exist, and as a rule each institute publishes its own. Informal contacts are, of course, very common at the national level, but they are heavily dependent on personal relationships, and are therefore, in my opinion, far from ideal.

However at the international level it is possible to identify at least two important initiatives to formalise the interrelationship of social science research institutes. One is CLACSO, the Social Science Research Council of Latin America. Virtually all of the "good" independent social science research institutes belong to it. CLACSO has been holding regular annual meetings of institute directors since 1966-67 and sponsors working groups to study some of the most important problems common to certain groups of countries. To carry out these and other functions it has a full-time professional management. The other initiative is ECIEL, an up to now relatively loose association of about 20 Latin American economic research institutes, coordinated by the Brookings Institution in Washington. This group is carrying out a few region-wide studies relevant for the integration issue, each institute making its own country study but using a common methodology. Researchers participating in the ECIEL studies have been meeting regularly on a semi-annual basis for almost a decade.

### Staff

The available information allows us to have some notion of the size of the professional staff employed by the better independent and international social science research organisations, although there is no way in which I could estimate the number employed by governments. According to the LIAISON BULLETIN there are slightly over 900 professionals doing full-time research in the 49 independent and international organisations listed, that is, an average of about 15 per institute. There are, however, two institutes (Getulio Vargas and the Economic Commission for Latin America) which together employ about 300 research staff members, reducing the average for the remaining institutions to slightly over 10. Moreover, I have the impression that most institutes included--for the purposes of providing information to the LIAISON BULLETIN--among their staff members those which are on extended leave of absence, so that the average effective number is in all likelihood below 10. Finally, a question as to "full-time" arises. It is again my definite impression that very often "full time" does not mean more than 6 hours a day, and that--in general--it refers to both training and research activities. At the university institutes, certainly, the average staff member probably does not devote more than 30 percent of his time to research.

The information provided in the LIAISON BULLETIN also allows us to make a rough estimate of the researchers general discipline. Of the over 900 research staff members employed by the listed institutes (both independent and international) about 75 percent are economists, 15 percent are sociologists and the remaining 10 percent are political scientists, anthropologists, historians, psychologists, etc. This fact has, of course, a substantial impact on the research topics chosen, as well as on the feasibility of undertaking, in the short run, any interdisciplinary work at a significant scale.

It is, however, much more difficult to say anything definite about the quality of this staff. On the one hand, it is almost impossible to choose a yardstick of quality with which everybody agrees. On the other hand, even if we agreed that the simple doctor-master-bachelor classification without regard to the university granting the degree is meaningful, we still have unfortunately very little information on those variables. Only nine institutes of the 25 listed in the 1969 Directorio of CLACSO indicate the academic degrees of its staff members. Out of a total of 139 of their staff members (including research assistants in some institutes) 27 held a Ph.D. from a foreign university, 27 were Ph.D. candidates at foreign universities, 43 had either a Master's degree or some training at a foreign university, and 42 had only local training. Most of the latter, however, were research assistants and, in general, people who were about to leave to get foreign training. My impression, however, is that these nine institutes had a staff of a somewhat above average "quality", so that in general in Latin America the relative number of Ph.D's and Ph.D. candidates is somewhat lower and the senior staff without foreign training somewhat larger than those of these nine institutes. In spite of this, the general conclusion that a very high percentage of the researchers is foreign trained remains, in my opinion, valid.

Finally, it should be said--although I do not have any direct reference--that the average social science researcher in Latin America is relatively young. Although there are exceptions, most of the social scientists in Latin America received their formal training abroad during the late 50's and 60's, which implies that the "senior" staff members are, at most, in their early 40's.

#### Research Priorities

If, on the basis of available information, it is difficult to assess the "quality" of the social scientists in Latin America, it is even more difficult to make any meaningful statements about research priorities. I am going to abstain from classifying on-going (and/or completed) research projects into a few subject categories according to titles. This exercise is only of some value if we previously screen each title to homogenise for quality, but this is an extremely time consuming exercise. Schuh in his book on Research on Agricultural Development in Brazil does precisely that.<sup>1/</sup>

Besides the information contained in the Directorio and the LIAISON BULLETIN, our main recourse will be to use our own impressions

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<sup>1/</sup>G. Edward Schuh, Research on Agricultural Development in Brazil, The Agricultural Development Council, Inc., New York, 1970.

on the basis of the experience gathered attending meetings of CLACSO, ECIEL and other organisations where research was discussed, as well as on the basis of knowledge of research institutions and their aims.

There is no doubt that most of the serious social research in Latin America is done in the economic discipline. Within the economic discipline, perhaps the area of most concern has been economic development and planning (including "model building"), but most of the work done on these general subjects has not been--in my opinion--too original. Empirical research has concentrated on monetary and fiscal matters (including the problem of inflation), industrial economics (including production functions), human resources, agricultural economics and international trade.

Research in each one of the other social sciences is negligible compared with the work done in economics. Among the other social sciences, research in sociology stands out, both because of its quantity as well as its "quality". As in economics, most of the work in sociology has been related in general to the social aspects of economic development, including the study of the industrialisation process. Relatively little work has been done either in social psychology, history or political sciences. From an interdisciplinary point of view, research done in the areas of urban and regional studies and demography stand out. There is much less on rural development than the subject merits.

Another characteristic of the research topics chosen has been that they are usually either general-theoretical or specific-empirical. I would venture to say that it is possible to find a relatively high correlation between the age-training variables and the topic-methodology choices. The young, foreign-trained social scientist shows a tendency to select a very specific topic ("the demand for sugar in Haiti", "the influence of religion on working habits in Cacuy") and to test his hypothesis empirically. The more experienced scientist, who in general in Latin America has had much less contact with "modern" social science techniques, shows a tendency to select more "important" subjects, but is only able to treat them in a very general and "theoretical" way. The proportion of social scientists that fall into this latter category has fallen sharply over the last 15 years, and so have their research topics and methodologies. Moreover, by comparing the Directorio (1969) with the LIAISON BULLETIN (1973), one can observe that the number of general empirically-based studies increases. We will later interpret this fact as the maturing of the profession.

A final characteristic of research in Latin America is the relatively large average number of ongoing research topics per scientist. Only very few independent institutes show a number of research topics equal or less than the number of full-time researchers. In most institutes each full-time researcher is working on several projects,

usually not too closely related to each other. The explanation of this fact is varied. For one, most research institutes are an integral part of a training program, and the social scientist is both teacher and researcher. Usually he has to teach several subjects and is therefore also tempted to do work on subjects related to each of his classes. Also, research topics are very seldom chosen on the basis of a comprehensive research program of an institute, but usually reflect individual preferences and contract possibilities. An individual might be working on a subject (his thesis for instance), and is offered to do some contract research on some other topic. Finally, the fact that interesting research needs are so enormous in Latin America, and social scientists are young and enthusiastic, tends--in the absence of institute-wide structured research programs--to induce them to try to tackle more than they are perhaps able to do.

Turning now briefly to the subject of agricultural development, perhaps the results Schuh found for Brazil might be helpful in illustrating research priorities within that general field. Schuh divided the research topics into 16 subtopics, and found that those receiving most attention were "supply and demand for agricultural commodities" (19.1 percent), "farm management and organization" (18.6 percent), "economic and agricultural development" (11.9 percent), "agricultural marketing" (9.6 percent) and "rural life and organization" (7.3 percent), while those receiving least attention were "agricultural mechanisation" (.4 percent), "values, attitudes and motivation" (.3 percent) and "rural industry" (1.1 percent). Although I have no way of judging in an objective way the relevance of the above priorities for Latin America as a whole, it is my impression that, in general, demand studies for commodities and management aspects have had a relatively high priority, while social and motivational studies have been much lower on the priority scale.

#### Independence to Evaluate Policy Issues

I will now turn to a subject very closely related to the choice of research priorities and the financing of research, namely the independence of social scientists in Latin America to evaluate policy issues.

This independence is, as a rule, not too large in government organisations, where obviously political pressures tend to limit possible criticism of official policies. To a certain extent, but to a somewhat minor degree, this same observation is true for research being carried out in international organisations.

Independent research institutes were up until the mid 60's in most countries quite free to study and publish the results of whatever subject they deemed interesting and relevant. This, in

spite of the fact that most of these institutes were heavily dependent on financial assistance from governments and the results of their research, did not always support government policies. Since the mid 60's, however, and in line with the changing political climate of the region towards more military controlled governments, even "independent" institutes seem to be losing their freedom to evaluate certain critical policy issues. Recent events in Chile, Peru and Uruguay confirm this general trend, which has turned into a major concern for many social scientists of the region. There is now a more or less generalised opinion among social scientists that many Latin American governments will, over the next years, strictly limit research support to those institutes which work within their general policy lines. This will, of course, inhibit free research on the merits of alternative socio-economic systems and other similarly broad subjects, in spite of the fact that it will have only a minor effect on research on more specific topics.

Alternative means of financing research will become, therefore, a key element in the freedom of choice of research topics social scientists will face. Since it is expected that only in extreme cases will governments prohibit certain types of research, the ability to finance from other sources those projects the government will not fund will be essential to allow for wide research coverage. Private and foreign resources will, therefore, play a key role in the determination of the scope of social science research in Latin America.

## II. THE LINK BETWEEN RESEARCH AND POLICY FORMULATION

In this section we will describe the influence of social science research in Latin America on policy formulation. The prevailing opinion among the region's social scientists is that this influence is well below what it ought to be, given the amount and quality of the profession's output. We will therefore concentrate our effort to explain why, in our opinion, policy makers do not make sufficient use of research results, and also why the profession is not producing enough research which is relevant for policy makers.

However, before turning to the above, let us refer briefly to the difficult question of what ought to be the relationship between research and policy formulation. Should social science research be primarily oriented to be of use to policy makers in the day-to-day operation of government? Or should it be primarily oriented to the task of "nation building", that is, in providing a better understanding of the basic social relationships existing in the different countries? If our main concern is with the first aspect, we should perhaps judge the influence of social science research

on policy formulation by the number and importance of the decisions taken on the basis of specific research pieces. If, however, our interest lies in the broader influence of social science research, we face a much more difficult--if not impossible--task. We would have to somehow identify the indirect influence of this research on the development of the sociopolitical system of a country, admittedly an overwhelming task.

In this paper we will take the more narrow view, that is, we will try to assess the use policy makers make of social science research in their day-to-day decisions. This is not to say that it is our opinion that social science research ought not to contribute to "nation building". It only means that at this time it is almost impossible to assess its influence in this sense because it is conceptually very difficult to do. Moreover, with the exception of ECLA and very few national institutes, social science research organisations are of recent development in Latin America and one would expect them to have had still limited influence in this broader view.

This does not mean, however, that several social scientists have not influenced the course of development in some Latin American countries after leaving the research institutes and becoming high government officials. Through the policies they advocated, which certainly considered the results of their experiences as social science researchers as well as of the international literature, they might have made an important contribution to the task of "nation building."

Before turning to the main body of this section it seems appropriate to make a few comments about the impact of the research of foreign social scientists on policy decisions within the region. This impact has been considerable in some countries, although it is our impression that in this case most of the influence came through consulting work and research orientation. However, it was the research that allowed the foreign scientist to become familiar with the region's problems and other characteristics. Moreover, it was this work which made him well known and trusted by governments.

#### The Nature of the Development of Social Science Research Institutes

As indicated above, it is possible to find hundreds of institutes doing social science "research" in Latin America. If, however, we concentrate on those which do research of "good" quality by internationally acceptable standards, we generally find that they are very few in each country, and they normally belong institutionally to either a university or the government. In the next few paragraphs we will refer to the development of social science research in these two institutions.

First, universities in Latin America have put in general a heavy professional emphasis in their faculties. The social sciences developed, if at all, only as subjects necessary to educate a lawyer, architect or accountant. The professional schools did not consider it necessary to dedicate scarce resources to do research in the social sciences, and, in general, only required the presence of a teacher who would organise his class work around foreign literature.

During the 50's and 60's, when Latin America became more and more concerned with the problem of development, the subject of economics became of high priority, first within the professional schools, and later on independently. In spite of it, the big development of economics as a science in Latin America was induced by foreigners during the last decades, mainly by foreign foundations and aid-giving agencies. Those foreign institutions, concerned as they also were with the economic development of Latin America, considered that Latin America did neither have the knowledge of the workings of its economies nor the people capable to propose rational policy measures to make development possible. A necessary condition for economic development was therefore the development of this knowledge and people, and the place to do it--it was decided--was at the universities. Fellowship programs for studies in the United States, and occasionally in Europe, were created, as well as university-to-university programs between departments of economics in the USA and counterpart schools in Latin America.

The universities, and in some instances a few private research institutes--like di Tella in Argentina for instance--adapted to the inflow of resources and initially developed economics and later on other social sciences like sociology, political science and anthropology. Social science schools (or departments) were created, and, in the better schools, serious research was initiated. Professors were hired on a full-time basis and salaries, as well as status were improved. On the whole, however, and as a consequence of the type of training programs normally available abroad, the research and training programs set up in Latin American universities did not follow any interdisciplinary approach, with the possible exception of urban studies. Only recently have some universities begun to create research centers concerned with high priority problems which require for their solution a multidisciplinary approach.

Second, most governments also do social science research mainly in the area of economics. Generally this is done within special research departments, notably in the Planning Offices and the Central Banks. In general this activity, while existing prior to the 50's, has had a parallel development with the availability of well trained personnel produced by the universities. It is perhaps not exaggerated to say that very often in Latin America the supply of professionals itself created within the government the demand for their services. Moreover, research in government takes, as a

rule, one of two forms: (a) the preparation of largely data-gathering exercises, and (b) the evaluation of different alternatives for specific, short-term policy measures. In general, little, if any, basic research is done. A notable exception is the work done by the Vargas Foundation in Brazil, although this semi-independent institution fits hardly the description of a typical government institute.

Another exception is perhaps research in the area of rural development, and the subject merits a brief digression at this point. Rural development, in spite of its importance for Latin America, has been one of the neglected subjects of social science research in Latin America. So much so that the Latin American Social Science Research Council (CLACSO) has declared it as such, and is beginning a concerted effort to do something about it. Almost all present local efforts in this area can be found in government institutes, mostly related to agrarian reform programs. There are, in addition, a few international projects and beginning programs in some local universities.

It is of some interest to the subject of this paper to try to explain why research in rural development has been, up until now, located principally at the government, instead of at the universities, where most of the other basic research in the social sciences is now being carried out. In our opinion, there are at least three concurring reasons: (a) agrarian reforms are massive programs, very little foreign knowledge is available, and governments needed immediate responses to their questions and felt that the best way to obtain them was to establish their own research efforts; (b) rural development requires an interdisciplinary approach and the universities were not institutionally prepared to deal with it, and (c) "the class origin, training and bureaucratic organisations of most agricultural researchers have impeded their perceiving and finding solutions for the problems of the rural poor".<sup>1/</sup>

Returning to the main theme of this subsection, we can say that as a result of the development process of social science research institutions in Latin America, most of the serious basic research is being carried out by a few university institutes or independent research organisations, mainly on a uni-disciplinary basis, and usually with heavy past (or present) ties to foreign institutions. Governments are also involved in social science research, but in general with emphasis on data gathering or on subjects requiring immediate solutions.

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<sup>1/</sup> Consultative Group on International Agricultural Research, Program and Background Papers, World Bank, Washington, D.C., 1973, page 72.

Any history, no matter how brief, of the development of social science research institutions in Latin America, has to refer to the Economic Commission for Latin America (ECLA) and the Alliance for Progress. ECLA did most of its research and training in the area of economic development, and had a profound influence on economic policy decisions in several, if not most, Latin American countries during the 1950's and early 60's. The professional staff of ECLA and its "school" had--as opposed to its present generation counterpart--very little training abroad. In a way it can be said that they "created" the import substitution growth model, and developed the "structuralist" explanation for inflations in Latin America; its followers are today primarily concerned with the problem of dependencia, that is, the interrelationship that exists between the economic development of the rich countries and the poor ones, which, they sustain, implies that the former grow to an important degree at the expense of the latter.

The influence of the Alliance for Progress in the development of social science research, especially in economics, was the requirement that aid would be allotted on the basis of national plans, which induced the creation of national planning offices.

The Training of the Social Scientist and the Nature  
of the Demand for his Services

Today most influential 1/ social scientists in Latin America have had some training abroad. It is our impression that most economists have been trained in the United States, most sociologists in Europe, while most political scientists and anthropologists divided themselves about equally among those two regions.

Local governments, while obviously approving of foreign training, were not--as a rule--participating actively in these programs. It is my impression that local bureaucrats and politicians felt in a very general way that these programs were "good" for the country, but that they did not know exactly how to make the best use of the research capabilities of the returning trainees. The social scientist, therefore, returned to his home country to either teach or write his thesis.

The question that comes to mind immediately is why were government officials not able to make better use of the newly acquired research capabilities of the foreign trainees. The answer seems to me quite simple. To use a caricature, in Europe and the USA the development of problems, data, and techniques to analyse those problems and propose solutions, was quite harmonic. The "language" differences between a public official and the social scientist were tolerable and permitted effective communication.

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1/ Influence within the profession.

After all, very often both had studied together. Not so in Latin America. Foreign training meant a "big jump" in knowledge and language for the social scientists, unaccompanied by a similar "jump" for the bureaucrat and politician. Communications between one group and the other became difficult, and as a result the latter had no way in which it could use the former effectively, except for training purposes. I should add that in my opinion to a large extent the effectiveness of ECLA in getting governments to adopt its suggested policies has its origin in ECLA's civil servant training programs, which allowed for an easy absorption by the latter of ECLA's research results.

Although the social scientist's research services were of little demand by the government, he had a "demand" for them in his thesis requirement. As mentioned earlier, initially people were sent for training abroad with the purpose of, on return, either training additional professionals or doing basic research. Therefore, a considerable proportion of those trained abroad did it at the Ph.D. level. However, the granting of a Ph.D. requires the writing of a thesis which, even in those schools in the USA and Europe where professors are aware of problems in underdeveloped countries, generally require work which demonstrates that the student is able to apply effectively to research the techniques he has been taught.

In choosing a subject for his thesis the young social scientist faced, however, a dilemma. On the one hand he had to fulfill the minimum requirements of his foreign department as to application of "modern" techniques, and this--in general--required (a) data which was very difficult to obtain in his home country, and (b) choosing a "problem" where those "modern" techniques could be applied. On the other hand, I believe, most Latin American social scientists have a desire to make a positive contribution to the solution of some of the many important problems that face the region, as opposed to just "learning something" about the world. They therefore would have liked to choose a relevant problem.

The result of this choice was, I suppose, in general some sort of a compromise. It is therefore inexact to consider research priorities in Latin America today as totally irrelevant. They are not. They address themselves--as we have seen in the first section--to many important problems, especially in the area of economic growth and price stability. It is, however, very clear that research priorities do not reflect appropriately all of the main socio-economic problems of the region, principally because several of these problems do not exist or are of lower priority in the United States or Europe, and techniques to deal with them are not taught, and

therefore, required to be applied in thesis work.<sup>1/</sup>

To just mention a few of the major categories of problems not adequately covered by researchers in Latin America today, the following come to the mind of economists immediately: (a) the effect of income distribution on growth (or, perhaps better put, means by which income can be distributed in accordance with the objective function of a society and at the same time have its negative effect--if any--on growth minimised); (b) the whole set of problems centered around unemployment; (c) the behavior and problems of marginal groups in the urban sector (favelas, callampas, etc.); (d) rural development;<sup>2/</sup> (e) the questions involved in population growth, nutrition and education; (f) problems related to effective means of communication and transportation; (g) the question of dependencia, economic, cultural, intellectual; (h) the topic of participation, at all levels. This list is certainly incomplete, even from an economist's point of view, but it illustrates the kind of problems that seem to us to be relevant for research in Latin America and around which relatively little work has been done.

#### Implications for the Future

The whole process of training social scientists abroad, building up local training programs and doing research (although very often about topics of lower priority) has been paying off during the last few years, especially in the area of economics. As mentioned earlier, governments today are staffed very often with well trained and competent economists who can make use of modern research results. Also, a substantial number of economists have completed their Ph.D.'s and are now tackling more relevant questions, trying to develop or adapt techniques to solve those problems. Moreover, several local graduate programs have, without losing theoretical rigor, changed their curriculum from a copy of a foreign

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<sup>1/</sup>The data "problem" is, at least to some extent, the result of this lack of adjustment between problems, data and techniques. Techniques were developed to solve certain problems with existing data. In Latin America sometimes the problem is the same as in the USA or Europe, but the existing data are different. If the social scientist tries to apply the technique developed in the USA or Europe, he will very often of course have a data "problem".

<sup>2/</sup>For a long list of new research topics in this area see, among others, Consultative Group on International Agriculture Research, *ibid*, pp. 73-74.

curriculum to something better suited to the needs of the country. The process of developing the economic profession is by no means completed in Latin America, but I would say it is quite advanced.

Not so in the other social sciences, where substantial work remains to be done, especially in political science, anthropology and history. The question here is, should Latin America follow the model it has used to develop the economic profession, or should it use a different one? An alternative to massive foreign training could be the establishment in Latin America of a few graduate level training and research centers. These centers would, initially, be staffed largely by foreigners committed to them, however, for a long-term period. Students would be relatively mature persons, as opposed to the case in economics. They would have chosen a topic of importance to be studied before entering the training program. The training method, instead of being principally based on course work as it was in economics, would center around the research topics. In this way, the center's staff would transmit only those existing techniques that are relevant to the problems to be studied in Latin America, and both students and staff would develop new techniques wherever they are necessary.

I am sure there are more alternatives to develop the non-economic social sciences. It is very difficult to assess which one is the best, or even the most economic one. The massive foreign training of economists has yielded positive results in our opinion, but it has taken very long. Something along the lines of the Latin American centers proposed above was tried through FLACSO (Latin American Social Science Faculty) with mixed results, although it should be said that the strategy was different.

In any event, there seems to be a need to strengthen the non-economic social sciences in view of the fact that many of the relevant socio-economic problems of Latin America require some sort of an interdisciplinary approach to be effectively studied. As long as a very big gap exists between economists and other social scientists, it might be even much more difficult than it usually is to put up effective interdisciplinary teams. However, interdisciplinary work might be a complement to the training of social scientists, that is, techniques useful in several of the social sciences might be transmitted from those who master them to others in the team.

There seems to be a great need to integrate the results of the research done up to now into models (not necessarily mathematical) which--on a comprehensive basis--explain national development paths. Because of the very nature of most of the research done up to now, there exists in Latin America a great deal of knowledge of specific aspects about the different societies. However, very few centers of social science research have made the effort to integrate these results into a framework which provides a--tentative--explanation

of the countries development process. This should be done, and on the basis of it new research areas can then best be defined. Those few research centers in Latin America who have done so in the past have, in our opinion, maximised the impact of their resources.

The same process of providing a better understanding of the national development paths should contribute to another missing element in the development of the social science in Latin America; namely the role they should play in evaluating and guiding research in the non-social sciences. Precisely because research in the non-social sciences--except medicine perhaps--is relatively insignificant, it is important to assure its maximum productivity. The productivity of it ought, in our opinion, to be judged on the basis of its contribution to the general welfare of the country (region), and the social sciences have a key contribution to make in this judgment process.

Finally, social scientists should make over the next years a much greater effort than in the past to "extend" the results of their research so as to make sure they are "consumed" at the right time and place. We already mentioned how effective ECLA was in the past in doing this, and some of the methods used by ECLA should in all likelihood be imitated. Seminars for civil servants and publications should play an important role.

### III. THE RELATIONSHIP BETWEEN LOCAL AND FOREIGN RESEARCHERS AND INSTITUTES

We already mentioned in the previous section the fact that a high proportion of the younger social scientists received their training abroad, and that social science departments of foreign universities had been or were still engaged in special programs to help develop the social sciences in some Latin American countries. These relationships have had as by-products an increasing interest of foreign scientists in Latin American studies, as well as an easy access by these scientists to data and research facilities in Latin America. It should be mentioned here that the overwhelming majority of foreign scientists and institutes involved are from the United States.

#### The Role of Foreign Researchers and Institutes

Besides the obviously important contribution of training hundreds of economists and other social scientists, foreign researchers and institutes have been contributing significantly by directly doing research in the social sciences in Latin America.

To begin with, international organisations in Latin America, as well as some of those in the rest of the world (IMF, IBRD, OAS, and others), are devoting substantive resources to research on topics of interest to Latin America. One can get an idea of the size of the contribution from the LIAISON BULLETIN; out of the over 900 social scientists working in research for the listed institutes, more than 270--that is, almost one third--belong to international organisations operating in Latin America. Although this proportion in all likelihood overstates the contribution of the international organisations because, as mentioned earlier, the BULLETIN does not cover all independent institutes nor does it include government organisations, it indicates the importance of the volume of research done by international organisations. Moreover, in the area of demography, the Latin American Center for Demography (CELADE) is by far the most important institute in Latin America. We already mentioned before the influence of ECLA's studies during the 1950's and early 60's.

Hundreds of foreign social scientists have done important work on Latin America, either on an individual basis, usually under some sort of a grant to either write a thesis or a specific piece of research, or as part of a special program (for instance, the Land Tenure Center at the University of Wisconsin on rural development; the University of Chicago on inflation and other general economic problems; Michigan University and Stanford on management sciences; the University of North Carolina on agricultural economics; Yale University on development problems, and many other US and European universities).

While this research often has been extremely valuable for policy makers in Latin America, research done by foreigners has also been criticised by the region's social science community. The two main reasons for this criticism have been (a) the choice of topics of research and (b) the fact that the results of the research very often were not published for Latin American consumption.

More often than not, research topics chosen reflected the priority of the foreign researcher rather than the needs of knowledge of the country about which the research was done. The foreign social scientist, as a rule, was more interested in writing about something publishable in a US or European journal, than in something important for Latin American policy makers.

It was also rather common to find that a foreign social scientist would get substantial aid in Latin America for his research in terms of data, contacts and assistants, but that he would write up and publish his research piece abroad, without making any effort to rewrite it for, and publish it in, Latin America.

In spite of these criticisms, relations between the region's and foreign social scientists have been remarkably good. They should, if anything, improve in the future as, on the one hand, the area's profession improves its quality to eventually match that of the foreign social scientists, and the need for foreign research becomes indispensable in certain areas.

We referred above to the fact that several Latin American institutes would in all likelihood not receive government support for projects in certain topics. This shortcoming can be overcome either by providing private or foreign funds for local researchers or institutes or by having foreigners directly do the research. This latter solution will, for political reasons, not be possible on all subjects and in all countries, but very often it might prove to be a very good, and necessary, substitute for research done by local scientists.

As more and more research institutes in Latin America structure their research programs better, I feel that the marginal contribution of the foreign scientist will become increasingly valuable to Latin America. The Latin American institutes will then be able to invite down those foreign social scientists they need to complete their research programs, either because the institute does not have anybody with a particular skill, or because they feel a different point of view can improve their program. In any event, I would foresee in the future less foreigners than in the past doing research independent of the host institutes' programs, and integrating more of their work into the hosts' programs.

#### The Need to Develop Research Networks

We indicated above that, at the international level, Latin American social scientists had, at least, two organizations (CLACSO and ECIEL) to associate themselves. There is, however, a need to create a discipline-wide forum to discuss research results. It is my opinion that at this moment this forum (or set of forums, one for each discipline or interdisciplinary problem studied) should be international rather than national.

The position taken here is that any profession needs, for its fullest development, a place where its practitioners can get together to discuss methodologies and techniques on the basis of advanced or completed research. To be able to set up such a place, however, a minimum of practitioners of uniform "quality" is required. At a national level this minimum exists in Latin America only in economics, and in this field only in a few countries. However, at a regional level such minimums would exist for almost all disciplines of the social sciences, especially when we include foreigners doing research on Latin America.

As described above, neither CLACSO nor ECIEL are providing this place now, although the former could become the institutional basis for such regular professional meetings. Neither are the very important seminars organized around a specific topic (employment, capital markets), sponsored by the National Bureau of Economic Research together with other institutions, sufficient. These professional meetings seem to me especially important now when, as indicated before, the profession is trying to develop new ways of solving relevant problems with available information.

However, these meetings are relatively expensive, and they almost certainly will require some sort of foreign financial aid. I see in this aid a new and perhaps one of the most important contributions foreign institutions could at the present time make to the development of the social sciences in Latin America.

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NOTE: Although the paper and everything written in it is exclusively my own responsibility, many valuable ideas have been suggested by others. In addition to the sponsors and their staff, I would like to express my appreciation for their contributions to Drs. E. Fontaine, J. Grunwald, J. Matos Mar, F. Molina, E. Oteiza and A. Valdés.



## SUMMARY OF DISCUSSION

The discussion during the Conference ranged widely over the many aspects of this complex topic. No attempt was made to maintain a verbatim record. Instead this summary of proceedings was prepared for the interested reader. It focuses on the main points of the discussion grouped by the principal themes of the informal agenda -- Research Priorities, Interrelationships in Research and Coordination and Dissemination -- and generally does not reflect individual views but the composite of views expressed by all speakers on a particular subject.

## INTRODUCTORY REMARKS

Opening the Conference, Mr. Bell (Ford Foundation) said that a better understanding of the role of social science research on development was urgent because immense human needs for food, health care, education and employment were not being met and the underlying problems of development were still inadequately understood. Despite an encouraging growth of social science research capacity in developing countries, only a small part of the intellectual resources needed had yet been brought to bear on major development problems, such as the determinants of human fertility, the measurement of unemployment in less developed countries and rural development.

The Conference was the first of its kind. The task of the participants would be to examine the worldwide system of social science research on development and to consider what ought to be done to improve the system. They would need to discuss such questions as the scale of the system, its control by governments, scholars and donors, its impact on policy makers, how to train research workers and build research institutions, and how to accomplish research on international and comparative questions as well as nationally-oriented research. He hoped that they would be able to arrive at operational conclusions that would be of value, not only for the organizations represented around the table, but for other governmental and private supporters of research. Mr. Bell commended the background papers as stimulating, penetrating, full of wisdom and of intriguing diversity.

## PRIORITIES AND INTERRELATIONSHIPS

The first two days of the Conference were devoted to topics falling under the headings of "Research Priorities" and "Interrelationships in Research" which, while shown separately in the agenda, were combined in the actual discussion.

Mr. Hopper, the discussion leader on the first day, said, in introducing the discussion on priorities, that he had been struck by the lack of divergence of views on long-term priorities among the authors of the background papers. As Paul Streeten had expressed it in his paper, there was a unity of scholarship which cut across international boundaries, although it was necessary to be sensitive to the variations of social circumstances from one country to another.

Mr. Patel, the discussion leader on the second day, introduced the discussion of interrelationships in research by distinguishing among three groups: those providing inputs, those actually conducting research, and the consumers. Inputs could be classified as "real" -- the analytical framework, personnel, equipment, statistics and evaluation -- or "financial"; the latter could come from government, business, institutional or external sources. Research was conducted by universities and other institutions, by governments and by businesses. The consumers of research fell into two categories: on the one hand, decision-makers in government, business, aid agencies and the general public and, on the other, universities and evaluating agencies to which it was an input.

### A. The Donor Point of View

Participants from the agencies who finance research described how the agencies selected research projects and institutions for support.

#### World Bank (IBRD)

The activities that comprised the development research system could be classified according to the lead-time they required. At one end of the scale were training and institution building; it was not always realized that training had the longest lead-time, of the order of 10-25 years. Next came the accumulation of the data base for research and the laying of theoretical and methodological foundations, and, finally, output in the form of policy conclusions. While the Bank's

interest was primarily in the output of the research process relevant to its operations, it found it necessary to engage in projects to improve the data base and the methodological tools for analyzing national economies, sectors and projects. Although it did not provide funds for general institutional support, its collaboration on specific research projects with institutions in developing countries was designed to strengthen these institutions.

Although research had formed an integral part of the Bank's economic work from the outset, it was not until recent years that it had consciously sought to define its role in this field. The research the Bank conducted in conjunction with other institutions was called its external research program, for which \$1,750,000 was allocated in its current budget; this sum did not include the cost of regular staff time devoted to the program. In addition to the external research program, there was a research component in the Bank's regular country and sector studies and the Bank conducted other internal research. About 50% of the Bank's total research took the form of country and sector studies, about 20-30% studies of theory and method, and about 20-30% policy studies; the latter were becoming increasingly important in connection with the country and sector studies.

#### Rockefeller Foundation

The Rockefeller Foundation provided support for social science research under the following programs:

- a. The University Development Program, recently renamed the Education for Development Program, provided "core" support at the rate of \$6-7 million annually for about a dozen universities in developing countries, selected on the basis of their potential contribution to national development. The emphasis of this program was on institution building conceived in terms of long-term (15-year) programs. Appropriations for these programs were usually made for one-year or three-year periods, and were renewed if progress was satisfactory. These programs were formulated and implemented on a basis of mutual discussion, and abrupt termination was very rare. The social science research component might be about 10% of the annual financial support. The directions of the research carried out were largely determined by the institutions themselves.

- b. The Problems of Population Program, conducted jointly by the Rockefeller and Ford Foundations, provided grants for research into social science aspects of population problems to institutions and individuals in many countries.
- c. The Conquest of Hunger Program included some research into the socio-economic aspects of food production.

#### International Development Research Center (IDRC)

The primary objective of the International Development Research Center (IDRC) was to help developing countries to build up the research capabilities and the institutions needed to solve their own problems. Its funding of social science research (about \$5 million annually) was mainly directed to specific projects undertaken by indigenous workers at institutions in the developing countries, rather than in the form of "core" support for the institutions themselves. The main emphasis of IDRC was on rural development in low-income countries. Its principal support for social science research was provided through its Social Sciences and Human Resources Program, which included research into science and technology policy, rural-urban dynamics, and education. IDRC's agricultural and population programs also included social science components. IDRC had focused on research leading to policy conclusions or practical applications, rather than on basic research. The research institutions and workers themselves were primarily responsible for identifying projects.

#### OECD Development Centre

The principal purpose of the OECD Development Centre, which was an integral part of the OECD, was to help in the transfer of the technical and economic experience of the developed countries to the developing countries. Its function was therefore primarily one of communication, and its research, which was problem-oriented rather than academic, was designed to support this primary function. The Centre tried to avoid duplicating the work of the other international agencies. The three main areas of the Centre's research (on which it spent about \$1 million per year) were: economic development and statistics, technology and industrialization, and social development and demography. The Centre could respond to both governmental and unofficial approaches from developed and developing countries. Some of its research, e.g. a study of the debt of the developing countries, and a review of development policy concepts (the latter in the course of preparation),

had been prompted by the Development Assistance Committee (DAC) of the OECD. The Centre sought the collaboration of developing countries in its research projects, and had been active in encouraging the establishment of regional associations of research institutes in the developing countries.

#### The United Nations Development Programme (UNDP)

The United Nations Development Programme was an operating rather than an analytic agency (other UN agencies had analytic responsibilities) which financed various kinds of preinvestment studies and technical assistance to developing countries. Most of its assistance (82%) was provided for projects within individual "Country Programmes"; the balance (18%) was used for regional (15%) and for interregional and global (3%) projects. About \$300 million annually was allocated to these projects. It was difficult to say how much of this \$300 million could be regarded as financing research.

UNDP financing for "Country Programme" projects was largely confined to their external costs. This meant that it could only finance national institutions to the extent that they were engaged to do project work in countries other than their own. Many UNDP projects included a training element which UNDP could finance to the extent that it took the form of fellowships outside the recipient country or was given by expatriates in that country. Training had to be in the specific skills required by the project, and could not take the form of general academic courses. Thus the degree to which UNDP could provide institution-building support for national research institutions was limited. UNDP's regional projects, however, included considerable support for regional institutions. Many country projects had important data-collection components. UNDP was not doing enough social science research, and would welcome suggestions as to how it could do more.

#### U.K. Overseas Development Administration (ODA)

The bulk of official U.K. financing for social science development research was provided by the Overseas Development Administration (ODA) and the Social Science Research Council. ODA's predecessor, the Colonial Office, earlier had provided large sums to build up institutions in U.K. dependencies but after independence aid in this form tapered off and was replaced by support for projects in which U.K. research teams collaborated with research institutes in the developing countries. It was difficult to separate research sharply from technical assistance;

the ODA budget provided about £2 million annually for work strictly classified as research. About £250,000 - £300,000 of this amount was used for grants to research workers at British universities.

While ODA had no formal list of priorities for research, it had given special emphasis to rural poverty and population. It asked applicants for research grants, from both U.K. and overseas sources, to show that their work was likely to have practical value for development within a reasonable period, and to indicate how far it would be carried out in collaboration with research institutes in developing countries. ODA was providing institutional support to the Institute of Development Studies at Sussex University, a research as well as training institution, and to the University of Reading to promote research and training in rural development. It planned to set up an applied population research unit at the London School of Tropical Medicine and Hygiene.

### Japan

Research into development problems in Japan was mainly carried out by the following:

- a. The Institute of Developing Economies (IDE), which carried out research through its own staff with the cooperation of outside experts as needed. Its 1973-74 budget totalled about \$4.7 million, 90% of which came from the Government. IDE concentrated on basic research, most of which was done in Japan.
- b. The International Development Center of Japan (IDCJ), a private non-profit institution, which carried out project-oriented research mainly on behalf of the Japanese Government and its aid agencies, although it had done some work for developing countries and international organizations.
- c. The Overseas Technical Cooperation Agency (OTCA), a government agency which undertook grant-financed project-oriented research in developing countries at the request of the latter.
- d. Three university institutions:
  - i. The Economic Research Institute at Hitotsubashi University.
  - ii. The Center for Southeast Asian Studies at Kyoto University.

iii. The Tropical Agricultural Research Institution.

All three carried out research on their own initiative, mostly in Japan itself, except for the Center for Southeast Asian Studies, which was concentrating on an integrated area study of Southeast Asia covering natural resources, social structure, economic development, political relations and education.

Hitherto Japan had done little institution building in developing countries, but recently OTCA had responded to requests from institutes in Southeast Asian countries by providing fellowships and technical assistance. The Center for Southeast Asian Studies was collaborating with BAPPENAS (Indonesia) in the preparation of an input-output table for Indonesia.

So far, the development research carried out in Japan had been primarily based on Japan's own needs, although much of it was relevant to the needs of the developing countries; however, the translation of the results into other languages presented a serious problem.

### Germany

For historical reasons, there had not been as much social science research on development in Germany as in some other countries. The largest part of this research was commissioned by the Federal Ministry of Economic Development, primarily for the purpose of improving Germany's own development assistance techniques. The bulk of the research commissioned by the Ministry was problem-oriented, intended to meet the needs of the German aid program. A number of studies of the development aid policies of other donors, including the Eastern Bloc, had been made.

The Ministry had recently redefined its research policy so as to give priority to the following six subjects:

- Country and sectoral programming and sectoral strategy
- The measurement of economic and social performance
- The relevance of global development problems (e.g. the limits to growth) to German aid
- The planning and implementation of projects
- Intersectoral problems, e.g. employment, income distribution
- Appropriate technologies

German institutions were as yet largely unaccustomed to collaborate with research institutions in developing countries, but the Ministry was trying to encourage such collaboration. There was a certain amount of institution-building assistance (e.g. to the Universities of Madras and Kabul), but the research element of this assistance was difficult to identify and probably small.

It was difficult, conceptually and statistically, to separate research from technical assistance, and to separate social science research from other research, but \$2 million per year would be a rough approximation of expenditures on social science research.

#### U.S. Agency for International Development (AID)

AID country programs included support for training and institution building that strengthened research capacity within the recipient countries and promoted collaboration between institutions in those countries and in the United States. AID had also contributed institutional support to such international research centers as CIMMYT (wheat) and IRRI (rice). Research and evaluation components formed part of many of the development projects financed by AID. It was difficult to separate AID's support for social science research from its support for other forms of research; of its annual central research budget of \$9 million, probably not more than about \$1 million was devoted to social science research. In addition to this, however, AID's population program included a considerable element of social science research, and \$3 million per year was used to help U.S. institutions to strengthen their capacity to help AID and the developing countries.

The bulk of AID-financed research in the past had originated with and had been carried out by U.S. universities and other institutions, but AID was now -- somewhat to the discomfiture of these institutions -- beginning to assert its own ideas of priorities, and also to insist on their collaborating with institutions in the developing countries. In particular, AID had recently encouraged research into the "interface" aspects of the economic relationships between developing and industrialized countries.

#### Ford Foundation

The Ford Foundation, in accordance with its long-standing general policy of trying to help developing countries to tackle their own problems, had concentrated on developing their capacity

for research by providing support for training and institution building. Although most of this support had been within individual countries, some of it had recently been for regional institutions and for international networks of research workers and institutions. The Foundation was increasingly providing direct support for research programs carried out in institutions of proven capacity. The Foundation had also provided support for institutions in developed countries engaged in development research, e.g. the Growth Center at Yale University and the Institute of Development Studies at the University of Sussex. Its total expenditures in these directions were of the order of \$15-20 million per year.

In these activities the Foundation relied largely on the advice of its field offices, and its decisions were therefore influenced by local views on priorities. The subjects to which they had given most attention were rural development, population, education and development planning and management. The Foundation paid little attention to the dividing line between basic and applied research, supporting either as the needs of a particular case required. While most of their support in the past had been for economic research, they found themselves increasingly turning their attention towards sociology, political science and anthropology.

The Foundation did some "in house" research of its own, into such subjects as the international economic order, public management, and unemployment in developing countries, as a guide to its own activities. In the future, it hoped to support more comparative studies of development experience.

#### B. The Position in Developing Countries

These statements about the activities of donors were followed by statements about the social science research situation in developing areas.

##### India

There were considerable differences among developing countries as regards the capacity to carry out social science research. India was among the more advanced in this respect, having in the last twenty years built up a number of fairly strong and well-staffed research institutions. The best of these institutions were of the autonomous non-university type; in the universities, whose staffs were torn by factional politics and lacked security and incentive, research had

fared badly. The disadvantage of this concentration of the most productive research in non-university institutions was that there was no feedback into teaching.

Apart from the weakness of the universities, there were two main deficiencies in the Indian research system which external assistance could help to fill. In the first place, there were gaps in expertise; it was therefore desirable that students who went abroad for training should concentrate on these deficient disciplines, and return to India to apply them in the field. Secondly, most institutions lacked the material infrastructure of research in such forms as computers and libraries.

India's experience suggested that, in the more advanced of the developing countries, the main objective of donors should be to help to create self-sufficiency in social science research and not, as so often had been the case in the past, to serve their own operational needs, to satisfy pure intellectual curiosity, or to create employment for intellectuals in the rich countries. There should be no further expansion of research in developed countries into the development problems of the developing countries; the lending agencies should commission local institutions to do the research necessary to guide their operations. For every expatriate employed in research, two local workers should be trained. Given adequate training and physical facilities, indigenous research workers were as efficient as expatriates. Only through the "localization" of research could rural development priorities appropriate to the needs of particular areas be established.

More research was needed in India on:

- Multipurpose regional projects
- Technology and employment
- Ex-post project evaluation
- Development administration, especially with respect to multilevel planning
- The organization of the poor.

It was not certain, however, that more social science research would result in a more rapid advance towards the objectives of growth and better distribution in India; during the past quarter-century the chief constraints had been those imposed by vested interests.

### East Africa

East Africa differed from Asia and Latin America in that there was a great need for institution building and the training of competent staffs by such means as fellowships. East African institutes would be unable to do good project research until they had an adequate human infrastructure. Kenya, for example, could use 60-70 more Ph.D.'s in the social sciences.

There was no systematic way of determining research priorities, which were usually established ad hoc and were much influenced by individual interests (very often those of foreign scholars) and international research fashions (e.g. population and environment). Kenya needed a system for determining its own priorities; they were trying to do this by holding periodic "workshops" in which they tried, not always successfully, to get policy makers to participate.

### West Africa

The process of institution building and staff training was still at a very early stage, and there were few institutes engaged in development research. Civil service and university teaching posts had hitherto been more attractive to graduates than research, the value of which was only just beginning to be appreciated. The division of the area into many small and poor countries made it difficult to organize research on a national basis, and some regional organization was needed. Such subjects as water, energy and markets could not be treated on the basis of individual countries; this applied even to Nigeria. It had to be remembered that the gross product of the whole of Africa was only half of that of the U.K. The problem was complicated by the differences of approach between the Francophone and the Anglophone countries. Institutions that had been set up in the former with French technical assistance were essentially extensions of French institutions, without real local roots.

There was an urgent need for problem-oriented research that could provide quick answers to questions asked by governments. The chief constraint was staff; it was difficult to recruit and retain local staff, and for some years to come there would still be an important role for expatriate research workers. It was desirable, however, that expatriates should increasingly fit themselves into the local framework, rather than conduct research on behalf of donors, whose association with local institutions should in future take the form of partnership rather than contract.

## Latin America

To assign research priorities effectively, it was desirable to set up a tentative model based on a set of national or regional objectives, and an explanation, derived from basic research and statistics, of how the development process had proceeded up to the present. Because Latin America generally lacked a summary of the basic research already done, broken down either by disciplines or by problems, most decisions about the direction of research were taken on an unsystematic basis.

Until recent years, there had been little effective research on Latin American problems, except for the illuminating studies made by ECLA in the 1950's on such subjects as import substitution, the structural theory of inflation, and "dependence". Social science students had been mostly trained at universities outside Latin America and when they returned home, finding no demand for research from government or industry, which were unaware of its value, they wrote the kind of theses that would win them Ph.D.'s in the countries where they had studied, choosing subjects and techniques usually irrelevant to Latin American needs. The situation was now changing, however, and although there were still only two or three home-trained economists for every hundred trained abroad, there was much more disposition to tackle relevant problems, and more appropriate training was now given in Latin American universities.

Partly because AID and the Foundations had provided training in the United States mainly for economists, there was too high a proportion of the latter in Latin America compared with other social scientists. There were some sociologists, mainly trained in Europe (one or two to every ten economists), but a marked deficiency of anthropologists, historians and political scientists. There was some multidisciplinary research on rural and urban development, mostly under government auspices.

The fact that there were now only three democratic governments in Latin America had implications for social science research. Up to the mid-1960's there had been great freedom of choice of research topics even though the research in question was to be financed by governments. There had been a big change in the last eight years, and while social scientists were still free to do research within the framework of the prevailing socio-political system, governments would not usually support research that involved a comparison of systems. This situation enhanced the role of external donors and regional organizations, which could provide funds for such

research. It had been found that, rather paradoxically, authoritarian governments did not interfere with externally-financed research, even when it took uncongenial directions. One channel for external funds was the Latin American Social Science Council (CLACSO), to which nearly all the independent social science institutes in Latin America belonged, and which had been formed primarily to safeguard independent research.

C. Following the consideration of this background material, the ensuing discussion was wide-ranging. It is grouped for convenience under seven headings.

#### "Intellectual Imperialism"

Several speakers asserted that many of the research-financing agencies in the developed countries gave priority to research that met their own needs, which were primarily those of the national aid agencies supported by those countries. While this was not wrong, it was not enough, and more weight should be given to the research needs of the developing countries themselves, and to the building up of research capacity in those countries. Donors should take a longer-term view; research performed by expatriates in the developing countries gave quick results, but it was more in the long-term interests of those countries to create indigenous capacity to generate knowledge. It was questioned whether it was appropriate to use funds provided for aid to developing countries to pay institutions or individuals in developed countries for research into development problems. Research initiated in this way tended to create problems of the "intellectual imperialism" type described in Professor Streeten's paper.

#### Freedom of Research

It was pointed out that, because research in developing countries was largely financed by funds which, whether they were provided by external donors or their own governments, were tied to specific research projects, the research institutions were often unable to undertake the kinds of research that they themselves considered of highest priority. The research they were commissioned to do was often of a kind that could be done better by government statistical offices or consultants. It was therefore desirable that governments and donor agencies should provide them with a higher proportion of untied funds, either in the form of endowments, or contributions to current expenses. (The Ford Foundation had provided some endowments; the Rockefeller Foundation had

avoided endowments but had provided funds for current expenses; IDRC looked upon endowments without enthusiasm; and endowments were not considered feasible by either the Bank or AID.) A method sometimes used by institutions to finance training and "free" research was to include an "override" for this purpose in the fees they charged for commissioned research. Because some research institutions depended so much on payments for specific pieces of research, which were received irregularly, they were very short of working capital, a factor donors and governments were urged to bear in mind.

Some speakers thought that it would be dangerous for donor agencies to promote research in "sensitive" fields deliberately neglected by governments, as had been suggested; it was better for them to avoid entanglement in local politics, which would support the charge of "intellectual imperialism". On the other hand, however, one speaker questioned whether it was always desirable to avoid friction, which could be productive, and suggested that it might be easier for foreigners than for local citizens to take responsibility in such cases. It was pointed out that, because both governments and donors tended to avoid sensitive issues, important problems were often neglected; for example, in Africa no research was being done on such subjects as tribalism, power structures and corruption, and in Germany no one had ever compiled adequate statistics of the distribution of land.

A speaker said that the problems -- such as racialism, unemployment and corruption -- that raised sensitive issues were common to both developed and developing countries, and a concerted approach was needed if a world crisis was to be avoided. Although these problems were grave, governments shrank from facing them. What was needed was a new non-governmental "foundation" type of organization which would relieve governments of the responsibility of initiating research into these problems. The proposed "foundation", like the World Wildlife Fund or COMSAT, would obtain funds from both governmental and private sources and make them available to national and regional institutions for research in the critical areas. Its views would carry great weight, like those of the International Court. One speaker, however, thought that such a worldwide organization would be too big and that a regional approach to such problems would be better.

It was noted that problems of "sensitivity" and "intellectual imperialism" were largely confined to the social sciences, which raised policy questions of a kind that did not arise in other fields. No charge of "scientific imperialism" had been levelled, for example, at research into tropical

diseases conducted in developed countries. The more policy-oriented the research, the more sensitive the questions it raised.

Some speakers stressed that a considerable proportion of donors' support for research was untied. Much of what was included under the heading of research was technical assistance, provided at the request of the recipient in the form of training and institution building to create research capacity. In this respect, the donor's role was largely passive except, perhaps, in the selection of consultants.

There was a danger, it was pointed out, that insistence on the untying of support for research would cause it to diminish in amount; its tying to specific projects was a manifestation of the shortage of funds. It would be impracticable, as well as wrong, for research institutions to insist that all financing should be untied, although their demand that some of their funds should be at their own disposal was legitimate. The best solution was for the donors and recipients to consult together to define common problems and to prepare joint budgets.

One speaker suggested that, whether the directions of research were determined by donors, governments, or the research workers themselves, there could be no certainty that they reflected the real needs of the community as a whole, in which the poor predominated.

#### Problem-Oriented versus Basic Research

There was some division of opinion as to the balance in social science research in developing countries between "problem-oriented" and "basic" components. On the one hand, the view was expressed that training in the United States, and the requirements for the Ph.D. degrees of U.S. universities, created a bias towards academic subjects at the expense of the more "down to earth" problems to which answers were urgently needed. Much academic research, it was said, paid little attention to the needs of the policy makers who were its chief consumers. The experience of irrigation projects in the Philippines and elsewhere showed, for example, that it was not enough to provide large-scale irrigation works; the "dam to ditch" problems of how to get farmers to make adequate use of the water had also to be solved. The Ford Foundation had done much to raise the prestige of problem-oriented research in the Philippines. It was stated that IBRD was increasingly financing research of this kind as a component of its loan projects.

On the other hand, it was stressed that theoretical research often led to highly practical results. The research worker's function was to raise and answer challenging intellectual problems without reference to practical applications, which were a matter for others. Although, for example, IBRD had abandoned the study of control theory as having no immediate practical application, in ten years' time it might become highly important. The problems of development called for an increasingly sophisticated analytic approach. It was the more basic types of research that tended to be starved of funds when priorities were largely determined by governments or external donors. There should be consultation between research workers and policy makers with a view to striking a balance.

### Data Collection

A number of speakers emphasized the importance of an adequate statistical basis for research. The problem was especially acute in household level survey data, where past surveys have tended to be too narrow in their coverage. Data collected for research in demography or health or household income and expenditures was too limited to be useful to researchers in other aspects of household behavior, or for the kind of multivariate analyses of interrelated aspects required to understand household phenomena. While it was agreed that the building of this base was in itself a task for research, several speakers expressed the view that, while a research worker should define his statistical needs (much of the data collected in Latin America, for example, was ill-designed to meet research needs), the actual collection and processing of statistical data was a task for specialized institutions, such as governmental statistical offices, rather than for universities or development research institutions; it needed to be organized systematically, and not on an ad hoc project basis. In no branch of research were economies of scale so conspicuous as in the collection and accumulation of data, in which computer techniques were playing an increasing part. While deficiencies of data collection in developing countries presented a serious obstacle to empirical and methodological research, to remedy these deficiencies was expensive and, because it did not offer a "glamorous" field for research, it was difficult to attract financial support. It would be difficult for UNDP to provide financial support for national statistical agencies, it was stated in reply to a suggestion to this effect, in view of its policy of not providing budgetary support for local institutions. It was further suggested that UNDP might provide such support through the intermediary of regional organizations, which could also serve as channels for the dissemination of data-collecting methodology.

### Training

There was considerable discussion of the problem of increasing the research capacity of developing countries in terms of the number of research workers and their training.

In many of these countries, it was said, the shortage of trained research workers was aggravated by the low status accorded to research, the competing attractions of work for governments or university teaching and administration, and the "brain drain" resulting from the higher salaries they could obtain in richer countries; sometimes, indeed, those who went abroad for training failed to return. The situation was improving, however, as more social scientists were trained, either at home or abroad; in Brazil for example, it was expected that research capacity would be doubled over the next four years as 100 new Ph.D.'s returned from training in the United States.

Training of the kind economists and other social scientists normally received in universities in developed countries, it was said, was not as a rule well-suited to the needs of the developing countries. Except in certain specialized institutions, such as the Institute of Development Studies at Sussex or the Williams and Vanderbilt development programs in the U.S., only a small part of the training was directed towards development problems. The requirements for the degree of Ph.D. meant that theses were mostly on subjects of little relevance to the problems of the developing countries. This meant, one speaker said, that research returning workers often went through various phases of adaptation. At first totally under the influence of, say, their U.S. training, they gradually reacted against it as they realized that it did not reflect local needs, and only in the long run attained a more balanced outlook. The IBRD, said another speaker, similarly found that its young economists had to go through an "unlearning" phase which, however, was made shorter by their contact with a "critical mass" of seasoned colleagues, an advantage not usually present in developing countries.

The training situation, however, was also improving. Some U.S. universities were now insisting that Ph.D. research should be on more relevant and problem-oriented subjects demanding field work in developing countries; in some cases, the research supervisor was actually required to visit the developing country in question. Institutions in developing

countries were becoming increasingly capable of training and supervising their own research workers. In Kenya, for example, PhD. students now went abroad for two years of course work only, and did the actual research in Kenya. In future, it was urged, social scientists should be trained in national or regional centers, rather than in the developed countries.

One speaker, however, expressed the view that if the trend towards a more "down to earth" training of research workers meant any dilution of the intellectual rigor of the traditional American approach, it was to be deplored. To be competent to give advice to policy makers, a research worker must be trained in sophisticated techniques; the allocation of funds among competing uses should be based on "input-output" analysis, not intuition; and as more attention was directed towards the non-quantifiable elements of growth, complex models incorporating many variables became necessary.

#### Self-Sufficiency in Research

While there was general agreement that, despite the striking advances of recent years, the social science research institutions in most developing countries were still far short of optimum capacity in terms of number, size, staffing and physical infrastructure, so that it was desirable that donors should continue to provide large-scale support, there was some difference of opinion on what constituted optimum capacity. There was considerable dissent from the view, expressed with particular reference to India, that the goal should be complete self-sufficiency, and that donor support should be directed to this end, and thus largely withdrawn from research institutions in the developed countries which, at the most, should confine themselves to the study of "interface" problems. National self-sufficiency in research was, it was suggested, analogous to national industrial self-sufficiency. The "infant industry" argument justified support in the early stages of institutional growth even though it might mean commissioning from an "infant" research institution research work that might have been done more efficiently by research workers from a more advanced country. But the principles of comparative advantage and international division of labor applied also in this field, and complete national self-sufficiency could only be attained and maintained at a high cost, even in a country like India. Research, of all things, should know no national boundaries, and countries that insulated themselves in this respect, as had China and

Russia, assuming that they could learn nothing from the experience of others, found themselves with frozen methodologies. From the manpower point of view alone, few countries could man both government and research institutions with social science experts of adequate quality. In a country short of such experts, expatriates could play a valuable part, and it was better that they should be in research institutions than in government. A world network of mutually reinforcing research institutions was needed if rapid progress was to be made in the solution of the grave problems which, as had been pointed out, were common to both "developed" and "developing" countries.

The use of aid funds to support developed country based research into development problems was defended. There was a role for laissez-faire in this field, it was claimed, and the independent initiatives of research workers in the developed countries often yielded useful ideas. It was reasonable that aid agencies should finance institutions in their own countries that provided training in development problems. Great care was taken to ensure that research workers from developed countries cooperated with institutions in the developing countries and did not offend local susceptibilities (the publication of research on controversial issues was a particularly delicate matter). It was also desirable that research workers from developed countries should realize that their claims on the time and attention of institutions in the developing countries caused these institutions to incur additional costs for which the sponsoring agencies should allow in their financing: Professor Streeten's paper, however, exaggerated the problems of collaboration, although they had some valid emotional basis which would subside when, as was already largely the case, research workers from developing countries could meet their colleagues from the developed countries on an equal footing. "Intellectual imperialism" was already largely an obsolete issue. While research into the problems of development should no doubt become centered in the developing countries themselves, it would be unfortunate if institutions in the developed countries were seriously weakened; an effective interchange between both areas was desirable. Development economics was already a languishing subject in developed countries, and it would be unfortunate from the point of view of the developing countries if it became a declining one, thus leaving the "neo-classics" alone in the field.

### Inter-disciplinary Research

Speakers noted that, although in most countries social science research had been preponderantly in economics, it was being increasingly recognized that development problems called for an inter-disciplinary approach. A good economist, said one speaker, thought of economics as a branch of sociology. Problem-oriented research had to be multi-disciplinary to be effective; this was particularly true of rural development, as was well brought out in Mrs. Castillo's background paper on experience in the Philippines. Some institutions, e.g. the Institutes of Development Studies at the University of Nairobi and the University of Sussex, had been set up on inter-disciplinary lines. The limitations of economics could be better appreciated when it was housed under the same roof as other sciences; while this propinquity could lead to clashes, they were healthy and productive. Not all the factors that influenced development could be expressed quantitatively, and brought within a linear programming model. Speakers gave as examples of important interdisciplinary research projects a study of the Kainji Dam in Nigeria; a study of income distribution being conducted jointly by AID and the Brookings Institution which combined economics and political science; a study of the substitution of labor and equipment in road and civil works construction sponsored by the IBRD; and a development survey of Southeast Johore. A successful Singapore low-cost housing project had been based on a multi-disciplinary study.

### COORDINATION AND DISSEMINATION

#### Coordination

Speakers emphasized the need for the better coordination of research and dissemination of its results. Speakers noted that while there was coordination among donor agencies, there remained considerable gaps, and there was widespread concern on the part of these agencies about the inadequacy of dissemination. It was difficult, it was stated, to obtain a complete picture of all the social science research underway in developed countries like the U.K., U.S. and Germany. It was easier, perhaps, to obtain such information from the sources of finance than the research institutions or workers, though in some cases grants were made in such general terms that the donor could not say exactly what was being financed until it was completed. In particular, there was a lack of information available on planned research, which would help to avoid duplication of effort; the OECD Development Centre had

made a start towards filling this gap which should be followed up; the information collected should cover the organization of projects as well as their scope. The Centre also published a list of research institutions and acted as a clearing house for information on institutions and specialists within its own fields of interest. Information was also needed about institution-building activities and the research elements of technical assistance and development aid; the coordination of these elements, one speaker suggested, would be a suitable subject for a separate meeting.

Besides the need for coordination in the form of consultation and exchange of information so as to avoid duplication, speakers emphasized the desirability for coordination or, as one speaker expressed it, mutual reinforcement, among donors in the more positive form of a concerted approach to institution building and specific research projects. A speaker suggested that donor agencies should at first experiment with the parallel financing of specific projects, which was feasible in so far as there was some agreement on methods and objectives and, if these experiments proved successful, proceed to joint financing. Research workers from both developed and developing countries should take part in these projects. Another speaker suggested that there might be projects too large for a single donor to finance. Comparative studies of a number of countries or regions would also be a fruitful field for donor cooperation.

It was agreed that regional (or, in large countries, national) associations and networks of research institutions could play an important part in the coordination of the financing and the execution of research in the developing countries. Hitherto the links between research institutions in developing countries and donors or institutions in the developed countries had been stronger than the links among themselves. In this respect, Latin America, with CLACSO and ECIEL, the latter a relatively loose association of about 20 economic research institutes coordinated by the Brookings Institution in Washington, was somewhat ahead of Asia and Africa, although the latter regions now had the Council for the Development of Social and Economic Research in Africa (CODESRIA) and the Asian Association of Development Research (AADR) respectively. In Africa, it was stated, it was difficult to raise funds to finance regional meetings and workshops; the Ford Foundation, however, has in the past participated in financing biannual meetings of CODESRIA and recently has made a grant to support seminars to plan cooperative and collaborative research. IDRC, it was stated, had tried to remedy the lack

of "East-East" and "South-South" coordination among developing countries by supporting the regional associations and helping or organize regional workshops and seminars, as well as arranging periodic meetings of research workers at the project level itself. Such meetings, in which research workers from two or more countries might take part, could be held at the start of the project to define its form, and later to review progress and to assess the results; at the latter stage "consumers" were invited to take part. As a step towards coordination within regions, OECD had asked the regional organizations for lists of research priorities reflecting the views of their members.

The success of regional networks, it was suggested, depended on the leadership of a strong institution or individual, assisted by an efficient secretariat. They could be a means whereby the strong could help the weak, but were more likely to be effective in fields where there were considerable economies of scale. They worked better, speakers said, in the natural than the social sciences, and in established rather than new fields of research. Regional associations could organize joint research projects and arrange their financing with donors; while they were the current vogue among donors, they were unlikely to become important intermediaries between donors and research institutions as far as the financing of other projects was concerned. There was a tendency for networks to become exclusive clubs reluctant to admit new members.

Coordination, some speakers warned, could have defects corresponding to its virtues. Coordination among donors should not mean that they formed a common front. Groups could suppress individual initiative. There could be fruitful as well as wasteful duplication of effort, and a diversity of approaches to the same problem was sometimes desirable, e.g. the IBRD and AID were deliberately financing separate studies of income distribution.

### Dissemination

Dissemination, it was said, was a major problem which was not given enough attention at the planning and design stage of research. There was often poor communication between the producers of research and its ultimate users. It was important to have in mind from the outset a target audience. If this audience consisted simply of research workers in the same field who could be reached through

journals, "workshops" or seminars, the matter was relatively simple. When governments commissioned research, however, they were sometimes reluctant to allow the results to be published, especially if they did not intend to act on them.

When research was not directly commissioned by governments, but produced results relevant to policy, it was often difficult to bring it to the attention of the policy makers. It was noted in this connection that in some countries, e.g. India, government departments often did good problem-oriented research themselves. Politicians, it was said, obtained information and ideas from personal contact rather than reading; they would only read papers if convinced that they were of immediate interest to them. Thus it was often better first to try to enlist the interest of their assistants. U.S. Congressional Committees, it was noted, which commissioned research, summoned expert witnesses, and published detailed proceedings, were effective in bringing the results of research to the attention of both politicians and the public, as was the British type of Royal Commission; in India, it was said, this type of inquiry was now not as efficiently conducted as in the past. It was desirable to express research findings in non-technical language if the attention of policy makers was to be attracted. The importance of bringing the results of research to the notice of the public had also been underrated. Devices that had been used in Kenya to disseminate results to ministries and the public included the appointment of specialists in ministries and in the Institute for Development Studies of the University of Nairobi for this purpose. The specialist "disseminator" at the Institute provided information in simple terms for press, radio, and television, and sent summaries to policy makers. Brochures like those distributed by this institute or the IBRD on their research programs were valuable, and more institutions should follow this example. USAID's Development Digest had a circulation of 10,000 copies; very few of the recipients, however, had returned the postcards in a recent issue on which they were asked to indicate whether they found it useful.

Some speakers defended politicians from the implication that they were uninterested in the results of research. Politicians had to act quickly; they might well come to the same conclusions as the research worker by a less circuitous route; they also had to take account of political and administrative realities that the former was apt to ignore, although economists were now more sophisticated in this respect than 25 years ago. One speaker questioned the validity of the antithesis between politicians and intellectuals. Another said that, during the last ten years, politicians had become

more responsive to academic views, as was illustrated by the adoption of flexible exchange rates in Brazil and the use of shadow prices in project evaluation. A speaker said that there was not enough mobility between governments and universities in developing countries, and suggested that government officials and university staff should exchange posts in sabbatical years.

Regional institutions could do much to disseminate research findings, it was said, especially in Africa, where not all countries had research institutions. On the international level meetings like that of African economists which the IBRD had organized were valuable. It was suggested that UNESCO, whose terms of reference included the social sciences, might play a more significant role in disseminating research results.

Speakers commented on the enormous volume of research material already accumulated. It was questionable how far some of this material was worth dissemination; some system of periodic evaluation was needed, although it imposed difficult, and controversial, responsibility on the evaluator. Much valuable material, however, remained in mimeographed form in the institutions that had produced it, and its existence was not known outside them. As the amount of research in developing countries increased, the problem of dissemination was complicated by that of translation into English, the international language of economics. Costs were an important factor to be taken into account in both the accumulation and dissemination of research information. The cost of recording information on computer tape could run high. In poor countries, the cost of importing learned journals was high, especially if they were airmailed. Microfilming was one solution, although this involved the expense of reader-printer and xeroxing equipment.

In reply to a suggestion that there might be useful social science research and data in the Eastern countries which was not adequately disseminated (translation, it was noted, was an obvious difficulty); it was stated that while a flow of information from Eastern Europe was beginning, it was not easy to obtain information from Russia and China. In the case of China, one reason was probably a deficiency of data-collecting machinery; China was in this respect far behind most of the developing countries.

CONCLUSIONS

The concluding session was devoted to a review of the conclusions reached. It was agreed that although, because of the "amorphous" nature of the subject, it had been difficult to reach clear-cut operational conclusions, there had been a wide consensus on the nature of the problems and needs confronting the institutions and agencies represented at the conference, and on the ways in which they should be approached. It was noted that the principal topics on which such a consensus had been reached were:

- (a) The fact that there now existed in many (though not all) developing countries a nucleus of institutions and trained manpower competent to undertake research relevant to local problems, and the need for donor agencies to shift the emphasis of their support accordingly. While the aim should be, at least as far as fairly large developing countries were concerned, to create sufficient capacity to deal with their own development problems, complete self-sufficiency was undesirable, and there would remain a role for development research in developed countries (not necessarily confined to "interface" problems) and for a fruitful interchange of knowledge and personnel between the two types of country.
- (b) The need for donors to allow in their planning for the differences between developing countries.
- (c) The needs of research institutions in the developing countries for working capital and untied funds.
- (d) The need to strike an appropriate balance between problem-oriented and basic research.
- (e) The danger that research on highly controversial (and therefore urgent) subjects might be starved of funds by both donor agencies and governments.
- (f) The bottleneck created by the lack of an adequate data base for research in many countries; while the accumulation and dissemination of basic data was primarily a task for governments, rather than universities or research institutions, it deserved greater financial support from donors.

- (g) The important part that regional associations of research institutions could play in coordinating research and disseminating its results.
- (h) The need for coordination among donors which, at the minimum, should mean that each was aware of what the others were doing or about to do.
- (i) The need for closer links between policy makers and research.
- (j) The need in most countries for a strengthening of research capacity in the social sciences other than economics.
- (k) The need for training of researchers to be improved, mainly by shifting more of it to developing countries and by developing more appropriate training patterns.
- (l) The need for most research institutions in developing countries to find diversified sources of funds, including governmental and non-governmental sources, endowments or endowment-like sources, international as well as national sources
- (m) The need for systems of responsible evaluation of research capacity and research results, which can help measure progress and identify future needs.