

148. IDDR: Report of the Board Review Panel,
Social Sciences Division

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PREFACE

Those who are associated with the IDRC are accustomed to hectic schedules and crisis atmospheres. Yet even by IDRC standards, the preparation of this Report has been unusual in its pace and its susceptibility to exogenous shocks. The most important peculiarities of the Board Committee's processes on this occasion were: (i) the unusual shortness of the time available for the overall IDDR processes; (ii) the unexpected loss of one of the Committee members at a crucial point in its progress; and (iii) slippage in the timetable of the Division's own IDDR process which led to an even shorter period of time available for the Board Committee's review of the Division's strategic plan and the re-scheduling of its meetings (at considerable personal inconvenience). In addition, this is the first IDDR undertaken in the midst of a period of major internal change - a new Director, an organizational restructuring, and a major rethinking of program directions.

We are very conscious of the fact that the important issues before the Social Sciences Division deserve far more time than we have been able to give them. We believe that the whole future of the Centre may be crucially influenced by the way in which many of these issues are decided.

We also note that, presumably in the interest of Divisional program momentum, Divisional reorganizations have been undertaken and new appointments planned, without waiting for the completion of the IDDR process, consulting with the Board's Committee, or allowing for Board discussions of the Division's plans. The initiatives, in the circumstances, may have been seen as appropriate - even necessary - but they have not been based upon the Board's review of the Division's plans. There is now little need for haste in the Board's "review" of these matters - since action has already been taken which would be difficult quickly to reverse.

For these reasons, we prefer to regard this report as an interim document and recommend that the progress of the Social Sciences Division be subject to continuing Board Review during the next year of its continuing "shakedown". Our report identifies a number of areas in which reconsideration and/or review may be appropriate. These include the optimal structure of the Division; the composition of research and IDRC structures for handling those that overlap Divisional interests; the modalities of decentralization; the role of the SSD in the Regional Offices; and the need for special approaches in sub-Saharan Africa.

We have received complete cooperation in our efforts from all parts of the IDRC, and particularly from the Vice-Presidents, the Social Sciences Division itself, and the Office of Planning and Evaluation. We would particularly like to record our special debt to Anne Whyte, Gordon MacNeil, Doug Daniels, and Andrew Asibey of the IDRC; our senior consultants, Professor R. Albert Berry of the University of Toronto; and Mrs. Frances Stewart of Oxford University and our research assistant, Cindy Germain (none of whom bear any responsibility for the contents of this Report). We also profited from the cooperation of Dr. Francis Keppel and Richard Wilson in our initiation of what, to our knowledge, was the first modest effort at "cross-divisional" Board Review.

The Committee met formally four times - in March, May, October and November 1987. It also enjoyed the opportunity of participating in parts of the Division's annual Ottawa meetings in May. Because of the slippage in the SSD's IDDR timetable, it was not possible to address its strategic plan until late October and November. As we wrote - in December - we saw it as undesirably and unnecessarily long, and in many places still rough; and, to our knowledge, it had still not been discussed by the Division as a whole, or even seen by some of its professional staff. Even as our final draft was prepared in January, we found ourselves rewriting our comments on the Division's statement of objectives in light of a number of (unannounced) changes

introduced since December! (There may well be other late changes that we have not noticed.) We have been unable to make further last minute alterations to take account of important decisions made by the Executive Committee at its January meeting.

Despite the problems of the SSD - which we believe to be considerable - we have encountered nothing but keen interest, openness, dedication and courtesy throughout our review activities. We would like to begin by recording not only our thanks but also our respect for the qualities of the IDRC staff, and particularly of the SSD.

We hope that the frank comment and criticism which we at times offer will be taken as indicative of the high respect in which we hold the IDRC and the IDRC's future potential. We have done our best to be constructive rather than merely polite. The IDRC's role in worldwide developmental research is too important for us to do otherwise.

J. Hardoy
G.K. Helleiner

SUMMARY AND RECOMMENDATIONS

1. Introduction

The IDRC's support for social science research in the Third World has helped to broaden understanding of the social, economic, and political forces that influence the way in which a vast number of people, especially the poor, live. The networks of researchers which have been formed in the various regions, as well as the numerous publications and dissemination of the results of a broad range of projects/activities show how even with limited resources, the Centre can help find solutions to the most pressing human problems. But more can be achieved.

Emerging from a period of uncertainty, the Division is now at a critical turning point. With its new leadership, and a fresh mandate for staff recruitment, it could be at the "leading edge" of IDRC activities and in the vanguard of worldwide research on poverty issues. We believe that the SSD should aim high.

Social scientific research differs from other types of research in development. Among the differences are: topics are more sensitive, results are less certain, projects are smaller, and research activities are often more useful when closely linked to research in other fields. These differences must be recognized and addressed as IDRC expands its efforts to support both multidisciplinary and more traditional social science research in the Third World. We have endeavoured to offer recommendations that do so.

2. Mission and Objectives

While we agree with the broad intent and spirit of the SSD's statements of mission and objectives, we believe that specific refinements are needed to ensure clarity of purpose. We support the addition of the objective of "sustainability" to the mission statement, and would suggest greater emphasis on indigenous determination and on culture. In the formal statement

of Divisional objectives we make the following suggestions: (i) equity in development should be an explicit objective; (ii) the intention to increase capacity for multidisciplinary research should be added; (iii) the precise meaning of one of the stated objectives (5) should be clarified, or it should be dropped; (iv) the objective of facilitating South-South exchanges of information, research results, etc. should be added.

3. Organization

We see little scientific logic in the proposed re-organization of the Division into 3 programs: Human Resources and Social Development Program (HRSD), Regional Development Program (RD), and Economics and Policy Analysis Program (EPA). Overall, in administrative terms, we perceive two broad categories of SSD activity planned for the next 4 years - those undertaken collaboratively with other Divisions and those undertaken independently (see Table 2 on P. 23). Administrative re-organization reflecting this distinction might make sense. We urge further consideration of this issue.

4. Priorities

The suggested list of activities to be covered under the new programs is too long, and particularly the proposed activities in the new Regional Development program, too broad. The Division should seek gradually to focus more sharply on areas for priority support. We suggest that approximately 10% of SSD's total budget be set aside to cover projects, especially small scale projects, that do not fit neatly under such priority headings. Another 5% or so should be reserved for regionally determined projects.

We applaud the new IDRC and Divisional initiatives in Women-in-Development and Public Policy. In the area of Resource Management and Environmental Protection, however, we suggest a more cautious and Centre-wide approach rather than the suggested major expansion of SSD activity. We have serious reservations

about the new areas suggested if further resources become available, and urge consolidation of existing programs instead.

The IDDR proposes significant cutbacks in SSD support for Science and Technology Policy (STP), and for Education. STP research should continue to play an important role in the IDRC, be managed on a Centre-wide basis and focus on fewer topics as indicated in the full text. We agree that support for Education research can be reduced, and offer some alternative suggestions as to priority topics.

5. Multidisciplinary Research and Interdivisional Collaboration

Pioneering in multidisciplinary research could be a major IDRC contribution. Our investigations suggest that such research is usually best achieved through formal, rather than informal, interdivisional collaboration. We also found an unfortunate IDRC weakness in economics that SSD should repair.

In addition to Women-in-Development, Public Policy, and Nutrition, there is scope for IDRC innovation in multidisciplinary approaches to Health Behaviour, Resource Management, Environmental Protection, and Science and Technology Policy and Management. In the case of Health Behaviour, it may be best to integrate planned research activities in a unit located in the HS Division.

There is also scope for joint research on agricultural and rural development. While we agree with the Centre's support for agricultural activities which focus on specific products that can yield concrete results for small farmers, such an approach does not sufficiently take into account the social, political and economic factors which often influence agricultural productivity. The impact of agricultural innovation is most effectively analyzed through complementary research by agricultural and social scientists. The SS and AFNS divisions need to review periodically their understanding of agrarian processes in order

to identify ways to ensure that the Centre's overall contribution to agricultural research achieves maximum results.

6. Decentralization

We support wholeheartedly the decision to expand SSD's representation in the regional offices, because it is our conviction that the benefits far outweigh possible costs or disadvantages. We urge a review of regional program officers' remuneration, RDs' and POs' spending authority, and other IDRC administrative practices that seem expensive and cumbersome.

We applaud the Division's proposal to delegate signing authority to Regional Directors for research which is collaborative in nature and for which regional office staff are available for project development and monitoring. We recommend that the SSD allocate about \$1 1/2m of its total budget per year for such initiatives. We also urge expansion, at least on an experimental basis, of budgetary authority for projects developed by groups of program staff in the regional offices. There should be a significant increase in the amounts that can be authorized by program officers.

We suggest the recruitment of a small number of young Third World professionals, for periods of two to three years, to help program officers and the RDs with the preparation of background materials and monitoring of SSD projects. This strategy would give these researchers much needed skills in research administration, including familiarising themselves with the IDRC's approach and practices.

To discern appropriate regional priorities often requires a greater degree of involvement of those who are most familiar with the particular needs and problems of the respective regions. We recommend that the regional offices use top researchers in their region and elsewhere to assess overall regional priorities.

7. Funding Strategies

SSD projects are, on average, significantly smaller in size than those of other divisions. There are good reasons for this. We nevertheless recommend that the SSD experiment with larger, more program-style grants. Such grants might support particular institutions' research in more broadly defined areas. We also recommend relatively greater Board attention to the SSD projects not elaborated upon in the project docket.

Networks and small grants are useful mechanisms, particularly for developing activities in the poorest regions. For greatest effect, these must frequently be Centre-administered. We recommend that the SSD network and small grants activities in Africa be exempted from IDRC guidelines with respect to Centre-administered projects.

We support the Division's plan to improve links with NGOs, particularly those in the Third World. These organizations are playing an important role in terms of helping to adapt technologies and other innovations to the needs of very low income groups.

There is an obvious logic in expanded IDRC effort in sub-Saharan Africa. However, budgetary reallocations such as those planned, are not necessarily the best response to Africa's research needs. We recommend that IDRC launch a Centre-wide review of the practicality and productivity of its current funding and administrative practices in sub-Saharan Africa before it significantly expands its expenditures there. The review should consider such issues as the divergent needs of the various program divisions, the potential for increased inter-divisional collaboration, decentralization, and the role of the regional offices.

8. Program Officers' Morale and Working Conditions

Morale has been undesirably low amongst SSD program staff. IDRC and Division management should endeavour to create a professionally more satisfying working environment for POs.

Heavy and increasing workloads will be somewhat eased with the filling of existing staff vacancies. Still, Program Officers spend too much of their time on routine administrative work. We propose that there should be: (i) greater use of secretarial and support staff for routine administrative tasks; (ii) reduced nursing of projects, where appropriate, especially in countries where the researchers have attained considerable experience; (iii) greater use of outsiders to help review proposals; and (iv) reduced duplication of efforts.

We are concerned that very little attention is paid to the professional development of SSD program officers. The situation should be rectified through better incentives, research assistance, and sabbaticals.

We concur with the PPR IX (1987) statement that "program staff time allocation is the most significant strategic decision made by the Centre," and support the Division's stated intention to deploy a large number of its staff in the regional offices. If budgetary authority is not also decentralized, however, much of the potential benefit from this change will be lost.

We recommend that IDRC-wide recruitment procedures be reviewed to ensure that they are both efficient and equitable, in particular with regard to the hiring of well qualified and competent staff from the regions to work in the regional offices.

9. Project Evaluation and Follow-up

Premature abandonment of projects and failure to gain full advantage from experience are both matters deserving greater attention.

The PCR system is inadequate for evaluation purposes. We recommend that the SSD systematically undertake in-depth evaluations of "clusters" of projects within a related area, and undertake evaluations of selected projects several years after they have been completed.

More creative mechanisms could be developed to facilitate the dissemination of research results. We recommend such

approaches as: (i) sponsorship of studies that synthesize world-wide, regional or sub-regional research results and draw lessons therefrom in selected problem areas; and (ii) use of regional centres of excellence to disseminate research results.

I. INTRODUCTION: THE SOCIAL SCIENCES AND THE IDRC

(i) Social Sciences and Development

We begin this review with some broad reflections on approaches to development, the concerns of social scientists, and the role of the IDRC. We do so in the belief that the role of the SSD can only be assessed and planned if it is seen in its appropriate context.

The "macro" issues with which social scientists attempt to grapple include the very meaning of "development" itself and thus some of the central elements in the determination of Centre directions. In many of their activities, not only is scientific "truth" somewhat elusive but eclecticism of approach and recognition of the diversity of human experience are also positive virtues (as demonstrated by the damage wrought within the World Bank's Research Department in recent years by an effort to ensure commonality of approach). At a more micro-level, the role of the effective social scientist is more frequently to reduce the risk of egregious error rather than to establish "correct" answers. The social scientist is also less likely than other scientists to be impressed by the desirability of responding to governmental priorities, since the origins of those priorities are themselves matters for analysis rather than starting points in research.

An approach that concentrates on purely technical solutions to problems which have profound political, cultural and ecological causes risks missing or misunderstanding the major forces that, both regionally and worldwide, lie behind them. As IDRC strives to help peasants, rural workers, recent immigrants, urban proletarians and minorities threatened by racial or religion discrimination, it must strive to understand who these people are, the causes of their postponed integration into national and regional societies, and their dormant potential. It is not enough simply to cross the lines that separate different disciplines, which so many within the IDRC are now committed to doing, if we do not reflect upon the roots of social and economic

change, and the political forces (local, national, and international) that can encourage, delay or push back development processes that benefit the low income groups - both rural and urban - which are targeted for IDRC support.

A fundamental problem in the history of many Third World nations is the longstanding and growing concentration of economic and political power within a small social group in a small fraction of their territory. The concentration of the official culture in the national capitals - usually the former key colonial cities - has often postponed attempts at cultural advancement outside them, and even challenged the collective existence of individual nations.

The contributions of the inhabitants of these peripheral regions to the culture of their countries, which is the same as saying to the development of their countries, have been hidden by the messages that emanate from the national centres; the political, economic and social structures in these countries impedes entire populations from elevating themselves to the category of citizens, with equal rights and opportunities. Often, even researchers and professionals in developing countries do not themselves have accurate visions of their own countries in their entirety. It is not possible to aspire to the transformation of countries with 30 million (or 100 million!) people into nations with the same number of citizens without knowledge of how the country and its institutions function, the rights each citizen has and the means through which he or she may make those rights prevail.

Many will say that there are other, more dramatic, and more urgent needs: feeding people; avoiding the death of millions of children; controlling plagues and endemic diseases; avoiding the misuse of irreplaceable natural resources; providing people with potable water, and incomes, shelter, and education; and, while achieving those objectives, basic as they are, respecting people's rights and freedoms. There have been improvements in some of these dimensions and there can be more, often at

comparatively low economic and political costs. But the number of failures in development programs and projects has also been great. Many would say that we have failed to achieve meetings of minds, and, even more, the changing of minds, both in the developed and in the developing countries. Many "failures" can be attributed to outsiders' misunderstanding of the motivations of recipient groups, and insensitivity towards the relationship between culture and landscape which plays such an important part in the life of many of the poorest people. We question whether the Third World nations will ever surmount their more pressing problems in the absence of a wider understanding of the political ideas and the uses of institutions - local, national and international - in their own past and present, particularly when the authoritarian spirit of the "colony" still so pervades the daily life of their people.

Even in terms of the most "urgent" objectives, the question has also often been raised of whether the benefits of IDRC-funded (or other) research are likely to accrue to the very poor. That they may not is, of course, always a legitimate worry, and may be fostered by the appreciation that many technological improvements in sectors like agriculture and manufacturing cannot be directly used by the poor. Health, nutrition, shelter and primary education intervention clearly, more easily, can. Much of the potential contribution of social science research lies in understanding the impact on the poor of broader elements of public policy, e.g. macroeconomic policy under external constraints. Historically the very poor have been pulled out of poverty much more by their absorption into other activities than by raising the productivity of those activities in which they were originally found. Making micro-enterprise bloom through technological innovations, one-by-one, is a worthy objective, which should attract IDRC support. But its attainment is not likely, by itself, to provide the levels of productivity which poor economies ultimately need.

IDRC and its Social Sciences Division have made a major contribution to selected areas of knowledge for development. They have helped to form networks of researchers within the same disciplines, and across disciplines, in different regions. They have brought new encouragement to groups already engaged in research for development and helped to form new groups. This has been done quietly, patiently, with humility, respecting the specific circumstances in each country and even in regions within countries. IDRC and the SSD in particular have often worked in very difficult and sensitive circumstances, consistently making decisions with courage, foresight and respect for human rights. While thereby becoming one of the most respected agencies of its type in the world the IDRC has also won additional respect for Canada in the developing countries.

Research for development is potentially so broad as to incorporate almost all research activities. To select what is good from what is bad and what is urgent from what is less urgent, to steer the interest of researchers and development agencies to crucial themes and geographical areas while showing the interconnectedness of development processes, is a task for all. It is particularly, however, the task of the social scientist.

(ii) Social Sciences in the IDRC

Social scientific considerations should be integrated, as appropriate, with more technical development-related research in agriculture, health sciences, earth sciences and engineering. Even within the social sciences there is room for much more interdisciplinary cooperation - say, between economists, political scientists, anthropologists, sociologists, and historians - than there has typically been in development research. Obviously, research cannot and should not always proceed in a multidisciplinary mode. The IDRC's own organization reflects the Board and management view that traditional disciplinary boundaries are still usually the best basis for

organizing development research. Yet there have been growing efforts to utilize professional skills in the IDRC and in the developing countries in a more unified and holistic way. (We have more to say about this in section V.) It is now generally agreed that the contributions of social scientists to IDRC activities should not be seen as exclusively those made within the boundaries of the SSD. The potential contributions of development lawyers, historians, and even the social scientists usually more fully represented in development discussions have been insufficiently recognized in much of the IDRC's work--perhaps because of the sense of urgency that often prevails in its decision-making.

It is striking that in one of the most exciting of the development research institutions supported by the IDRC - the BAIF (Bharatiya Agro-Industries Foundation), Institute for Development Research, Pune, India, there is no separate social sciences division. Social science considerations are fully integrated in functionally organized divisions. In the IDRC, some aspects of such integration are found within the AFNS Division where considerable use is now made of expertise in agricultural economics in the preparation of its programs and projects. The HS Division has only one social scientist on its professional staff, but is clearly relying more on social science inputs than before. The Earth and Engineering Sciences Division has none, but also needs such inputs. It therefore falls to the SSD to play something of a "service" role within the IDRC at the same time as it develops its own independent research activities. One of the ways in which it can do this is to participate in, or lead, IDRC-wide thrusts in such areas as Women-in-Development, Public Policy, and Nutrition. We applaud the introduction of these experimental Centre-wide initiatives, and urge their cautious expansion into new areas like Environment and Resource Management, Science and Technology Policy, Human Settlements and Habitat, and specific Regional initiatives.

(iii) The Special Characteristics of Social Scientific Research

Important research in the social sciences has been achieved, overwhelmingly, via a "bottom up" generation process. Areas of focus have typically been chosen and defined by the researchers themselves, and they have normally worked alone or with only one or two colleagues. This pattern may be thought simply to reflect the structure of universities, since most of the work takes place in that context. More probably, it has been the other way round: the structure of university research reflects the advantages of this style. Most successful non-university research centers are fairly small, collegial, and non-hierarchical in their essential character. Larger development research operations, such as those of the World Bank, have achieved only partial success. The Bank's research department has been able to take advantage of well-qualified staff, unusual access to data-bases and unusually strong financial support, but its output has often suffered from excessive rush to finish products, and, most recently, a turbulent period of non-collegiality. No synthetic works of major importance have come from the Bank despite its major contributions in other ways.

The fact that social science research tends to be carried out on a more individualistic "small scale" basis than is research in some other areas and that research planning in the social sciences tends also to be "bottom up" suggests some potential problems from IDRC's increasing size for the SSD. Another special feature is the greater difficulty in pinpointing areas of highest research priority in the social sciences and in evaluating work and impact ex-post. There is usually a lack of consensus on many major issues in the social sciences. Most good research has a product which is in no way so straightforward as the discovery of a better strain of wheat or a new health technique. The inevitable data and conceptual imprecision and/or ambiguity with which the social scientist has to live means that the most impressive research contribution will not normally convince its potential users quickly, will often require

complementary research to test the robustness of results, and even if it persuades the experts, may only filter through to the decision makers years later. Often, valuable individual pieces of research provide only one element in the solution of a puzzle, and so have no policy implications by themselves at all. Some such features are present in all types of research, but they characterize social science research much more than most others.

Another difficulty facing social science researchers is the normal absence of consensus as to what is known and what the key questions are. (There is, of course, frequently consensus on some matters, but not on many others.) The contrast with agricultural sciences is striking; there, a world network of researchers and institutions helps to define the frontier at each point of time. In the social sciences the evidence which would ultimately settle an issue might be available now but the consensus that the issue has been settled might emerge 10 years hence, if at all. Often the lack of consensus reflects divergent ideological proclivities or values, a more serious problem than in most other branches of science; on other occasions it reflects differences in the capacities of researchers; and certainly it reflects the thinness with which researchers are spread across a myriad of issues. It means that honing in on the key areas and defining the frontier in those areas in such a way that new efforts will not simply replicate what has in fact already been shown (but not widely accepted) requires experience and good judgement as well as sheer talent.

The payoff to social science research is therefore often hard to assess. There are, of course, clear examples of "excellent" research projects; and there are others, which by common consent, misfired. But quality can often only be assessed via somewhat subjective evaluation by a variety of competent specialists of the extent of evident "learning by doing", and of direct or indirect policy impact in the relevant countries; and good assessments of this kind have been rare.

Those uninitiated in social science research tend to assume it is simpler, shorter-term and clearer cut in its answers than it is, and hence tend to have unrealistic expectations as to how quickly good research is done. They also underestimate the costs of shifting back and forth among areas. Most of the best products come from people working in much the same area, and thereby building up understanding of it, for at least a decade.

For all these reasons, it cannot be assumed that the best organization for other types of research is necessarily the best for social sciences research. When social scientists are not well represented in management councils for extensive periods the particularities of their approaches may seem, or actually be, underplayed or neglected. Across-the-board management styles and rules may be introduced by management with the best of intentions (usually for coherence and comparability), without appreciation of their possible negative impact upon those whose different research "cultures" are not well understood. The more "top-down" and "big-project" style of physical sciences fits only awkwardly the circumstances of a lot of good social science research. Development researchers should recognize this as an example of potentially inappropriate technology transfer.

Increased size has necessarily curtailed the degree of collegiality within the IDRC as a whole. The need for and possible dangers of more "top-down" decision-making and the question of the degree of independence of each division from the others and from top management call for thought. Following the usual model for successful social science research activities would require that the main role in identification of key research areas and methodologies would lie with the division and its members, particularly those in the regional offices, and that the best results would be achieved if a high level of collegiality and decentralization were possible in spite of greater size. Suspicion and misunderstanding will be at a maximum when social scientists feel that their interests are not being defended well at the level of senior management. Respect

and confidence are important for "top-down systems" to work, and both are likely to suffer when management does not speak the same language as the staff, or defend its decisions in the professional research terms that program staff most respect.

(iv) The Social Science Division in the IDRC

The recent history of the social sciences and the SSD in the IDRC has not been a happy one. Prior to the arrival of its present Director late in 1986 the Division had first been led for several years by an academic administrator who was not a social scientist, and thereafter, for two more years, been formally leaderless and managed, de facto, by a Vice-President who was also a non-social scientist. For many years the leadership of the SSD consequently carried less-than-complete confidence either in IDRC's senior management or within the professional ranks of the Division itself or, sometimes, both. Social scientists in the IDRC saw the SSD's share of the total budget fall, their own staffing - even of replacements - put on "hold", and their interests inadequately represented or defended within the organization. They have felt themselves both misunderstood and mistreated, while at the same time they (properly) continued to see their potential contribution to IDRC's activities as critically important. SSD programs and projects continued to maintain very high levels of quality throughout these difficult years. Imaginative and innovative research sponsored by the Division in many fields has earned worldwide respect for the IDRC's social science activities. But it is small wonder that staff morale in the SSD has not been high (see section VIII).

It would be pointless to try to assign blame for the unfortunate aspects of recent experience in the SSD; but the

Board must take a fair share of it, for not moving more quickly to arrest a situation that was permitted to deteriorate for much too long. The SSD is now at a critical potential turning point - with new leadership, a fresh mandate for staff recruitment, and the results of an IDDR. We feel that the Board's responsibility for helping the SSD to "get it right" in the next four years, after the relative wilderness the Division has lived through since the last IDDR, is unusually great. We believe that this may be the most important Board review of an IDDR since the IDDR process began.

II. MISSION, OBJECTIVES AND CONSTRAINTS

(i) Mission

The formal Mission statement of the SS Division (see box) broadly conforms to that of the IDRC as a whole (PPR VII). In a few respects, however, it may appear to deviate:

- (a) it speaks of particular emphasis on "meeting the needs of those who are most at risk and whose choices are most limited: the rural and urban poor, women and children"; PPR VII spoke instead of the "problems of poverty" in order to avoid "connotations of paternalism and charity inherent in 'the poorest people'" (PPR IX-p. 6), and consistency in this respect is probably desirable;
- (b) it adds sustainability to the objectives of development, an addition which we support and which might well be added to the IDRC's statement on its conception of "development";
- (c) it does not place the same emphasis upon indigenous determination of objectives as does the IDRC's statement, and perhaps it should do so.
- (d) we were disappointed that the Mission statement of the Social Sciences Division contained no reference to indigenous culture - except to the extent that it may be implicit in the statement of intention to encourage "a holistic approach to development"; the IDRC's one-sentence statement on "development" includes reference to "independence of spirit, pride in indigenous culture and respect for human rights" (PPR VII), and the SSD seems to us to have particular responsibilities in these spheres.

MISSION AND OBJECTIVES

Mission

To support indigenous efforts in developing countries and regions to achieve, and participate in, sustainable and equitable development, through the promotion and application of policy-oriented research in social sciences; and through collaboration with agricultural, health and other sciences to encourage a holistic approach to development, with particular emphasis on meeting the needs of those who are most at risk and whose choices are most limited; the rural and urban poor, women and children.

Objectives

1. To contribute to sustainable economic growth, income generation and employment, social participation, self-reliance and well-being for individuals, communities and developing regions.
2. To promote, and increase the capacity to undertake, high quality and policy relevant research in social sciences in developing countries;
3. To focus attention on the research and policy needs of the rural and urban poor, women and children, and the communities in which they live;
4. To assist developing country researchers and policymakers in the identification and analysis of developmental problems and needs, and in the establishment of research priorities;
5. To encourage and facilitate an understanding of the interdependence of development sectors, within a country, and of the global linkages in economy, culture and environment;
6. To encourage and fund collaborative and multidisciplinary research and research-supporting activities, in addition to good disciplinary research;
7. To improve the capacity of the Centre's recipients to disseminate research results effectively in order to increase their utilization by relevant bodies, such as grass-root and community-based groups, public and private organizations, local and national governments, other researchers and other countries;
8. To support collaborative research between Canada and developing countries and to encourage Canadian researchers to be more concerned with development issues.

(ii) Objectives

The statement of objectives (see box) is obviously influenced by the Mission statement. It incorporates sustainability objectives, and, as we have said, we believe this to be desirable. It also could be interpreted, however, as being rather more "top-down" (especially in objectives (5) (6) (7) (8)) and less culturally sensitive than we believe desirable (especially in objectives (6) and (7)).

We would make some further detailed suggestions:

- (a) equity in development figures in the Mission statement and should therefore probably be explicitly referred to in objective (1), as sustainability is;
- (b) to objective (2) there should be added an aspiration to increase capacity to undertake multidisciplinary research in which social sciences are a part;
- (c) we have experienced some difficulty in understanding the precise meaning of objective (5) - "to encourage and facilitate an understanding of the interdependence of development sectors, within a country, and the global linkages in economy, culture and environment"; this should either be dropped or, if it relates to specific research endeavors rather than merely attempting to show awareness of current "buzz-words", clarified;
- (d) we believe that the objective of facilitating South-South exchanges of information, research results, etc. should be added - perhaps to the stated objective on diffusion of research results (objective 8).

We should like to add some broader comments - beyond those on the formal statements of mission and objectives in the IDDR - on the role of the SSD in the current international context.

Social scientific research, oriented specifically to the problems of poverty, has not been in the ascendancy, to put it mildly, in recent years. Changes in ideological fashion in Washington and elsewhere, cutbacks in UN budgets, and a relative downgrading of poverty and distributional issues in academia, have together left a major research and monitoring "gap". Yet poverty has been an increasing problem, particularly in Africa and Latin American during the economically turbulent 1980s.

Non-governmental organizations, UNICEF, and scattered individuals and groups in other official organizations and universities are still active, and many Third World governments are desperately seeking answers. But none of these initiatives are well supported - either in intellectual or in financial terms. Many Third World governments, particularly non-elected ones, view social scientific research and the dissemination of its results in these subject areas as threatening to the status quo. The social sciences and humanities therefore frequently wither in universities subjected to repressive regimes, only gradually to re-emerge when more open political conditions return. The Social Sciences Division of the IDRC could, we believe, not only be the "leading edge" of IDRC activities (as Joe Hulse was fond of saying) but, even more, it could be in the vanguard of worldwide social and economic research and monitoring on poverty issues. (This is not to suggest that all of the SSD's projects should be directly linked to poverty issues.)

The shared vision for this role and the staff time and morale to play it effectively do not unfortunately yet exist. But we retain the hope that this role is nevertheless possible during the four-year planning period we have been asked to consider, and we are reluctant to settle for second best or third-best outcomes for the sake of achieving greater order and

stability in the shorter-run. Perhaps our ambitions for the SSD are too high. But better too high than too low.

(iii) Budget and Staff Constraints

We take the budget constraint as given, and therefore forgo the usual practice of recommending increased resources for whatever Division is under review in the IDDR process. We note, however, that the SSD share of the IDRC budget has fallen significantly in recent years and there seems to be no plan to restore its relative share. This reduced share for SSD was, we believe, not the product of conscious Board decisions as to the appropriate ongoing size and role of the Division. Rather, it was, in large part, a reflection of the temporary internal difficulties of the Division. The role of the SSD in the IDRC over a longer period during which it is expected to enjoy stable and effective management is what is now being reviewed.

The SSD program staff has been severely curtailed, first by the overall "freeze" and then by the "hold" on Divisional recruiting. It is with relief that we note that the SSD can hire again. The appointment of nine new program officers in the next four years will make a major difference to the SSD's capacity to do an effective job, particularly so if their capacities and location are appropriate (see sections VI and VIII).

III. ORGANIZATION AND PRIORITIES

(i) Proposed Reorganization

We have had difficulty understanding the scientific logic underlying the three-fold division (excluding the Women-in-Development unit) of the Social Sciences Division's research activities into 3 programs: (i) Human Resources and Social Development (HRSD); (ii) Regional Development (RD) and (iii) Economic and Policy Analysis (EPA). Population and education research, formerly independent units, have been grouped together under the Human Resources and Social Development Program. The Science and Technology Policy unit (including the Energy component) is now listed under the "Economics and Policy Analysis" Program. All three of the new programs are inter-related and there are areas of significant overlap among the "sub programs" listed under the new headings (e.g., subjects like employment, participation, service delivery, policy analysis, etc.). One of the stated reasons for consolidating the previous 5 Programs into these 3 is to achieve a reduction in the number of sub-programs. The number of "sub programs" does not seem to us, however, to have been reduced as claimed. Rather, existing sub-programs have been repackaged and sometimes consolidated so as to create an illusion of reduction. Indeed the list of potential areas for SSD activity, listed in Table 1, seems to us to have expanded.

Table 1
SSD Research Topics in IDDR

- A. HUMAN RESOURCES AND SOCIAL DEVELOPMENT Program
1. Human Survival and Development
 - a) Child Survival and Development
 - b) Adolescent Development and Participation
 - c) Social Behaviour and Mobility
 - d) Education for Health
 2. Population Dynamics
 - a) Mortality
 - b) Fertility and Family Planning

- c) Migration and Population Distribution
- d) Demographic Structure
- 3. Education Systems and Processes
 - a) Access and Efficiency
 - b) Quality and Effectiveness
 - c) Relevance and Responsiveness
 - d) Management Organization and Planning
- 4. Education, Work and Production
 - a) Process and Content of Education for Work
 - b) Transition between Education and Work
 - c) Qualifications and Performance
- 5. Community Participation and Knowledge
 - a) Community Participation and Social Services
 - b) Indigenous Knowledge and Culture
- 6. Special Activities
 - a) Nutrition
 - b) AIDS
- 7. Collaboration with Other Programs and Divisions

B. REGIONAL DEVELOPMENT Program

- 1. Shelter and Service Delivery
 - a) Shelter Policies and Programs
 - b) Land Use for Shelter
 - c) Shelter Finance and Ownership
 - d) Shelter Delivery
 - e) Financial Management for Services
- 2. Food Production and Distribution
 - a) Increase in Food Production
 - b) Food Distribution
 - c) Food Markets
 - d) Urban Growth and Agricultural Land
- 3. Resource Management and Environment Protection
 - a) Population Pressure
 - b) Plantations, Labour and Population Displacement
 - c) Resource Depletion
 - d) Energy and Environment
 - e) Impact Studies
- 4. Employment Generation
 - a) Employment and Mobility among the Disadvantaged
 - b) Employment Creation Programs
 - c) Informal Sector
 - d) Services to Informal Enterprises
 - e) Regional Development Programs
- 5. Regional Disparities
 - a) Agricultural Development Programs
 - b) Regional Economic Centres
 - c) Economic Decentralization
 - d) Infrastructure

6. Integrated Development
 - a) Integrated Rural Development
 - b) Integrated Urban Development
 - c) Grassroots Movement
 - d) Participatory Approaches
- C. ECONOMICS AND POLICY ANALYSIS Program
1. Macro Management and Finance
 - a) Macro Policies
 - b) Resource Mobilization
 - c) Financial and Real Economy
 - d) Parallel Economy
 - e) Debt
 2. Trade and Technology and Industrial Policies
 - a) Trade and Protectionism
 - b) Commodity Marketing
 - c) Incentive Structures
 - d) Foreign Direct Investment and Technology Transfer
 - e) Commodity Prospects
 - f) Regional Integration
 3. Labour Markets and Employment
 - a) Information Systems
 - b) Institutional Factors
 - c) Technological Change
 4. Markets, Institutions and Resource Allocation
 - a) Market Structure
 - b) Pricing Policies
 - c) Voluntary Associations
 - d) Marketing
 - e) Planning
 5. S & T Management
 - a) S & T Institutions
 - b) Technology Choice
 6. Special Activities
 - a) Economic Analysis
 - b) Public Policy and Participation
 - c) Environment and Economics
- D. WOMEN IN DEVELOPMENT
1. Industrialization
 2. Informal Sector
 3. Agricultural Production
 4. Social Participation
- E. OTHER CENTRE-WIDE ACTIVITIES
1. Pesticides
 2. Participatory Research

F. POTENTIAL NEW UNITS TO BE ESTABLISHED WITH ADDITIONAL
RESOURCES FOR THE DIVISION

1. Environment and Economics Unit
 2. Range Land Unit
 3. Land Development Unit
 4. Shelter Unit
-

While we see little scientific merit in the proposed reorganization, it may not do very much harm either. We do see the potential merit - on administrative grounds - of reducing the number of SS Programs. Such a change may make it possible to streamline approval procedures, focus future research activities, and ease the politics of achieving balanced SS representation in regional offices. The present headings - and their sub programs - do not, however, inspire confidence that the SSD has as yet acquired clear enough program directions. And we doubt whether either Program Directors or Program Officers can have the breadth of expertise - particularly in the case of the Regional Development program - to achieve the aspiration for integrated approaches. Moreover, as we noted in the Preface, the preliminary introduction of this new system, alteration of the terms of existing Program Directors' employment, and subsequent moves to hire the proposed new Associate Directors - during the IDDR period - unnecessarily called the integrity of the IDDR process into question, not only with some staff but also with us.

As we see it, the SS research program remains too diffuse and unfocused. The subject areas listed for support are so numerous as to constitute a shopping list rather than a prioritized planning document. Granting that maintenance of a high capacity to respond requires a fairly lengthy list, it is hard to believe that even an expanded staff would permit the Division to operate competently in all these fields. The budget assigned to individual subject areas is often so small, when they are so numerous, as to make it impossible for the IDRC to have any noticeable impact on the development of knowledge within them. Moreover, we question the rationale for the inclusion of

further research in the SSD in areas already receiving major attention and support from other better endowed and more experienced agencies, e.g. AIDS. We do not advocate overly tight "top-down" direction of research. Rather, we proposed more specific allocations to (i) regionally determined program priorities (see section VI), and (ii) a relatively "open window" for locally initiated proposals in general (perhaps drawing on the full list of proposed areas for research to guide staff response), together with a considerably increased degree of focus in that portion of the SSD program e, say 80-85% or so, that is subject to centrally determined priorities. We do not consider it helpful for a priority-setting exercise to produce a long list of sub-programs (numbering, depending on how one counts, between 16 and 70), each with insufficient funding or critical mass to make much overall impact. We reluctantly conclude that insufficient attention has been paid to what was probably the most important recommendation in the Board's last (and only other) review of the SSD: "... the Division should focus more sharply on clarifying areas for priority support" (Recommendation III).

We do see merit, however, in some degree of "creative disorder" - particularly valuable in smaller-scale social science research projects - and suggest designating as much as 10% of the total budget for projects that do not fit tidily under priority headings.

The problems of overdiffusion and lack of focus are particularly evident in the proposed new Regional Development (RD) program. This program appears to incorporate not only the previous Rural Development and Urban programs but also broad new categories like "resource management and environmental protection" (which is projected to become the largest sub-program in the entire SSD by 1991-92). The suggestive list of RD programs (and its description - no more than suggestions - of component activities) is too broad for any one group of program officers to be able to feel part of a coherent unit.

Since the staff positions for this suggested program are still largely vacant, management should urgently undertake some rethinking.

The most important sub-program in the proposed RD division, as we see it, is that on resource management and environmental protection, and its importance in the overall IDRC program suggests that it merits special (and careful) consideration by the Board and by management (about which more below). Other elements in the proposed RD program would fit quite comfortably under other program heads. The Employment and Income Generation sub-program - as described - would seem better placed as part of the Labour Markets and Employment sub-program of the proposed Economics and Policy Analysis Program (EPA) with which it, in any case, certainly has close affinity. Food Production and Distribution, with its emphasis on broader and more marketing-related food issues than are usually addressed in AFNS, also seems to belong, if in the SSD at all, in EPA. So, as a matter of broad policy, does Regional Disparities and Development. We agree with the suggestion that Integrated Development (ID) should be "decentralised" to the Regional Offices, with a subsequent review of experience. Responsibility for the relatively small number of ID projects that can be financed out of the proposed allocations (3% of total SSD budget) might rest with Division management directly. The Shelter and Service Delivery sub-program is then all that is left of the proposed RD program. There would be a certain logic in placing this (remaining) worthwhile sub-program under the HRSD head, if the latter head is retained.

The Human Survival and Population sub-programs of the HRSD program address issues so close to those of the Health Sciences Division that they are best addressed in ways that involve close coordination or amalgamation with that Division. The resources available for SS research in these areas are undoubtedly justified but we are uncomfortable with their continuing "residence" purely within the SSD. If the IDRC were to begin

again, we would prefer these social scientific inputs to be integrated with those within the Health Sciences Division. (This includes the Education for Health sub-program in the Human Survival program, in which collaboration with HS is explicitly proposed.) Now that the Health Sciences Division has moved towards more integrated approaches that take social and economic influences into account, the time may have come for creative organizational reconstruction in this area. If the Health Sciences IDDR comes to parallel conclusions we would support the location of all of these activities in one Division or the other; and the HSD is probably the better choice (see also section IV(ii)(a)).

Overall, we perceive two broad categories of SSD activity in the plans for the next 4 years: (i) activities that are integrally related to the activities of other IDRC Divisions, in many of which the SSD may take a leadership role, and (ii) activities that stand substantially by themselves as social science research that is necessary and of high priority, but which does not require inter-divisional cooperation. Happily, for the purposes of administration, these two categories (shown in detail in Table 2) each account for roughly equal shares of the proposed overall SSD budget. The former category could be effectively presided over by a well-rounded social scientist with administrative capacity, conceivably even a Divisional Deputy Director. The latter would probably best be managed by a general development economist (or, possibly, two). In both categories, more specialized program officers could carry responsibility for the professional quality of the relevant sub-programs. It is clear from the IDDR (section 7) that the proposed new Associate Directorships will have increased responsibility for personnel management and budget monitoring, and less time for project development and scientific activity. A further reduction of the number of such posts from 3 to 2 would undoubtedly shift the balance a little further in the same direction. We recognize the risks in attempting to second-guess the results of the IDDR

process but believe that our organizational suggestions may nevertheless make better use of available talent, better achieve the inter-divisional and inter-disciplinary cooperation that everyone seeks, and move the social science components of IDRC activity more effectively into the positions they should be occupying.

Table 2

Alternative Categorization of SSD Research Topics

Jointly with Other Divisions

Human Survival
Population
Nutrition
AIDS*
Food Production and Distribution
Women in Development
Public Policy and Participation
Resource Management/Environment
Science and Technology Mgt.
Integrated Development

SSD

Macro-Management and Finance
Trade, Technology, Industrial
Policies
Labour Markets/Employment/
Income Generation
Education
Community Participation and
Knowledge
Shelter and Service Delivery
Regional Disparities

*Better dropped from the SSD entirely

We do not see the role of the SSD as "servicing" AFNS, HS and other divisions, but, rather, of actively cooperating with them. That cooperation does not require that all, most, or even any, projects are necessarily jointly run (see section V). Indeed cooperation might even be enhanced when there are social scientists already working in these other divisions, as is already the case, for instance, in the Agricultural Economics Program of AFNS.

A second-best solution might involve the retention of the HRSD and EPA programs - amended so as to incorporate elements of an abandoned RD program, and with the Science and Technology removed from the EPA. Science and Technology Policy and Resource

Management and Environmental Protection could then join Women-in-Development and Public Policy as Centre-wide units based in the SSD (or conceivably, in the latter case, elsewhere). A more compelling way of highlighting and focussing the activities of the HRSD program would be to describe its target as the children of the Third World.

We believe that these organizational issues deserve more discussion than, we are led to believe, they have so far received within the SSD or anywhere else. We are concerned to stimulate such discussion and a reconsideration; we do not seek to lay down firm prescriptions. Nor have we sought to develop a list of research sub-programs on which we believe the SSD should focus. We do, however, have suggestions and comments which may be relevant as efforts to find more focus are pursued; and it is to these that we now turn.

(ii) The Setting of Priorities

The stated criteria employed for the selection of themes or "sub-programs" in the SSD are grouped under three broad headings - comparative advantage, research environment, and potential utilization of research results. These are broadly sensible criteria, with one possible exception. "Comparative advantage" is said to include consideration of whether IDRC research can be "closely aligned with Canadian ODA priorities, and CIDA-IDRC linkages are likely to be possible". We see some potential for inconsistency and conflict between this consideration and other elements of comparative advantage which we rate as much more important - notably IDRC experience and capacity, and world-wide niche-filling (or "making a difference"). We would urge that relationships with CIDA, while important to nurture and develop in areas of mutual interest and overlapping activity, be accorded no more than secondary importance in the establishment of IDRC research priorities (and it seems to us that so far they have been).

There are important potential gains to greater theme focus and greater country focus. This undoubtedly requires more planning and forethought. If the Division chooses to move this way it needs to do so gradually. There is always loss from too abrupt shifts of direction. More important, some of the most interesting new research areas involve complicated and untried analytical approaches; "throwing more money at them" requires some prior evidence that they can be carried out. A potentially fruitful approach in new research areas may be to embark on a theme with a strong research group in one country and extend it only if the results from that effort are sufficiently encouraging. It is less reasonable to expect to locate six groups capable of high quality work on a given theme than one group.

Important to remember in the process of increasing the degree of focus are: (i) the key role of people close to Third World research; while they have their biases and foibles, those not close to the field have serious problems in framing the issues and are too often off the mark, obsolete, or oversimplistic; (ii) the probable need for non-IDRC people to participate in the discussion of the pros, cons and issues in a given area of possible research focus; this is the state of free-wheeling discussion where outside inputs are cheap and useful and where the regional offices may play an important role; (iii) while there are important concrete cases of the need for a specific result which is likely to be utilized straight away, the research which matters most in the long run often involves things of no interest to governments with their short-term planning horizons and unrealistic goals, research areas where results are likely to enter the political process only gradually and to bear fruit only in the future.

To gear SSD research closely to governmental priorities, whether in rich countries or poor, would remove much of its potential for good (though it might also reduce work which is of little potential usefulness in the short or longer run).

IV. CHANGING PRIORITIES

Some comments on the record of particular SSD programs may be appropriate - particularly those for which it is proposed that support be cut back (Education, and Science and Technology Policy/Management); those for which there is particular need to address cooperation with other Divisions (Health, Population and Nutrition, Rural Development); and those which are new (Women-in-Development, Public Policy, Resource Management and Environmental Protection). In these comments we have been greatly influenced and assisted by our consultants who were able to explore some of the issues in-depth.

(i) Areas Proposed for Cutbacks

(a) Science and Technology Policy/Management

IDRC has played a highly significant role in the financing of research on science and technology policy in the past - especially with the Science and Technology Policy Instruments projects in the Andean Pact countries and Latin American technical change studies. These programs made a major contribution to understanding and formulating science and technology policies in the Third World, as well as helping build up research capacity. In the 1980s, there appears to have been a less focussed approach in the STP program, with a great number of projects in a wide range of areas. Moreover, even where there have been quite a large number of projects in the same general area - e.g. technical change - they have not been organized into a program, and have tended to follow different methodologies.

Nor have they always been integrated with related activities elsewhere within the IDRC. This latter approach has clearly helped establish research capacity, and has also produced some good projects with interesting results. In addition, the networking and workshops in Africa have been of great - and pioneering - value in building up research capacity there. (Asia has been under-represented in the STP with only 7.5% of projects, 1982-87.) But because the research has been rather dispersed the impact on our understanding of science and technology has probably been less than it might have been. There has probably been too much responsiveness relative to program focus.

Science and technology is a difficult area in which to organize and conduct research. On the one hand, because of conceptual and measurement problems, for the most part more can be learnt from micro-case studies than from more generalizable research approaches (e.g. macro-statistics, or sample surveys). On the other hand, by their nature it is difficult to generalize on the basis of micro-studies. It follows that doing clusters of micro-studies, pursuing similar questions in a similar way across different industries, countries etc. is one of the most fruitful approaches if one wants to combine real understanding with general conclusions. In the view of our consultants, STP activities would benefit by identifying three or so topics and trying to focus quite a large number of future projects in these areas, thus building up clusters of projects. The results of these clusters could then form the basis for more general conclusions, which might feed into workshops, which could in turn both disseminate results and generate new research proposals.

The areas described as STP priorities in the IDDR (planning and resource allocation, the economic and social impact of technology and technology choice) are too broad to allow the focussed research likely to bring results; nor do they seem to be 'frontier' areas in the field.

The issue both of general priority areas and particularly of priority topics needs more thought. The topics chosen should not already be adequately covered elsewhere, and should be ones where IDRC could make a major contribution. Our consultant suggests the following topics as possible candidates:

1) Organizational aspects of R and D and technical change. This would cover studies of (a) how the organization of research and development institutions is related to the productivity of these institutions. This would include a study of the different contractual and financial arrangements made between the R and D institutes and the users of the R and D, comparing different institutions within a country and perhaps between countries. (b) Examination of the impact of the patent system, the utility system (Japanese model), and standards and regulations, on domestic innovation. Careful empirical work of this kind, particularly of a comparative nature, is scarce.

2) Analysis of agricultural research institutions - their organization, size, capacity of their researchers, relation with users etc., focussing on 'successful' and unsuccessful institutions, so as to draw policy conclusions about how to develop successful institutions, with special reference to Africa. This topic is especially important because the lack of successful local R and D institutions in sub-Saharan Africa is a critical constraint on technical change in African agriculture. This topic would require collaboration with AFNS.

3) Examination of quasi-rents received for technology transfer associated with different ownership structures (direct investment, locally owned companies etc.), and with different approaches to bargaining on the part of recipient governments. This line of research was much pursued in the 1970s but little has been done since. It is now time (a) to revive interest in the subject; (b) to assess the contribution made by various LDC government efforts at bargaining; and (c) to consider the likely consequences of the renewed encouragement being given to private direct investment in many Third World countries.

4) Traditional and informal sector technologies; characteristics; adaptation and improvements. The area is not well researched - and in particular while there are quite a few isolated studies, there are few attempts to be systematic. This is illustrated by STEP's own projects in this area: there have been seven since 1981, each following a different approach.

We would add a fifth suggestion: city technology. Investment in urban construction, both infrastructure and services, both private and public, accounts for a major share of overall investment in almost all developing countries; expertise is scarce and is usually narrowly and sectorally, oriented. There has been very little integrated and systematic research on appropriate technology for Third World urbanization.

The IDDR proposes to locate a sub-program on Management of Scientific and Technological Resources - rather awkwardly - under EPA. This is an issue that ought to straddle the IDRC's Divisions.

Resources to be devoted by SSD to Science and Technology Policy are being fairly severely reduced. What is done by the Centre as a whole in this area is no doubt greater than what will be done in this proposed SSD program alone. We would prefer that such research be structured so as to encourage a Centre-wide approach - perhaps led from a special unit based in the SSD-developing research activities in collaboration with the other Divisions.

(b) Education

In the area of education the Division's work has in the past shown a good deal of innovation and has made important contributions (e.g. in distance education, alternative pre-schooling experience, the more appropriate use of the formal schooling infrastructure, etc.). The current plans, as stated in the IDDR document include many worthwhile components; the attention to the effects of education on attitudes in such key areas as fertility behaviour, openness to change, and the like,

tend to be relatively neglected in previous research. In the past the Division has not focussed much on the strictly economic measurement of the effects of education (rate of return analysis and the like), perhaps in recognition of the considerable amount of such work going on elsewhere. The total research effort elsewhere in this area has probably included too many simple analyses of the rate of return to one or another level of education, and too little effort to trace out the processes whereby positive and negative effects have made themselves felt, and to tie an understanding of those processes into conclusions on the economic implications of various types of education and learning experiences.

As long as due care is taken not to push projects prematurely into a narrow economics straitjacket, there could be a high payoff to greater focus on the economic aspects of education and learning, in such areas as: how a particular learning process affects attitudes, and ability of peasants to undertake technological change, to defend themselves against various types of encroachment by more powerful actors in the socio-economic sphere, or to complement their agricultural activities with non-agricultural ones; how learning experiences affect one's capacities as a small scale entrepreneur, either in the informal sector or in the lower end of the formal sector size scale. Inclusion of an economic twist to a higher share of the research on education should sometimes involve members of the Public Policy group, and the Agricultural Economics group within AFNS. Such an integration could be quite an innovation in an area where too much of the "economic" research has been too narrowly economic and much of the rest of the research has not been capable of interpretation in economic terms for lack of its being framed partially in that mode. It has previously been very hard to say much that is useful in response to the big economic questions of whether too many resources or too few are being allocated to this sector, whether there are gross errors in how those funds are allocated to this sector, how privatization

affects the magnitude and the distribution of the benefits from education, and so on. Participation in aspects of the much needed integration of various disciplinary skills in education-related research could be a real contribution on the part of the Division, depending as always on the availability of the appropriate combination of inputs in the Centre.

The proposed sharp cutback in Education research therefore merits comment. The IDRC, we agree, was probably devoting disproportionate effort to this sector in the past and some cutbacks were desirable in light of the many competing claims on scarce IDRC funds. The proposed new emphasis on education for work promises to be fruitful. But these approaches are not now especially new and they seem to be matters of policy approach rather than research. IDRC may also be able to make a greater and more innovative contribution to educational research and capacity-building over the longer-run in other areas. IDRC has already led in research on innovative approaches working from the "bottom up" within the context of the formal educational system, and we are reluctant to see that area completely dropped. We are led by our concern with the increasing difficulties of many Third World universities, the need for professional training for scientific personnel in developing countries, and the frequently inappropriate curricula of professional faculties everywhere, to suggest that the IDRC devote expanded efforts to the problem of appropriate professional education for developing countries. In law, medicine, agricultural, and social science faculties, there are major curriculum reforming and text-book writing jobs to be done. We believe that the IDRC might be able to stimulate creative and innovative contributions that truly make a difference in this sphere. Such activities presumably would fall under an Education heading. FAD also has an obvious interest in such activities, as do HS and AFNS. We urge collaborative efforts in this area.

(ii) Areas for Extensive Interdivisional Cooperation(a) Health Education/Behaviour, Population, and Nutrition

It is increasingly evident that the social sciences have a great deal to contribute to our understanding of health. Biological and physiological processes are one important aspect, but behaviour is equally or more important in determining action that prevents or pre-disposes to disease, and responses to ill health. In poor countries particularly, mothers' behaviour is probably the most important determinant of children's health - which is shown by the strong evidence that levels of maternal education are far more important in determining children's health status than medical facilities, family income, etc.

Health education is an attempt to change behaviour by education. It is therefore one important component of health behaviour, but health behaviour goes beyond education. It is preferable to focus on behaviour, with health education as one aspect, rather than on health education, because focusing only on education omits some important components of health behaviour. For example, certain types of behaviour may be determined by economic circumstances and constraints such that health education alone would not change behaviour: e.g. lack of money for fuel can lead to infrequent cooking, and infrequent eating, and consequently child malnutrition. Educating mothers to feed children more frequently would not work in this situation unless the underlying economic constraints were changed. In other cases, the problem is lack of time among mothers. Again education cannot provide the whole solution. A focus on education implies that the initial behaviour - which health education is designed to change - was irrational. But the behaviour could be perfectly rational, and other matters such as the technology are what should be changed.

Health behaviour has previously been covered partly in HSD (especially in Maternal and Child Health) and partly in SSD (in

Population and Development), with some joint projects. The Health Science division has shifted attention to health education and away from the hard sciences in its approach to health, with increasing emphasis on community-based health interventions. This has been supported by the Education Unit in SSD, although until now this unit has done little independently in the area of health education. The Population and Development Unit's work involving health has had a strong behavioral element, while HSD has been more concerned with health education and has had a smaller behavioural element.

The area of health behaviour is clearly one where close collaboration is needed between HSD and the HRSD program of the SSD. There is considerable evidence that both health and population policy interventions are more successful when they are linked to one another. To date the collaboration achieved has been somewhat ad hoc, and on balance insufficient. Greater collaboration could be achieved by more joint projects, and with some systematic way of consulting all the relevant people, irrespective of where a project is located. An alternative would be to have a Health Behaviour Unit (presumably located in HSD, although it could be in SSD). Increased and systematic inter-divisional collaboration might suffice and would involve less radical organizational changes. However, the strong overlap between some of the concerns of Maternal and Child Health (HSD) and parts of HRSD (SSD) suggest it might make more sense for these elements to be part of a single organizational unit. (See also section III.)

Economics also has a contribution to make to the study of health - and one which it has not been making in the Centre. First, economic constraints and options are an important element in health behaviour. Secondly, the impact of the current economic crisis on health and expenditures has drawn attention to the need for cost-effective solutions. But there is remarkably little hard evidence in this area. This represents an important research opportunity which IDRC would be well placed to fill.

Individual economists have made a contribution in Population and Development, but the Economics unit in SSD has been preoccupied with more mainstream topics in economics. It needs the capacity, also, to contribute to topics in health and nutrition.

Much of Population and Development's work (the one-quarter of their projects with a strong health element) has been concerned with the study of health behaviour. The section has moved in a desirable direction away from narrowly-defined family planning projects and has been responsible for a number of projects on the determinants of infant and child mortality, infant diarrhoea and malnutrition, the relationship between behaviour and specific diseases and on the social and environmental determinants of levels of health. The workshops and associated bibliographies on child mortality and health in Africa and Latin America have been very valuable, bringing local researchers together from a number of disciplines. Some spin-off interdisciplinary projects have been generated by the workshops. Although it has had close collaboration with HSD, there is evidence - noted in section V(iii) of our report - that this has not been on a sufficiently systematic basis, and we have already noted further consideration of appropriate organization in this subject area (in section III).

Our consultants noted that the extensive work on rural urban migration has not paid off in terms of many important policy implications. Although the last decade has seen the emergence of new phenomena and issues in the migration area (e.g. the economics of international migration, the question of whether the rural-urban migration process will proceed in a similar fashion under slow aggregate growth as it did when growth was faster, refugee migration), and some deserve more research attention, they are of distinctly secondary importance within the SSD's mission, relative to the central question of how total population growth can be slowed down.

Nutrition is a subject of overwhelming importance. It is also one where IDRC's comparative advantage should lie - since it

requires a multidisciplinary approach, which potentially IDRC can give; moreover, it is a relatively 'new' area for research and one where research capacity particularly needs boosting.

Nutrition has of course always been important. But the recent famines and the prolonged economic crisis associated with falling nutrition standards has drawn attention to the prime importance of nutrition, and especially infant and child nutrition. Malnutrition among children greatly increases their risks of illness and death and can lead to reduced mental and physical capacity in later life. Standards of child nutrition are a sensitive indicator of the changing human condition and should rank at least as high as incomes as a measure of changing welfare. Yet our knowledge of nutrition is very deficient in many parts of the world. There is a need for research on measurement problems, causes of malnutrition, and the effectiveness of alternative interventions. There is also a need to build up capacity to ask and answer these questions in developing countries so that "nutrition" acquires a local lobby, and is not only of concern to outside agencies.

It is now widely acknowledged that nutrition encompasses far more than knowledge of the physiological effects of particular nutrients, and the nutrient content of particular foods (the first being in the purview of HSD, the second AFNS). Determinants of nutritional status also include the economic and social status of families, and the education and time of mothers. Recently, the social science aspects of nutrition have come to dominate the others, as is natural in view of the obvious economic causes of much recent nutritional deterioration.

In the light of this, we strongly support the new IDRC Nutrition Program.

(b) Agriculture and Rural Development

There are at least two broad problems in the process of agrarian change which make it important that technological and social scientific understanding be well integrated. One is that

the growth process, based on technological improvement and on public and/or private capital formation, may involve an increasing concentration of land and other key resources (such as water) and hence of income, such that it leads to or at least does not prevent immiserization and increasing landlessness of the peasantry. Interventions which pose obvious dangers of this sort include encouragement of labour-displacing machinery, research which raises productivity of crops specific to large farms, and credit systems biased in favour of large farms. It is not yet known whether immiserization occurs as a result of varietal improvement of crops grown by all types of farmers or of credit system improvements which extend access to medium-scale as well as large farmers, but still exclude the small ones.

The second general problem is how to raise small farmer productivity and the demand for labour in rural areas. Identifying the best kinds of technological change to achieve these effects, effectively supporting the needed agronomic research and contributing to better diffusion of new varieties and ideas are part of this challenge.

Both of these areas involve danger - in that the socio-political-economic dynamics may go wrong, i.e., may not correspond to what one would hope them to be. Only with a good understanding of those processes can the danger that research and investment in these areas have excessively undesirable consequences be intelligently confronted and evaluated.

It has for some time now been generally accepted that expenditures on agronomic research have a high economic rate of return, as conventionally defined (abstracting from income distribution and employment effects, etc.). But recent evaluations of agricultural research and extension in Third World countries have heavily emphasized the need for greater assessment of social, political, cultural, economic and environmental factors in such work. These aspects clearly remain the Achilles heel of research on raising agricultural productivity.

IDRC's focus within agriculture is properly on products which can be grown on small farms. This diminishes the danger of dramatically negative results. But the second problem - failure to achieve potential because of inadequate understanding of the social and economic dimensions of small farms - does imply an important role for those dimensions in the research. And though the first problem area cited above is probably best assessed by separate social scientific research on the dynamics of agrarian systems, including *ex post* studies some years after the introduction of important technological or other changes in the system, the second more often calls for joint efforts in the agronomic and the social scientific dimensions so that as good predictions as possible can be made *ex ante* as to the likely effects of particular technological advances. Since careful *ex post* analyses are too infrequently done on the impacts of technological improvement, IDRC could contribute significantly by fostering some in-depth analyses 10 years or so after the introduction of the changes in question. These are inherently difficult, requiring alternative counterfactual hypotheses; but they may be profoundly important. The analysis would usually call for complementary research by agronomic and social scientists, and could be enriched by IDRC's institutional memory on the pre-introduction context and the process of introduction. It is usually important for such analyses to be undertaken well after introduction of the change since many relevant after-effects do not show up clearly for at least a decade; most *ex post* evaluations occur too soon after projects to provide helpful evidence on the frequency and character of unpredicted or unwanted effects (like labour displacement through small farmer purchases of machinery financed by higher yields). Better timed evaluations could teach us a lot.

Given the impressive record of AFNS in working on the "right" issues from the points of view just mentioned, effective collaboration from SSD in the areas of its expertise can be particularly important. In part this would involve the sort of

cooperation on projects which is on-going. But there are also research areas totally within the social sciences whose importance is defined by some of the agronomic and economic issues which are being or have been studied. The policy processes involving small agriculture and how they change as a function of the type of agriculture, levels of development, etc. deserve priority attention. The SSD and AFNS need periodically to review their understanding of agrarian processes in order to identify the major possible foci for both of them if they are to maximize the Centre's overall contribution in this area. What has been achieved is impressive; what could be achieved is even more so.

(iii) New Areas

(a) Women in Development

The relevance of this area is not open to debate. Careful thought does have to be directed to what the important research issues are and how best they may be addressed. Since all elements of a socio-economic system are likely to be relevant to the condition of women, it is obviously important that research projects be routinely assessed in terms of whether they may provide useful answers if designed in such a way as to distinguish between the sexes appropriately. A high share of the useful evidence may be found in studies not specifically focussed on women. Here the debate on whether and why the poor gain or lose from development is central. Women make up, on average, a disproportionate share of the poor and many of the same mechanisms relevant to understanding the general questions will be relevant for women as well. This literature is rich in ideas if not unified in findings.

Many of the issues of interest in women's studies have a conceptual counterpart in more conventional social science research. The political economy of discrimination as a field is central here. In analyses of discrimination race has perhaps

more often been the focus than sex, but the mechanisms whereby these different types of discrimination occur are partly the same, and the policy issues and dilemmas are probably similar. Women are disproportionately engaged in jobs with low wages, low security, and bad working conditions. One policy dilemma involves whether legislation to counteract these problems, e.g., minimum wage legislation, will help (by raising women's wages) or hurt (by decreasing women's employment). Though not of course a substitute for the addressing of this issue in the context of women, the existing literature is worth careful study both for the results it reports and for the methodological advances which have been made.

Another research area with obvious importance to women's issues is the economics of the household, and in particular, the impact of technical change on the utilization of the time of its individual members. One motive for the recent growth of research in this area has, no doubt, been to better understand the determinants of the status of women. Much of the research, however, has not been so oriented but may nonetheless have powerful implications for understanding what is more easily alterable and what less so in that status.

The subordinate status of women has been so general, and had so many common features across countries, that the historical record in the industrial countries, notably Japan, is an unusually rich one to be understood and to act as a source of useful hypotheses as the patterns and trends in the Third World are probed.

Since available evidence on trends in women's status in industrialized societies, and on related issues (patterns of general poverty alleviation or exacerbation) in LDC's is so pertinent to many important issues, a useful early step in this new area of emphasis in IDRC would be an attempt to summarize and ponder some of the readily available information.

Agriculture and small enterprise are among the areas where socio-economic policy might be directed more effectively from

women's point of view because of the importance of those sectors and the prominent role of women in them. Public sector employment may also lead in terms of equal or preferential opportunities for women. The ways in which public policy and procedures might be altered to the benefit of women would probably be quite different across these areas; research is needed on how decisions are taken, how administrative systems and procedures affect the way government touches those working in the sector, and what could be improved in each of these dimensions.

(b) Research in Public Policy

It is undeniably the case today that most social science researchers are neophytes in terms of their understanding of public policy processes (the politics of decision making, its administrative aspects, etc.). One result is that many important conclusions of research, while deserving of implementation, stay on the shelf. Another is that research may focus too often on areas where there is no hope of implementation and eschew areas where there is, or focus too little on those elements of more general issues which will be pivotal to the decision makers' views and/or to a successful administrative process. Among the notorious gaps in the development literature are (1) analyses of political processes in the detail pertinent to assessment of the chance for policy change and effective pursuit of that change, (2) integration of understanding of those processes (currently very defective) with understanding of the economics of the issues (even rarer) and of both with an understanding of administrative processes (also very rare).

Research on public policy processes is thus a very worthwhile area. Like the area of women in development, to be most effective it must be well grounded in the accumulated knowledge of the relevant disciplines or areas of specialization:

(c) Resource Management and Environmental Protection

If resource management, environmental protection and even, as suggested, if resources permit, an environment and economics unit are to become major new areas of IDRC activity, this new thrust should be carefully thought through. In particular, the following issues should be addressed:

i) Environmental and resource management questions are important elements in AFNS and Earth Sciences programs; and HS can also provide important inputs. Cooperative inter-divisional approaches to environmental concerns have an important precedent in pesticides research. A Centre-wide approach, or at least a Centre-wide review of current Divisional perceptions and plans in this area, rather than a significant - and still vaguely stated - expansion of SSD activity seems appropriate at this stage. It would be appropriate for the SSD to take the lead role, as it does in "Women-in-Development".

ii) There has been recently a burst of international interest in this area, and there are, therefore, many others already engaged in or planning to initiate new research. There is risk of unnecessary duplication, confusion and overall waste of scarce international research resources as so many rush to follow what many see as the latest developmental "fad". IDRC may have an important, perhaps even a pivotal, role if it chooses to move more significantly into this area. But it will not succeed merely by "throwing more money at it". It can and should encourage international cooperative endeavors and find an appropriate niche for itself. This may involve evaluation or galvanization of others, as well as its own research. Having done so it will be in a better position to decide how best to organize its own contribution. This may take the form of a special unit located in the Social Sciences Division, but we are

not yet confident that is the best solution. Nor are we clear as to the importance attached to these issues by individual Regional Offices.

(iii) Environmental issues obviously need coordinated efforts of various specialists including physical and social scientists. The traditional overlap of technical and economic considerations on issues of appropriate technology and improvements in productive technology, while producing its tensions, involves a situation where there are precedents to go on and by now a considerable body of relevant information. In the area of environment many of the dangers are only recently identified and hard to study. Often the losses may be dispersed over the population, may occur with lags, and may be hard to link with causes. This can be a recipe for inadequate research and policy response.

Effective policy in the area of technology has often been hindered by refusal to integrate socio-economic considerations with those of engineering efficiency (the "engineering man" syndrome). In the environmental area the challenge will be to integrate the insights of environmentalists, who are sometimes hostile to the idea of finding a middle way between all-out environmental defense and all-out economic exploitation, and correspondingly unwilling to attempt the quantification of the social and economic benefits to environmental protection, with those of social scientists.

(d) Possible Further New Areas

We have serious reservations about some of the "second order" resource utilization plans - those that are to be added if resources are available:

1. Rangeland management (with AFNS)
2. A Centre unit on Economy and Environment
3. Experimental work on Law and Development
4. Centre-wide unit for Shelter

The rangeland management case has not been argued earlier in the IDDR. The case may be quite valid; we have just not seen it. And we question whether there will be enough activity to occupy the time of the proposed specialist in the MERO office.

If environmental and resource management programs are to be launched they should include a solid economics component from the outset. Such a component is unlikely significantly to involve issues of macro-economic management, as is suggested in the IDDR, given the current states of both environmental and macro-economics. We earlier have suggested a Centre-wide approach to environmental concerns - conceivably a "unit" - and see little merit in this peculiar and only conditional "add on" in economics and environment.

We support some initiatives in the sphere of Law and Development but would not go along with the implication that issues relating to land and resource access are the obvious priority research areas. Many others are implicit in the previous account of social scientific research needs and problems. Before hiring Centre staff in this area, it would probably be wise to engage further consultants with varied geographic and legal interests, and convene a workshop or two to consider the possibilities. These initiatives might sensibly be taken without any additional resources.

The proposal with respect to a Shelter unit is too narrowly conceived; it would be more appropriate as a unit on Habitat, allowing for other issues relating to settlements, within which Shelter could be an important part.

An alternative which we would favour is exploration, via some exploratory conferences and exchanges with UNICEF, of the needs for research on the determinants and effects of child welfare in the developing countries. The physical and psychological impact of poverty upon the Third World's children, who make up over half of the Third World's population, may profoundly influence these societies' futures, yet is only very imperfectly understood. Such a new thrust could be built upon

already planned activities on Child Survival and Development, and would obviously also involve collaboration with HSD (see also section III(i)).

In general, we would prefer to see extra resources directed to the strengthening of existing programs and sub-programs, which we have noted are already very numerous, rather than to the initiation of still more.

V. MULTIDISCIPLINARY RESEARCH AND INTERDIVISIONAL COLLABORATION

(i) Multidisciplinary Approaches

Effective interdisciplinary research on development is rare. To pioneer in this area would constitute a major contribution by IDRC. Key needed integrations are among political science, administrative science and economics in the analysis of public policy in many areas; between the physical sciences and the social sciences on environmental issues; between the health sciences and the social sciences on health/demographic issues; and among agronomy, political economy, and other social sciences on the process of agrarian change. The risks are high however, since much interdisciplinary work has turned out to be of little use to anyone.

Failure has indeed been frequent. Sometimes the failure reflects the daunting complexity of the issues addressed, sometimes lack of effective communication, and sometimes inadequately prepared researchers. Research planning must allow for the possibility (or even probability) that as research becomes less narrow it may become more superficial, and address the question of the optimal combination of inputs given this danger.

Interdisciplinary research (or holistic research, if one wants to go that far) comes in different modes. The most impressive has often been carried out by one exceptionally broad researcher; then the communication problems between disciplines and modes of thought go on in the same head.

Since each researcher or research user knows more about some disciplines than others, one cannot expect to get a consensus as to what a "balanced" understanding of a particular issue is. In most areas, however, we are so far from such balance that its achievement is a matter of only academic interest. Rather, the question is how to avoid those gross manifestations of disciplinary narrowness which lead to recognizable disasters and/or serious problems of one sort or another. One should distinguish, conceptually at least, two different types of "narrowness" problem. One involves the lack of post-primary research integration either by the policy maker or the persons who package the views which might reach the policy maker. When the research on, say, the technological, economic, and social sides of an issue can appropriately proceed separately with the results integrated later, what is necessary is that "late stage in the game" integrator. The second, more implicated type involves the fact of separate research streams (with different disciplinary homes) which cannot in fact be carried out effectively when independent of one another.

How and how well is interdisciplinary balance achieved in general, and how do other agencies which fund significant amounts of research handle the problem? Clearly the tendency to narrowness has some of its roots in the university setting and the somewhat arbitrary dividing lines across disciplines and sub-disciplines. These divisions show up in the character of most university-based research, with effective collaboration across traditional lines usually reflecting the recognition by the participants themselves that it is desirable, and sometimes encouraged by multi-disciplinary research institutions, programs, etc. Research activities undertaken in or supported by policy oriented agencies like the World Bank, USAID and other national and international participants in development are less likely to be characterized by transparent narrowness, since its real-world costs are better understood and/or experienced.

It appears that, over time, the potentially perverse effects of the "engineering man" syndrome (preference for the modern, the high labour productivity, the high conversion ratio technology) have become broadly recognized, and with this recognition has come an enhancement of the role of the economist. But this issue of appropriate technology is one of the many in which the potential contribution of the other social sciences has not yet been achieved. Economists and engineers continue blithely to disregard political, administrative and social aspects and constraints as they go about both their research and their work, with often no outside control to force an end to this behaviour pattern.

The reasons for this, apart from the normal intellectual insularity which afflicts us all, seem to be several. One appears to be the technocratic view that whereas research in technical fields where value judgments can be avoided is "appropriate", the study of political or social constraints on or implications of a given policy, investment, or regulation is not. One should not "manipulate" the political process, technicians may think, in order to achieve a specific technical outcome. By extension, one should not study how to manipulate that process. Instead one should simply present the technical alternatives to the politicians. Though one may applaud the essentially attractive vision in this idea, one cannot neglect the element of naivete. Some social scientists, particularly political scientists and sociologists, have contributed to their relative exclusion from relevant policy-oriented research by their tendency to focus more on how systems function (the big picture) than on how policies can be implemented (the nuts and bolts). The considerable influence of radical ideas in the latter disciplines makes many of the ideologically more conventional hard scientists, engineers, and economists wary, as does (for the economists at least) their disinclination to think in terms of resource constraints and trade-offs. The upshot of this lack of collaboration is that for many major issues of policy involving

economics, there exists no competently thought through political/economic analysis, let alone a broader treatment including insights from sociology and anthropology. Often the political analysis is satisfactory but the economics flimsy, or vice versa. And in most development agencies, such thought as is addressed to the political and social dimensions of policies or projects is often the laymen's efforts of policymakers not professionally trained in these areas.

(ii) IDRC's Weakness in Economic Analysis

We have been somewhat shocked to discern the relatively weak capacity of the IDRC as a whole and the SSD in particular to conduct economic analysis. There are at present only two holders of a Ph.D. in economics in the entire SSD permanent staff, and only five who hold degrees in economics or political economy at any level (AFNS and OPE have a few economists as well, making a grand total, as far as we know, of three doctorates, and probably about eight with other degrees in economics, in the IDRC as a whole). In these circumstances it is perhaps not surprising that the economics and public policy component of SSD-supported research in education, population and health has been so weak; that AFNS has developed its own agricultural economics unit; and that economic analysis in STP programs has not been more prominent. Weakness in economic analysis has undoubtedly contributed to the SSD's somewhat "soft" image in some quarters both inside and outside the Centre. Perhaps the shortage of economists has also contributed to the difficulty the SSD has evidently encountered in assessing its own programs and making tough decisions among alternatives. It may also help to explain the relatively low esteem in which IDRC employment appears to have been regarded by development economists.

It is unusual - to the point of being striking - to find an extended exposition of the possible usefulness of economic analysis in the prospectus of a development research institution, even more so in that of its Social Sciences Division (i.e. the

SSD IDDR). The frequent separation of "economics" from "social sciences" in the exposition of all but the final IDDR draft also suggested to us an unusual "mind-set" in development discussion. The plan for an Economic Analysis Service - which, in the circumstances, is an understandable one, and one that we support - also suggests the existence of a serious (and again highly unusual) problem. It may be that in many other development institutions the role of economics has been over-emphasized. But the IDRC seems to have erred seriously in the other direction. Economics is a key developmental social science and the IDRC's weakness in this respect should be repaired as a matter of high priority. The large number of vacancies in the SSD offers an opportunity to fill this gap but we are unsure that its existence is sufficiently recognized. Every regional office should have at least one economist in its complement of program officers, and this economist should normally be an SSD officer. Stiff market competition for good development economists may necessitate special staffing arrangements - at least on a temporary basis - to meet this need. Holistic and systems-oriented development approaches are unlikely to progress very far in the absence of basic professional inputs, particularly from the relevant branches of economics.

(iii) Categorization, Organizational Boundaries and Disciplines

An organization like IDRC - which covers a very great range of topics and has quite a large staff - has to be divided up into smaller units, each of which specializes in certain areas. The question at issue is what is the best way of dividing up the organization for administration of research.

It is important for IDRC to have as good an idea as possible of those research areas where effectiveness calls for interdisciplinary research as opposed to those which simply require post primary research integration of various strands, and, in the former cases, what sort of IDRC processes can best induce the desired collaboration, e.g., participation of more

than one division of IDRC in the processing and setting of research proposals, ready availability of each division to provide services to the others when needed, etc. Whether multidisciplinary approaches have to involve more than one division of IDRC or even more than one program officer depends on the case, and each case needs to be treated on its own merits.

Pertinent to the question of optimal organization of the Centre itself is the degree to which individuals can reflect breadth of scope, as opposed to its being achieved by involvement of several people. Some of the inevitable difficulties can be avoided if all personnel can be induced or nudged up to a minimum threshold understanding of why their own specialization is of limited use without inputs from others. Study directed at really understanding issues such as what goes wrong and why when premature mechanization occurs, health systems are too expensive for a country, environmental issues are treated as afterthoughts rather than forethoughts, and the like might help.

Pending the development of more appropriate post-graduate training programs for those embarking upon research careers in the social sciences (and related areas) in development, we urge the IDRC to join the U.S. Social Science Research Council in the financing of natural or technical science training for social science Ph.D. candidates working on development problems. (The U.S. SSRC program relates only to agriculture and health, and only to Africa). Similarly, special funding to permit scientists better to familiarize themselves with social scientific dimensions of the development problems they address may also often be appropriate.

What actually happens in the field - the character and quality of research submissions and the subsequent research - of course is to a great extent independent of what happens in IDRC, being a function of the capacity and interests of Third World researchers and research institutions. However, the way IDRC is organized does have an effect on Third World research by indicating to Third World researchers the sort of research the

institution is likely to support, building up particular types of research capacity, stimulating new ideas for research through travel and workshops, and changing the research through the processing of requests. These effects are likely to be greater the more innovative the IDRC is, and the less it is financing the same sort of research as everyone else.

In theory there are a range of ways of dividing up the administration of research - for example, by region, by problem area, or by discipline. The IDRC has chosen a combination of these three, although disciplinary categorization perhaps dominates, as suggested by the names of the major divisions. Within the SSD, categorization is largely by subject area. The regional offices provide a geographic classification.

There is no categorization that divides up the subject areas in a way that perfectly corresponds with the actual problems. Consequently, whatever the boundaries, there will always be some problems of overlap (two parts of the organization having something to contribute to one problem area) and also of exclusion (some problems getting inadequate attention because of the way the boundaries have been drawn). Each presents serious problems for the organization of research. Where there is a lot of overlap between the program areas adopted by the organization, there arises a need for collaboration between organizational units if the problems are to be dealt with adequately. But because this tends to be administratively cumbersome, it is often neglected and as a result some aspects of a problem may be under-emphasized. The exclusion problem means the neglect of a problem area as a whole. It is therefore desirable to get the organizational boundaries to correspond - as nearly as possible - to the actual problems that present themselves in developing countries. The better the fit, the less the dangers of overlap or exclusion. But it should be emphasized that there are normally no 'right' boundaries, although some types of categorization are better from the perspective of overlap and exclusion, some worse.

(a) Interdivisional Collaboration

Inter-divisional collaboration has increased over time, as shown by the increase in joint projects in the 1980s. In discussions most program officers emphasized that the figures for formal collaboration understate the extent of collaboration because of the existence of informal collaboration, which is claimed to be high and increasing. Moreover, different disciplines are represented within each division so that it is not always necessary to have inter-divisional collaboration to get multidisciplinary research.

Inter-divisional collaboration has been especially high between SS and HS in areas covered by the Population and Development program, partly because of close personal relations as a result of an earlier shift of the program between the divisions, and because some individuals have worked in both divisions. There has been particularly effective collaboration on the series of Workshops on Child Mortality and Child Health in Latin America and Africa. These were organized on multidisciplinary lines and have stimulated some multidisciplinary research in Latin America. However, even between Population and Development and HS - where collaboration was particularly good - out of the twenty-three projects with a strong health element in the Population and Development program approved between January 1981 and March 1987 only six involved formal collaboration between SS and HS.

(b) Formal versus Informal Collaboration

Joint projects are only one way of collaborating. In discussions with program officers almost everyone emphasized that informal collaboration occurs a great deal, so that the 'formal' figures of joint projects quoted above mean very little. It was suggested that informal collaboration has considerable advantages compared with formal, in particular (i) that joint

projects are administratively cumbersome, duplicating efforts and bureaucratic procedures; (ii) that they sometimes emerge as odd mixtures unrelated to the capacities and preferences of the researchers involved; and (iii) that cooperation cannot be enforced by formal procedures; people have to want to do it to make it work.

To test the extent of informal collaboration one of our consultants read through a sample of files supplied by the Population and Development program (the program with best contact between SSD and HSD for historic and personal reasons). Of nine files analyzed, four were formal joint projects, another four were SS projects, covering areas where collaboration would have been desirable, and where the subject matter would suggest that there should be evidence of informal cooperation, and the remaining one covered an area where no cooperation was appropriate.

Of the four where one would expect informal collaboration, there was evidence of it on the file in only one case. In the one case, the relationship between SSD and HSD ran into obvious difficulties at one stage, with complaints that the HSD staff had not been kept up to date with developments. The three cases where there was no collaboration included topics on which HSD had a clear contribution to make, but there was no evidence on the file (which contains all written communication plus some reports of conversations) of any consultations.

The files on the joint projects suggested the collaboration worked well, with useful comments supplied by both divisions and a clear improvement in the projects as a result.

This exercise suggested that informal collaboration is neither as prevalent as is claimed, nor does it ensure smooth relations. Formal collaboration appears preferable, where a significant contribution would be appropriate from two divisions.

The joint projects did involve careful reading and commenting by both divisions. However, this was the case equally for informal collaboration, when it worked, as for formal.

Moreover, the projects where there was no collaboration also involved careful reading and comments from more than one person - 3 or 4 Social Sciences staff were involved on a single project. From this perspective, therefore, joint projects do not need to generate more work than "mono"-projects. Interdivisional projects do duplicate some procedures - in particular, the divisional review occurs in both divisions. However, this is a small part of the total operational procedures. Work requirements for lower level procedures - preparation of project summary etc. need not be increased, but merely changed so that rather than two or more people being involved from the same division, one is from another division. This presumably would make the procedures a bit more cumbersome and less easy to handle, particularly until it becomes routine. Higher level procedures, subsequent to the divisional review - review by the Vice-President, Program Review Committee and submission to the Board - are not increased.

There are ways of achieving formal collaboration besides jointly financed projects. One possibility, for example, is joint committees on certain areas, containing members from the relevant divisions, but the responsibility for the project remaining with one division. This procedure is particularly appropriate where the topic is mainly the concern of one division, but where more than two divisions should be involved. The new initiative on Women in Development is organized in this way, with prime responsibility in SSD, with the support of a committee of members from throughout the Centre. The initiative has quickly generated a number of projects and has enjoyed a high degree of collaboration across divisions. The program officer in charge of this initiative felt that the 'formalization' of collaboration through the new organization had greatly increased useful collaboration. The approach can generate mono- or joint projects. This model might be appropriate for other subject areas which we have discussed above.

In discussions, program officers frequently emphasized that work supporting projects in other divisions (especially informally) was not recognized sufficiently in work appraisal and career prospects, and this was a disincentive to doing too much. Clearly, this should be changed.

Our investigations suggest that formal collaboration across divisions works better than informal. Both budget and staff time must be formally allocated for it to work to its full potential. Program officers' work in collaborating with other divisions should be formally included in their work appraisal.

(iv) Conclusions

The SS and other divisions of the IDRC have great potential to support innovative research approaches and in particular to bring different sciences to bear on the problems of development. To some extent this is already being achieved, but organization within the Centre has divided up subjects and disciplines and prevented full use being made of this potential. The experimental innovations in Women-in-Development and Public Policy are important, and may offer experience for others. Nutrition and Health Behaviour are high priority subjects where the IDRC could also play a very important role. In Nutrition, efforts have begun to permit IDRC to play it, efforts that we applaud. In Health Behaviour, (which includes Population) more inter-divisional collaboration is called for; how this can best be achieved may best be sorted out during the IDDR for HS. We have tried to prepare some of the ground in our collaboration with the Board panel and its consultants in the HS Division, and we have made tentative suggestions in this regard in section III.

Resource Management, Environmental Protection and Science and Technology Policy and Management are other obvious areas for increased inter-divisional cooperation and Centre-wide approaches (see section IV).

VI. DECENTRALIZATION

The SSD proposes to expand its representation in the IDRC's regional offices substantially. This implies some reduction in Ottawa staff and thus a considerable degree of staff decentralization. We strongly support these plans. We also believe it important to call attention to some implications and further dimensions of decentralization in the SSD. Our comments relate to: (i) the potential benefits from staff decentralization; (ii) the potential costs of staff decentralization; (iii) the potential advantages of increased decentralization of budget authority; (iv) regional approaches and the role of the regional offices. Many of our comments relate to IDRC activities as a whole. We do not see how we can comment upon the SSD's plans (which include some elements of decentralization) without raising these issues now, rather than waiting for their discussion on a cross-IDRC basis. We believe that it is now time for a detailed Board discussion of the decentralization question in general.

(i) The Potential Benefits from Staff Decentralization

The Winegard Report has provided an eloquent and persuasive statement of the benefits to development programs from providing them on a more decentralized basis, and CIDA is seeking to respond. The Report concludes its discussion of the issues:

"... decentralization is not a cost-free process - financially, administratively or politically. Financially, it entails spending a large portion of the aid budget on administration; administratively, it means losing some control at the Centre; and politically, it means accepting the risks of a ... program truly responsive to the needs of our developing country partners. We strongly support substantial decentralization only because we are convinced that its costs are far outweighed by its likely benefits in a more effective ... program." (Winegard, p. 20).

We agree with the Winegard argument. In the sphere of research, the case is, if anything, even more compelling. There is probably greater risk of arrogance at headquarters in the sphere of scientific research endeavours than there is in the aid sphere, where at least there is usually a presumption that requests are "legitimate". The risk of seriously impinging upon the prospects of success through pointless bureaucratic delay is also probably greater in research support than in other aid. Particularly is the case for decentralization strong in the poorest countries, on which IDRC proposes further to concentrate. In these countries, the number of skilled local researchers are few, many of them are fairly mobile as between jobs, and the need for responsiveness and flexibility are much more likely to be critical.

Research needs in many such poor countries, moreover, are frequently simple and/or have little to do with where the development debates are internationally. A country may, for instance, urgently need better wage statistics or better data on its forest resources, and a project assessing how to achieve this may be important. Such country-specific needs are hard to assess sensibly from afar. The identification of potential researchers may also be harder in such countries because of their scarcity; and the sort of direct substantive research assistance IDRC personnel can offer is certainly more important. In countries at the upper end of the spectrum of research information and sophistication, proposals may be hard to assess unless one is specialized in the area of the proposal, so the logic of the central office playing a greater role, though perhaps still only in the form of technical backup, is greater.

The degree of effective decentralization in AFNS should by now have inspired imitation in the SSD. That this has not occurred is not the fault of the Division. The SSD has long pressed for similar decentralization of staff but has been frustrated in these aspirations by management and Board-imposed decisions motivated by budgetary considerations. The restoration

of health to the SSD now requires that its staff decentralization at last be given the green light.

(ii) Potential Costs of Staff Decentralization

The costs of decentralization are often cited - particularly by Centre managers (or, perhaps, those who fear their own relocation or loss of power) - as too great to permit more of it in a period of budget stringency. To this we would say that it is an elementary principle of economic analysis that costs are to be assessed in relation to the benefits associated with them. In this case, the benefits are now widely seen (Winegard is not alone) as considerable.

Reference in this context to IDRC's objective of holding administrative costs to 30% of the total budget, to which we have been exposed, are logically irrelevant. At best, this consideration invites a careful assessment of the benefits and costs associated with other large components of the administrative budget.

The performance and reputation of the IDRC rests ultimately upon the activities of its program officers in the field. In thinking through the benefits and costs of decentralization and the various other organizational questions with which it must deal, IDRC and SSD should presumably work backwards from the substantive matter of how research is done in the developing countries and what inputs contribute to its quality and usefulness. Bureaucratic principles which may have much validity in many other settings should not be crudely extrapolated to this one. We believe it is time for a major independent review of IDRC administrative practices which impress increasing numbers of informed people, both inside and outside of the IDRC (many of whom have made a point of speaking to us about them) as unnecessarily cumbersome and expensive. It strikes us as anomalous that IDRC, a non-governmental body, should now be seen by some as more ponderous and less flexible than CIDA!

That said, we are not persuaded that there are necessarily any extra costs to staff decentralization. At least one review of this issue within the IDRC (see section 7 of the IDDR) suggests that if there are any, even with current practices, they are likely to be small. We believe that they would vanish if there were both continued heavy reliance (at present about 75%) upon professionals who are citizens of the developing countries, as is appropriate for the IDRC, and if the current practice of providing all of them with the full range of Canadians' salaries and other tax, housing, overseas, and relocation allowances (Policy Manual, section 7) were stopped; even those who believe the costs of staff decentralization are now large, agree on this point.

We recognize that a review of current equal-pay practices raises an important issue of principle, indeed of staff relations more generally, one upon which feelings and views are strong. But it is an issue which is fundamental to the overall style, image and purpose of the IDRC as an organization; and we believe it should be discussed by IDRC's international Board. It is perhaps appropriate that this "social" issue should be brought to the Board in consequence of a review of the Social Sciences Division.

(iii) Potential Advantages of Increased Decentralization of Budgetary Authority

Perhaps no less controversial in the decentralization debate is the question of decentralization of budgetary authority. If SSD staff are posted in increased numbers to Regional Offices, we expect the quality of SSD programs to improve. But the potential for developing projects and programs which are agreed by staff in the Regional Offices to be of high priority there, both within the SSD and across divisional boundaries, may still be undesirably constrained by the need to resort to the standard chains of budgetary command. We recommend that ways be explored to permit the expansion, at least on an experimental basis, of

budgetary authority for projects developed by groups of program staff in regional offices. Budgetary responsibility for such projects would presumably rest with Regional Directors who, in some way, would be answerable ultimately to the Board. Professional advice for such joint projects could, of course, be solicited from IDRC headquarters staff or elsewhere as required. It would be appropriate for a specific program officer of the SSD to be designated the lead person in each such joint project, whether it is organized entirely by SSD personnel or by a wider range of Divisional representatives. As such activities develop it might be worth considering the appointment of a Director of Research in each Regional Office so as not to place inappropriate burdens on the Regional Directors.

We applaud the statement of willingness on the part of the SSD to delegate signing authority to Regional Directors for "research which is collaborative in nature and for which Regional Office staff resources are available for adequate project review, development and monitoring" (pp. 7-14). We could easily see a first-step allocation of \$1 million each to SARO, EARO, and LARO over a two year period, beginning in 1989-90, in the first instance; and recommend that the SSD explicitly budget about \$1 million per year (i.e., half the required total) to encourage others to join in this experiment; this would amount to about 5% of the total SSD budget in the 1989-91 period.

These and other suggestions we have made might seem to impose additional burdens on the regional offices that could be difficult to fulfill, given current procedures and their present and projected human resources. But we do not believe that such difficulties need arise. Rather, we see the possibility of freeing the time and energy of Program Officers for more important tasks. Moreover, we believe that the professional-academic performance of regional offices could be improved. One way may be to recruit as "trainees" small numbers of young professionals from the country where the office is based or other countries in the region. Their tasks would be to help program

officers and the Regional Director with background materials and monitoring during the different stages of SSD and other projects. This could provide much needed experience for research administration in Third World countries. The "trainees" could be compensated in local currency and according to their comparative qualifications in their own nations. They might remain for a period of two to three years and be tutored by the program officer in the discipline chosen.

We also recommend a significant increase in the amounts that can be authorized by program officers in the field on their own authority for worthwhile smaller projects (always subject to appropriate accounting after the fact). Program Officer's current (PODA) limit of \$5,000 is ludicrously small, not available for projects at all, and certainly far less than the professional standing of these officers would seem to justify.

Furthermore, we find it ironic that a proven device for effectively achieving decentralized budgetary authority - the "Centre-administered project", as in the Macro-Economic project in EARO - is being discouraged at the very time when there is increased rhetoric about the need for more of it. Other donors are mystified by the declared opposition of non-SSD IDRC officers to what must be among the IDRC's most successful initiatives in Africa to date. This example seems amply to demonstrate the inability of bureaucrats and scientists from other divisions (who do not hesitate to transfer large sums for administration by UN agencies and CGIAR institutions) to understand the needs in SSD-initiated endeavours.

(iv) Regional Approaches and the Role of the Regional Offices

The role of the regional offices, the definition of their priorities and the quality of their projects and programs are essentially in the hands of the IDRC staff (in the regions, divisions, and Ottawa management), with little or no systematic "region-specific" input from the Board. Once every year the Directors of the Regional Offices present to the Board the

problems of each particular region and the priorities for research and training they have detected. The basic idea is good but the time allocated to the presentation and discussion of the reports presented by each regional director is so short that it is largely a formality. Regional Directors cannot gain much from these exchanges; nor can Governors learn much more than what they acquire from reading the reports or what they already know of what is happening in each region. There is not much the Board can contribute to Regional Office activities if the method is not improved or drastically changed.

The following remarks have the basic purpose of opening ways for a more fruitful dialogue between the Board and the regional offices and for a more substantive contribution of the regional offices to IDRC programs and priorities. We believe that the initial step is for the Board to help to redefine the contributions that the regional offices could make to IDRC overall efforts.

There should be increased opportunity for the Board - perhaps in its Program and Policy Committee - to discuss both inter-regional priorities and research priorities within each region. The IDRC management's decision to direct more resources to Africa, for example, was never systematically discussed by the Board. The views of program officers within the SSD as to the desirability or modalities of this inter-regional shift were evidently not canvassed either. Neither they, nor we are at all sure that significant budgetary reallocations towards SSD projects in Africa are feasible or desirable in the short to medium-term. Altered ways of doing IDRC business in Africa may be much more important than dollar reallocations. An expanded role for regional offices in Africa may be part of the appropriate approach. These issues require more deliberate and careful analysis than they so far appear to have received; and this analysis must involve those with direct experience in Africa.

Priorities at the Regional Level should also receive more systematic attention. It is clear both from the Regional Directors' reports and from the project docket (and these two are not always mutually consistent) that these priorities vary widely from region to region. "Local vision" is very important in the setting of research agendas. The regional offices should be encouraged to hold regional meetings with top researchers on overall research priorities and on the research situation in each region. These regional meetings could take the form of annual or bi-annual meetings with selected, say seven or eight, specialists in the broad areas covered by each regional office. Board members, particularly those from the relevant region, might usefully participate in such meetings. The Board has discussed and supported such proposals, and they should be experimented with, and developed. It might be the role of the SSD to "service" such regional meetings with broad socio-economic and political analyses on which the participants and the Regional Director could draw.

The mix of projects within each Region is not at present highlighted in formal presentations to the Board. As an experiment, it might be worth presenting and considering the project docket on a region-by-region basis, instead of on the traditional divisional one, on the next occasion at which Regional Directors are present at a Board meeting (presumably October 1988).

We are fully conscious of the traditional disciplinary organization of the IDRC; indeed we support this form of organization. We do not advocate a major organizational change. Rather, we propose that greater prominence be given to questions of regional and inter-regional priority-setting, greater advantage be taken of the opportunities created by well-staffed regional offices, and greater use made of the SSD's capacities in these regards.

VII. FUNDING STRATEGIES AND RELATED ISSUES

(i) The Small Size of SSD Projects

One hesitates to impugn the potential of large research projects at a time when so many socio-economic crises do call for quantum leaps in our understanding of how to handle them. Yet given the record, neither can one be sanguine. It often seems that big research efforts are destined to "fail" in the sense of yielding much less than hoped for. Institutions which fund larger projects often lack sufficient understanding of social science research to be aware of the dangers such projects entail, and hence to avoid the failures by careful screening of projects, participants, etc. Given the attractiveness of large sums of research money it is obvious that many ill-designed teams will put themselves forward. Non-specialists are not well placed to screen effectively or identify weaknesses. As with investment projects, one of the lures of the large research project to the funding agency is that it places the funds while usually calling for less preparation time, easier decision-making, and reduced monitoring inputs. This is often false economy in the case of investment projects, and even more so in the case of research activities. Unless the lead time after firm commitment of funds and prior to initiation of research is fairly long (not often the case, given the practice of funding agencies) it is not normally possible even for the best placed team leader to get commitments from the most skilled researchers, so the team usually has a very "second best" character to it. Finally, some projects whose scope implies or should imply at least a minimum of interdisciplinary collaboration and a range of approaches fail to satisfy these requirements.

The upshot is that many \$50,000 projects pay off more than their \$5 million cousins. Unfortunately, when research is planned and executed in small modules, each reflecting the interests of one of a heterogeneous body of researchers, it is to be expected that the package of results will have an accidental and random aspect to it, and that frequently the components will not "add up" to the answers one is searching for. Hence the need for networks, conferences, and bigger research programs with the adding-up feature built in. Bigger projects would often be the obvious solution were it not for the quality problems which typically plague them. Those problems can sometimes be surmounted, but an agency which is not fully aware of their origins is not very likely to surmount them. IDRC should have an advantage relative to some other funders of comparable social scientific research (e.g., CIDA, most Foundations and multilateral agencies) in its closeness to field-level Third World research, an advantage that shows up in its experience in effective organization of networks and small teams. Perhaps this advantage constitutes reason for the IDRC to move cautiously in this direction.

At present, SSD projects are, on average, significantly smaller in size than those of other Divisions - roughly 70% of average IDRC size in 1985-86. Many projects fall below the \$100,000 cutoff and are not discussed by the Board. Most of these are described in one sentence or so in the "white" paper of the project docket with which Board members are presented on the first day of their meetings. At the October 1987 Board meeting, for example, of 33 Vice-Presidentially authorized projects (between \$50,000 and \$100,000), 18 were from SSD. It is striking that in 1985-86 the Board was responsible for approving only 30% of the SSD proposals approved by the IDRC, accounting for 60% of the dollars approved. Fully one-half of all SSD proposals approved, accounting for over one-third of their dollar value, were approved by the President or Vice-President. Of the proposals discussed within the SSD (about one-half of which were

not approved), the Board saw less than 15%. Of the total evaluated, the Board saw less than 8%. Comparable data from other Divisions were not made available to us but we suspect that these figures are unique to the SSD. They undoubtedly contribute to the program officers' expressed uncertainties as to what is expected of them and the Board's previous failure to adequately monitor what was going on in the SSD. During periods when leadership in the SSD was lacking this state of affairs was particularly damaging to program staff morale.

We consider this low a degree of involvement and knowledge on the part of the Board in any Division's activity inherently undesirable - at least as long as the Board purports to play a "screening" role for IDRC projects. But even more important to us is the fact that such a high proportion of the project decisions have been made neither by the Board nor by the program officers but by Centre management. This management has often not been close enough to the particular SSD projects or experienced enough in social scientific research approaches in the Third World to be able to carry the confidence of SSD program officers as to the sagacity of their decisions. We suggest below that the SSD experiment with larger, more program-style, grants; these, together with the increased use of inter-Divisional collaborative projects (which are necessarily larger) would, as a by-product, help to overcome this problem. To the extent, however, that the SSD necessarily must respond to smaller requests, Board responsibilities may have to expand. We recommend systematically greater Board attention - via its Project Screening Committee in the first instance - to the white and blue pages (under \$100,000) at the back of the SSD project docket. (This recommendation is the product of the relatively small size of so many SSD projects, and is not necessarily intended to be generalized to other Division dockets.) But, more fundamentally, we favour more decentralized signing authority for program officers, (see section VI).

It is also important to note that large numbers of small projects imply a heavy workload for SSD program officers relative to officers with equivalent dollar appropriations in other Divisions. With the decline in program staff in recent years this already heavy workload has been growing even heavier.

(ii) Networks

The use of networks has been extremely productive in the SSD's program. In some instances where network members are well-established and highly qualified, e.g. in the Latin American macro-economic network, it may be possible to draw on them more effectively in the elaboration of regional plans and priorities in the IDRC's regional offices (see VI-4).

Networks may be the only means for developing activities in sub-Saharan Africa and other poor countries or poor regions within better-off countries. If there are to be expanded activities in these poorer areas not only will networks have to play an important role but so will Centre administration of them. As the IDDR says, "In the poorest regions, there is simply no alternative ... Centre-administered networks ... now form the core of the Division's strategy for Africa and are an efficient, flexible and cost effective mechanism" (VI-5). We agree. We believe that the IDRC, through its regional offices, can and should continue to assist in the management of research and research support in sub-Saharan Africa. We therefore recommend that SSD activities in Africa, particularly those involving networks and small grants, be presumptively treated as exempt from IDRC guidelines with respect to Centre-administered projects until the Board recommends otherwise. (We suspect this recommendation may be appropriate for other IDRC Divisions as well but we have not explored the matter further.)

We note too that shortage of DAP funds are "a serious constraint on development of first phases" in network-building (VI-5), and urge that ways be found to ease this constraint

immediately if it is at present the binding one on effective project development in Africa.

(iii) Small Grants

Small grants programs in the SSD include competitive awards in priority subjects and/or areas, but they also include some that are "not necessarily linked to Divisional priorities"; the latter less focussed funds have accounted for about one-quarter of total small grants activities (which themselves make up about 10% of SSD appropriations in 1981-85). We agree with the suggestion that the extent of use of focussed small grants programs be left to particular SSD programs. More general small grants programs - in modest amounts in areas like Central America and Africa - should continue in cases where regional strategies, developed at the regional level, call for their productive use.

(iv) Program Support

"Program support" to social scientific institutions has accounted for a relatively modest share of SSD allocations (7.5% in 1981-86). Use of this funding modality implies a higher degree of local control over research programs and correspondingly greater IDRC risk. In other IDRC divisions de facto program support has been more frequent - by the nature of programs with a specific task orientation, long gestation period, and often major equipment requirements. Perhaps it is time for the SSD to experiment a little more with larger grants for research in broader "target areas" to be undertaken over a period of years, in a few more institutions. The Board and the SSD may together - in their cautious approach to "longer leash" approaches in the inherently less "targetable" projects of social sciences - have inadvertently generated the relatively large number of small projects that is distinctive to this Division and that contributes to its staff's work overload. Program support may be unwise, however, in many of the poorer areas in which

activity is to expand. We are not recommending a sharp across-the-board shift in SSD approaches so much as a little more experimentation and risk-taking where appropriate. To the extent that there are more joint programs with other Divisions some SSD changes towards more "typical" IDRC project modalities will probably occur anyway. We also support initiatives towards cross-Divisional "integrated support for research institutions", and hope that this proposed new modality will encourage more interdisciplinary cooperation both in the developing countries and in the IDRC.

(v) Non-Governmental Organizations (NGOs)

We attach special importance to the proposed "modest, streamlined increase in interaction between the Division and NGOs" (VI-16) - incorporating research both on and by these organizations, and research training for the NGO community. Efforts in this direction might usefully draw on the experience of CIDA's NGO program.

Recent decades have witnessed the formation of a great number of Third World NGOs, typically linked with community organizations, active in areas which have close links with the social sciences, in particular, in education, shelter, and health. There are also local NGOs active in other areas related to the social sciences such as population control, environment, food distribution, etc. but these are less numerous and without the strong networks of the first three.

NGO's in the Third World, although small and leading precarious lives, are very committed, much more than most governmental offices; they are less expensive to run (although there is a risk in the lack of professionalism among most NGOs); and they have a continuity in their efforts, that is frequently lacking in governmental or university programs. They have also maintained a greater independence from political parties and coalitions, although they are frequently consulted by democratic regimes. NGOs have not only been able to mobilize a large

number of young people but, in certain areas, such as shelter, human settlement, education and population, they have produced some of the best and most original research. (In many countries, these NGOs were created and staffed by university professors with solid academic background and good research experience who were forced to leave universities for political or economic reasons). In some countries of Latin America, francophone Africa and South East Asia, the contribution of NGOs to research and action programs have been quite remarkable. (Of course there is also much to be learned from the weaknesses of individual NGOs and of some NGO networks.) Perhaps the most critical point is the isolation in which many of them have worked, and continue to work, understandable under repressive regimes, but less so when faced with different possibilities under democratic or politically more open regimes.

NGOs work in a problem-solving mode rather than in the fundamental research aspects of shelter, health and education. NGOs move in the middle-ground between research and empirical application of the results of research. They have a crucial role to play as adapters of technologies geared to the needs of very low income groups and as advocates of social change. In order to fulfill this role, they need to expand their own research base and connect with those engaged in research elsewhere in related disciplines. This knowledge is essential to launch campaigns that could open minds and, thus, attitudes towards prevailing injustices, and to move towards the solution of specific problems.

(vi) Special Needs in Sub-Saharan Africa

Africa is at present in the international spotlight. Fifty per cent of CIDA bilateral aid is to be spent in sub-Saharan Africa by the year 2000. From these facts and from IDRC's own mission statement it is logical that the SSD devote more of its attention to low-income Africa. But there are obvious dangers in attempting to "gear up" research activities in an area that is so

different from others and in which the constraints are different. We do not believe that budgetary reallocations are always the first-priority means of responding to Africa's need for expanded research.

There is no doubt that, in the crisis situations of many of the poorer developing countries (particularly in Africa), IDRC's traditional responsive modus operandi may not be optimal. There are few enough well-trained social scientists to begin with, many are in worn-down institutions, and the pressures of other activities are heavy (including sometimes the pressures of dealing with the unending streams of foreign agencies). Something more than just the IDRC's (selective) response cum guidance approach is often desirable. Expatriate involvement can pay off greatly, as can networking, leading to greater externalities among local researchers. But particularly in the social sciences, both must be well planned and well carried out, especially the expatriate involvement, if they are to contribute much in either the short or in the long run. Essential to such planning is recognition that (i) there are very few corners of the social science research domain in which a quick highly competent burst of research activity can provide the answer to an important question for, say, the intermediate run; more commonly returns come from an ongoing research process; (ii) hence local involvement in the first stages of research is normally the only way to achieve continuity; (iii) large groups of expatriates are usually undesirable because of the threatening aspect to locals, the increased penchant for interacting too much among themselves and too little with nationals, etc.; (iv) many expatriates, regardless of their overall research skills lack prior awareness of the country context, the data problems or the sensitivities of nationals and/or lack the capacity to communicate and interact well with local counterparts; (v) in many relevant specialties the Canadian community of experts is very small, so it cannot be taken for granted that there will be any one in it qualified to undertake a particular task in a given developing country. If

IDRC were to increase the number of projects with foreign involvement, the task of identifying appropriate expatriates would have to involve a worldwide recruitment.

We recommend that the IDRC launch a Centre-wide review of the practicality and productivity of its current funding and administrative practices in sub-Saharan Africa before it significantly expands its expenditures there. This review must take account of the divergent needs of the IDRC's divisions, the potential for increased inter-divisional collaboration and decentralization, and the future role of the regional offices. It is essential that any such review be undertaken by a multidisciplinary group that includes those - either in or outside the Centre - with working experience in Africa.

VIII. PROGRAM OFFICERS' MORALE AND WORKING CONDITIONS

The real strength of the IDRC has always been the quality and dedication of its professional staff, and in particular the program officers. For them to be able to make the full contribution that they can - and want to - make to IDRC activities they must have a clear and realistic mandate. They must also have the time and the incentives to perform it. In our judgement, the program officers in SSD at present are not now well served in these aspects. Repackaging and reorganization of the administrative structure are largely irrelevant to the operations of the program staff. The key issues for them are (i) the clarity of their mandate and their authority, about which we have already had a great deal to say; (ii) their workload and incentives; and (iii) their location (see section VI). In this section we devote primary attention to workloads and incentives.

Management of both the Centre and the SSD, while well aware of the longstanding problems of the Division, is almost certainly underestimating the continuing extent of the low morale of SSD program staff. The malaise and disaffection among program officers are infectious; they affect the morale of support staff as well. Severe grievances regarding workload, uncertainty of direction, unsympathetic bureaucratic responses and/or meddling, and a perceived failure to be understood, some of which are common to all large organizations, are not merely characteristic of a few "malcontents" (as has sometimes been suggested by outsiders). They are pervasive. Knowledge of the Division's problems discourages good applicants for the many vacancies. At the same time, it encourages those who are there to consider alternative employment more seriously. We have been genuinely shocked by the degree of unhappiness of so many of the SSD staff. The hurried - almost chaotic - IDDR process and the extra meetings and paperwork required for it have not helped matters. Nor does the IDDR seem to have increased staff confidence that problems may soon be set right. The "churning" and

reorganization have not, as we have noted, achieved internal agreement on new, more focused, priorities. The continued good performance of the program staff is entirely attributable to their dedication to the IDRC's stated objectives, not to a sense of belonging to an effective and focussed IDRC "team". The problems besetting the SSD must somehow be addressed more effectively, and quickly. Hasty, ill-considered, and insufficiently discussed responses to the Division's longstanding problems may ultimately do more harm than good. The hiring of several new professionals offers opportunities for some fresh beginnings - but only if the environment into which they step has been dramatically improved.

(i) Workload

Heavy and increasing workloads have been a key constraint upon SSD effectiveness and the maintenance of SSD morale in recent years. Increasing paperwork and other "in-house" demands on staff time have combined with the staff freeze to place unprecedented demands on Program Officers. Contract personnel, with weaker professional credentials than permanent staff, have (necessarily) been doing some of the work of Program Officers, despite the impropriety of this approach and some "official" denials that it is occurring. It is critically important to expand the SSD professional staff and we warmly welcome the news that this is, at last, to happen. At the same time that new professional staff are added to the SSD complement, more serious efforts must be made to utilize available resources more efficiently. There is a profound sense at present that professional staff are not being well utilized, and that important tasks are not now being adequately performed.

Proposals to increase collaboration with other Divisions, to improve evaluation and follow-up activities, and indeed to process more research funds, may founder if the expanded staff remain bound by current constraints. There are a number of

"reforms" that we believe should be considered. These include:

(a) Greater use of secretarial and support staff for routine administrative tasks (e.g. organizing research clearance). Our consultants commented on the very high proportion of rather routine work in the project files, for which the very high capabilities of program officers are quite unnecessary. Probably about two-thirds or more of the material in the files is of this sort (e.g. letters reminding researchers of deadlines, asking for better financial estimates, arranging visits, writing about research clearance).

(b) Reduced "nursing" of projects, especially in countries where the researchers are quite mature. For instance, does the IDRC staff member need to identify and negotiate for consultants for particular projects? Could this not frequently be done by the researcher? Researchers could sometimes also be asked to prepare their own Project Summaries. Pushing more of this work onto the researcher would in the long run add to the capacity of researchers, through the process of learning-by-doing.

(c) Greater use of outsiders to help review proposals. WHO and the British Overseas Development Administration, for instance, make enormous use of outside advice to review research proposals, much of which is provided free. Established research scholars or groups of research professionals in the developing countries, with whom IDRC has maintained long-standing relationships, could also be employed more frequently for this purpose.

(d) Reduced duplication of effort. The files suggest that in some cases the Regional Office of IDRC and the Ottawa office both review the same proposal, and this is surely not always necessary. More generally, the limited autonomy of Program Officers in the field has emerged as a serious problem-generating cumbersome and time-consuming approval processes that other funders of research in Africa and Latin America described to us as "constipated" and "sclerotic". Increased delegation and decentralization of budgetary authority would ease this

problem (see section VI).

(e) Having larger projects and providing institutional support in some cases, rather than project support (see section VII).

(ii) Maintaining Professionalism

The maintenance of IDRC staff morale involves far more fundamental issues than workload, important as that is. The challenge of maintaining a professional cadre whose members' main task is to participate in the process of research funding, while having little if any time to undertake research or even reading themselves, is undoubtedly a difficult one. In these circumstances there are bound to be some tensions and dissatisfaction. It is important to be aware of their potential severity, implications, and some of the ways by which they might be at least partially alleviated. The problem is now severe in the social sciences.

The dilemma is that to fulfill a heavy research funding load the individual has little time left to read or engage in research; but unless he/she at least keeps abreast of his/her areas, competence in the principal task is likely to suffer. When the average level of professional skills slips, the air of intellectual excitement is likely to slip too.

All research-related institutions, and their component divisions, tend at times to become introverted (e.g., the World Bank, many individual university departments, etc.) and to lose some of the desirable contacts with the broader research community. For the SSD, the fight to avoid such introversion is particularly difficult given workloads, the inevitable myopia attendant on a lot of contact with a specific set of researchers (the clients), and the influence associated with having monies to dispense. Counteracting such introversion is essential to the performance that we expect and hope for from the Centre and the SSD.

The implications of the failure of a staff member to be up-to-date with current research results and methodologies may be serious. How can this problem be overcome?

The previous (1984) Report of the Ad Hoc Committee of the Board of Governors on the SSD noted that there had earlier been an implicit policy that there would be regular turnover of program officers "which would allow for the injection of more recently trained or up-dated professionals" but that in fact tenures were tending to lengthen. This was in turn felt to imply the need to ensure the maintenance of high professional standing of the program officers through such devices as sabbaticals, the opportunity to take courses and to participate in special training programs, etc. Undoubtedly there is a real need along these lines, one which warrants careful consideration. The tendency to slip out of date methodologically after 5-10 years of program work is one side of the story, though perhaps not as important a side as sometimes imagined. Given the range of issues which SSD officers seem to handle, a recent Ph.D. is in any case not "well trained", since the job calls not only for depth and sophistication in a narrow area but also for breadth, more likely acquired by wide reading and experience than from most graduate programs. Especially in matters of priority setting, it is implausible to expect recent graduates to have the feel which tends to come from involvement in various areas over a period of time, and the resulting capacity to make knowledgeable judgments about their relative importance. A program within the Division, and certainly the Division as a whole, should function better with a good mix of maturity, breadth, and up-to-date knowledge of new methodologies and approaches. This almost certainly implies an increase in the proportion of professionals from the Regions in the Regional Offices (see also section VI). Management needs to think in these terms as it undertakes the hiring and retreading of personnel.

One approach is to acquire quick access to the state of the art in the various fields. The reading (or, even better, the

writing) of survey articles is one component of this; personal contacts with the relevant researchers, contracted papers or talks by researchers, attendance at professional meetings, periodic requests for the participation of outside reviewers in assessment of projects or comments on final products, are others. Though IDRC personnel are well aware of the payoffs to these tools, the pressure of work doubtless restricts their use below what it might be and in some cases should be. One tool which might be quite useful but is seldom available in the social sciences would be a guidebook to methodological issues (approaches, pitfalls, etc.) in given research areas. Unfortunately researchers themselves, while living through the problems (at least the ones they are aware of) and usually describing some methodological issues but not others, tend to leave an incomplete record in published work. In any case the record tends to be scattered through the published work. Compilations of problems would facilitate the work of the staff member who would otherwise not be able to put himself abreast of them and would thereby be a less capable screener, monitor, and constructive critic for funded research.

For the research-inclined staff member, aids would include modest amounts of research assistance (especially economical in Third World countries), facilitation of contracts with researchers in the same area, and recognition of research achievements.

Program Officers at present have little time and little encouragement to develop professionally and to maintain the high standards expected by the Centre. The Centre offers no effective incentives for research and publishing (on an individual basis) and study leaves are hard to obtain. These issues should be addressed both by the SSD and by the Centre as a whole.

(iii) Staff Location

PPR-IX (1987) states that "Program staff time is the most significant strategic decision made by the Centre" (pp. 10-11).

We concur. We are enthusiastic about the plans to expand the SSD staff in the Regional Offices. We would especially urge that hiring efforts be vigorous in the Third World. We were disturbed to learn that advertisements for posts were planned for Canadian newspapers but not always for those of the regions in which the officers were to work; and recommend that IDRC-wide recruitment practices be reviewed to ensure that they are both efficient and equitable. Deploying a higher proportion of program staff in the Regional Offices is an important strategic decision. As we have emphasized above, however, we fear that without concomitant changes in budgetary procedures, these changes will be far less productive than they might otherwise be (see section VI).

(iv) Board/Staff Interaction

We believe it important to report to the Board the view we heard (which may not be exclusive to the SSD) that Board members are unlikely to be able to appreciate or understand the problems of Program Officers at the project level because there are so few opportunities for Program Officer/Board interactions. Perhaps there may be some simple changes in the way the Board uses its time during its meetings, the accessibility of Board members to Program Officers and vice versa, etc. to increase the sense of collegiality in what are now seen by some as excessively hierarchical Board working arrangements.

IX. DISSEMINATION

The IDDR's thoughtful and extensive reflections upon the difficulty and complexity of moving from research in the social sciences to "utilization" (in chapter VI) are consistent with our own views on this subject. We sense, however, that the SSD may have been pressed by non-social scientists in the Centre towards more "marketing" efforts than many believe are likely to be productive. Efforts to increase the legitimacy and visibility of SSD research may sometimes be helpful through expanded publications, translations, wider dissemination, etc. We doubt, however, whether the SSD should, as suggested, spend much "more time expanding, systematizing, and accumulating information about its own dissemination efforts and those of the recipients it funds", and doubt that "of greatest potential ... is the strengthening of efforts to systematize and store information regarding research methods and results" (VI-22), or that the Division should aspire "to ensure the utilization of results from the research it supports" and employ "greater utilization of research results ... as an explicit criterion for performance appraisal" (VI-23). These are bureaucratic rather than analytical responses to the utilization issue - and, in most instances, will be like "pushing on a string". We are somewhat reassured by the concomitant statement, with which we agree, that "the Division should not direct large amounts of funding to the support of downstream activities" (VI-22).

There are, however, important issues here. And dissemination of the results of SSD research could be improved by some inexpensive innovations.

Research often goes undisseminated or is too narrowly disseminated. Sometimes it is not really completed at all; other times it is not brought to the quality level needed to be useful. For research on narrower local issues, its potential influence may be fully achieved if only a few users are reached. But in many important social science research areas individual pieces of

research have their principal effect by contributing little pieces to the solution of a bigger puzzle which is ultimately put together by second stage integrative research. In such situations the whole potential contribution may be lost through lack of dissemination.

Pursuit of publication can contribute an antidote also to the problem of the "nearly adequate" research product. Not infrequently a body of data is not mined quite enough, or comparisons and tests undertaken are not quite the most useful, with the result that a 10 or 20% shortfall of work leads to a 50-100% shortfall in value of the product. The final product risks looking like a Ph.D. thesis draft on which no critical comments have been offered. Too often projects are left incomplete because the researcher or his/her centre needs the financial support of a new project. "Follow-up" activities, e.g. focused conferences, can sometimes help to reduce lost potential, but the publication process is usually more important. It can both induce more effort from the research and provide critiques from other knowledgeable researchers.

IDRC should try to discourage the premature abandonment of research efforts by both its financial and monitoring procedures. As IDRC-funded research involves more and more complicated analysis, and assuming it continues to draw many of its clients from outside the most competent and experienced researchers, those situations (or premature abandonment) will be frequent. They highlight the possible value of:

- i) modest inputs from specialists in the research area at the project design phase and in final product review;
- ii) networking across researchers pursuing similar themes in different countries, even if only some are IDRC-funded;
- iii) attempts to contribute to the international dissemination of research results, even when the research is very country-focused.

The SSD does all of these things in varying degree. The payoff to its efforts would be greater were it possible to push them all further. In short, there is cause to worry that much potential is lost if IDRC's involvement in the research/critique/dissemination process is ended too early.

The Ad Hoc Committee reviewing AFNS worried last year, that whereas the division's deliberate focus on "neglected crops in difficult environments for poor people" was a fine example of "putting our money where our mouth is ... another round of neglect may ensue unless some follow-up is done to find out what has happened to the tender loving care lavished on these crops."* Much good social science research seems often to disappear without much trace. Burdensome and difficult as it may be, IDRC needs to study the outcomes of its efforts (see also section X) and consider more extensive post-research involvement.

More broadly, IDRC-financed social scientific research has been more oriented to specific countries (or regions) and specific problems than most academic research. Partly for this reason, much of the work does not enter what may be called the "international literature", and does not thereby play a significant role in shaping the broad evolution of thought on some major issues. There are obviously many exceptions to this, and it is in any case not a criticism per se since time-, area- and problem-specific research has its own important role to play. But this feature raises the question of whether the division should aim for a broader international impact (not necessarily only by itself but also via collaborative efforts with the other divisions) than it has had, and if so how this objective might be pursued. The question warrants special thought given that (i) the research potential in Third World countries is rising rapidly, and funding is a major constraint in many countries, (ii) work on development, and particularly poverty problems, by

*Ad Hoc Committee Report on the Review of the Agriculture, Food, and Nutrition Sciences Division of the International Development Research Centre, Jan. 1986, p. 22.

developed country academic researchers is not growing dramatically, (iii) major institutions like the World Bank have their own limitations because of ideological narrowness, lack of quality control and the pressure of time constraints. One can conceive of the SSD-IDRC role being a quite significant one in this domain if the secret of its greatest successes - identifying important problems earlier or better than others and helping to organize strong research efforts on them - could be repeated more often. A major contribution might be made via the sponsorship of more studies that synthesize worldwide, regional or sub-regional research results and draw lessons therefrom in selected problem areas.

Efforts might also be made to take better advantage of "centres of excellence" in the developing countries in the dissemination of research results, their own and others, to weaker institutions within their own countries and regions, and even in other regions. (Latin American participation in the African macro-economic research network, for instance, has been highly fruitful and much appreciated). Ways of involving such centres in assisting regional program officers in the development and monitoring of local projects and building research capacity in peripheral areas might also be explored.

X. PROJECT EVALUATION

While an enormous amount of work is put into the development of projects and their supervision, less attention is paid in a systematic way to the results, and to evaluation of the research after the project has been completed.

Project completion reports (PCRs) are supposed to be completed six months after the research has been completed. The SSD is by far the IDRC division most delinquent in the completion of the PCRs. The record shows that in early July, 1987, the number of project completion reports outstanding in the Centre was 824; of these, 419 were those of SS (as against 174 for AFNS,

130 for HS, 54 for IS, 24 for EES and 21 for COMM). Worse still, the value of the completed SSD IDDRs is often dubious. PCRs are frequently done by summer students who pore over the files trying to evaluate the projects and answer the six OPE questions. Two key questions such as "What lessons were learned which would allow IDRC to develop better projects in the future or to improve the policies and practices?", and "Was the project worthwhile?", for instance, cannot possibly be answered by a summer student unrelated to the project.

Moreover, the influence (if any) of most social science research probably comes with a lag. As we have noted above, many individual pieces contribute to a gradual build-up of information in a given area; in other cases the value of the research is not immediately recognized. A major element in any sensible and in-depth assessment of the payoff to SSD supported research would have to involve assessments say 3-5 years after completion, sometimes longer. The completed PCRs are, in any case, usually filed away and are not now widely read.

There must obviously be a formal "completion" of a project but the purpose of PCRs is not now clear. There is consensus among SSD Program Officers that each project should have a closure document and that some formal feedback evaluation is beneficial but also that the present PCR is not working.

A new PCR questionnaire has now been designed; its 36 items concentrate on bureaucratic matters, useful for the preparation of basic statistics for the Centre and its Divisions, but it is thin on those aspects which could be used for the evaluation of projects. The new questionnaire seems unlikely to be able to serve as the basis of SSD project evaluations. Yet such evaluations are essential. The SSD has the largest number of projects in the Centre, it works more frequently with less established research personnel and institutions than the other Divisions. And social science projects, by their nature, are often less well defined than projects in other divisions and, as a result, can experience more problems. Evaluation should be an

intrinsic not an ad-hoc procedure if SSD is to learn from past experience.

Program Officers in the SSD say they have no time to evaluate projects. But the evaluation of projects is still the best way to understand the performance of a division and of each sub-program, and it is the crucial input for the discussion of future activity. It is also crucial for the detection of errors, and the preparation of new approaches in particular areas of knowledge.

Ways must be found for useful evaluations to be undertaken. In quite a number of cases, for example, there has been a review of a set of projects after completion - e.g. six studies of infant mortality, and four Korean projects supported by the Science, Technology and Energy Policy Unit. These not only are interesting of themselves but they also contain lessons for future IDRC activity.

It would be desirable to make this type of evaluation regular rather than a sporadic part of program officers' activities. Periodic reviews of all or most projects, in clusters of similar projects would be valuable as an opportunity (a) to summarize and communicate what has been learnt substantively; (b) to criticize methodology and make proposals for improvements which could feed into future work.

Projects could be clustered by subject matter when a number (say 3-6) have been completed on the subject, allowing some time between completion and evaluation, and should be reviewed either by an IDRC staff member or a consultant. The evaluation should summarize projects and results and evaluate methodology. Questions which it would be useful to cover include: assessment of how far initial objectives were met and methodology followed; similarly with respect to budget and time; how far results have represented a useful addition to knowledge; contribution to building up research capacity; and lessons for future projects. Once completed, such reviews should be made available to interested Board members.