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The World Bank

Organizational Manual Statement

RESEARCH COMMITTEE

1. The Research Committee was established to advise the Vice President, Development Policy, on the scope and content of the research program, including its priority requirements and relationship to objectives, and on the implementation of the objectives of that program. Specifically, the Research Committee has the following tasks:

a. To recommend individual research projects, and the allocation of the External Research Budget among different subject areas;

b. To monitor the progress of the research program and recommend appropriate means of ensuring the success of research projects, including the allocation of staff resources;

c. To evaluate the research program and individual research projects;

d. To assist in the coordination of research with other agencies;

e. To encourage coordination between researchers and between researchers and users in the Bank; and

f. To foster the dissemination of research output.

2. The procedure for submitting proposals to the Committee is explained in a booklet called Policies and Procedures. Current research projects included in the External Research Program are described in World Bank Research Program: Abstracts of Current Studies, a booklet issued once a year.

3. The Research Committee is chaired by the Vice President, Development Policy, who appoints the members of the Committee. The members are selected from all parts of the World Bank and normally serve for a period of two to three years. The composition of the Research Committee is widespread to ensure a balance between the users and producers of research.

International Bank for Reconstruction and Development

FOR OFFICIAL USE ONLY

FOR EXECUTIVE DIRECTORS' MEETING

For consideration on November 6, 1979

R79-221

FROM: The Deputy Secretary

August 23, 1979

REPORT OF GENERAL RESEARCH ADVISORY PANEL

1. Attached is the report of the General Research Advisory Panel, chaired by Sir Arthur Lewis. The report completes the program of external review of the World Bank's research program initiated in 1977. The reports of six Specialized Research Advisory Panels have already been distributed to the Board as follows:

SecM78-493; June 6, 1978:	Report of the Research Advisory Panel on Income Distribution and Employment
SecM79-461; June 22, 1979:	Report of the Research Advisory Panel on Agriculture and Rural Development
" "	Report of the World Bank Advisory Panel on Commodities
" "	Report of the Research Review Panel: Energy, Water and Telecommunications
" "	Report of the Research Advisory Panel on Industrial Development and Trade
" "	Report of Transport Research Review Panel

Distribution:

/Cont'd.

Executive Directors and Alternates
President
Senior Vice President
President's Council
Vice Presidents, IFC
Directors and Department Heads, Bank and IFC

In addition, factual information on the research program was most recently provided by the Updating Report on World Bank Research Program, June 19, 1979 (SecM79-461).

2. Questions and comments on this report may be addressed to Mr. Shankar Acharya, Research Adviser (extension 60001).

Report of the

General Research Advisory Panel

August 1979

TABLE OF CONTENTS

General Research Advisory Panel

List of Members

W. Arthur Lewis (Chairman) - Professor of Political Economy, Princeton University, Princeton, New Jersey, U.S.A.

O. Aboyade - Professor of Economics, University of Ibadan, Ibadan, Nigeria

David E. Bell - Executive Vice President, Ford Foundation, New York, New York, U.S.A.

H. Giersch - President, Kiel Institute of World Economics, Kiel, Federal Republic of Germany

N. Islam - Assistant Director General, Economic and Social Policy Department, Food and Agriculture Organization, Rome, Italy

M.J. Kim - President, Korea Development Institute, Seoul, Korea

E. Malinvaud - Director General, Institut National de la Statistique et des Etudes Economiques, Paris, France

C. Massad - Senior Advisor, Economic Commission for Latin America, Santiago, Chile; Professor of Economics, University of Chile

Summary and Principal Recommendations

I. Introduction

1. The present General Research Advisory Panel was asked to evaluate the Bank's economic research program and to make recommendations for the future. The Panel has relied on the reports of Specialized Research Advisory Panels in the fields of agriculture and rural development; commodities; income distribution and employment; industrial development and trade; energy, water and telecommunications; and transportation, as well as on the reports of external advisory panels which have examined the entire range of Bank activities in the areas of population and education. The recommendations of the Panel are summarized below.

II. The Bank's Role in Research

2. Objectives and Criteria. The Bank's research objectives must be stated in relation to the policy needs of its member countries as well as its own analytical requirements. While focusing on its operational needs, the Bank should not neglect the contribution to the understanding of the development process that its central position makes it particularly qualified to provide.

3. We endorse the four objectives of Bank research formulated in 1971:

(i) To support all aspects of the World Bank's operations including the assessment of development progress in member countries;

(ii) To broaden understanding of the development process;

(iii) To improve the Bank's capacity to give policy advice to its members;

(iv) To assist in developing indigenous research capacity in member countries.

4. We further suggest that the Bank should rely on exploiting its comparative advantages in research, which may be defined as follows:

(i) The Bank should capitalize on the skills and information that it acquires through its activities as a lender and as an adviser. However, fruitful analysis of data presupposes an adequate conceptual framework, so we would expect some part of the Bank's research program to be concerned with theory.

(ii) The Bank should excel in comparative studies, though its program should not be confined to work of this kind.

(iii) The Bank is exceptionally well placed to handle larger investigations involving several manyears of research time, and we would expect rather more than half of its research manpower to be engaged on projects of this kind. However, many medium and small projects will also qualify for inclusion in the program, because they exploit the Bank's comparative advantage in data and experience, and because of their relevance to the Bank's immediate needs. In any case, the Bank should not embark upon an investigation unless it is willing to commit sufficient resources to see it through.

5. Relations with Other Research Organizations. As a major actor in the field of development research the Bank has a responsibility to consider the impact of its activities and decisions on other organizations active in the field. In general we would encourage the Bank to adopt a more collaborative research stance than it has in the past.

6. Researchers in Developing Countries. We endorse the general idea of collaborative research as the major instrument of interaction between the Bank's research program and developing country researchers and recommend that the number of such collaborative projects be increased. We also urge the Bank to exploit the opportunities for collaboration represented by studies financed under loans and credits as well as studies undertaken in connection with country economic and sector work. Furthermore, we recommend that the Bank join with other funding organizations in the support of regional research institutions and that it offer postdoctoral fellowships to researchers associated with institutions in developing countries for research to be undertaken at the Bank. Finally, the Bank might support the creation of socioeconomic research institutions as projects to be financed under Bank and IDA loans and credits.

7. Researchers in Developed Countries. We endorse the general reluctance of the Bank to finance research by institutions in developed countries. We recognize that cases may arise where special expertise is needed which is only available from institutions or consultants in developed countries. We would encourage the Bank to play a coordinating role in research involving developed and developing country researchers when it can do so at minimal financial cost.

8. Other International Organizations. It is our impression that a good deal more can and should be done to insure effective collaboration on research among the Bank and other international agencies. In particular, the Bank should be very active in communicating its research plans and inviting comments on them, while continuing of course to make its own decisions on what research to undertake.

9. Data Collection. We recommend that the Bank make a systematic effort at documenting and inventorying data generated by its research and operational efforts and that it establish the necessary in-house capability

in the specialist functions that statistical expertise and data management involve. However, expenditure by the Bank on primary data collection should be related mainly to specific data needed for its research or lending operations or to specific topics for which it feels a particularly urgent need. Finally, we recommend that the matter of data collection on income distribution and employment, raised by the RAPIDE panel, be studied by specialists in the field who would examine:

(i) How a program in this area might be organized from a few pilot surveys to a more systematic effort aimed at producing international comparability.

(ii) In what ways the Bank might contribute to organizing and supporting such an undertaking.

III. The Organization of Research Within the Bank

10. The Research Community. While it is difficult for us to enter deeply into organizational questions, it seems possible to suggest some criteria for improving the present organization. Among them would be the following:

(i) The need for a minimum critical mass, which is now lacking in certain areas.

(ii) The need to balance the requirement of continuity of research by individuals against operational involvement.

(iii) The need for a balance to be struck between the use of staff and of consultants on individual projects.

11. Influence of Operations on Research. Additional machinery is needed to bring research and operational staff closer together in the generation of research programs. We suggest that for major areas of research the Bank create 'Steering Groups,' consisting in about equal numbers of suppliers and consumers of research in that area.

12. Other Social Scientists. A final issue relating to the organization of the research community within the Bank is the role of non-economists. Bank research has been dominated by economics. However, with the Bank's more purposive attempts to encourage and support social change and to comment on such change, the Bank will need social scientists in other fields, particularly sociology and political science.

IV. Research Applications, Dissemination and Training

13. Application of Research within the Bank. As the research program has come of age, there are increasing demands for research applications from the operational departments. Since there are no explicit budgetary and staff allotments for this purpose at present, there is a problem of promoting the application of new analytical techniques in the Bank.

14. In cases where highly specialized skills are involved (such as the construction of sector programming models) it would be worthwhile to establish a special unit to undertake applications of these techniques on a Bank-wide basis. Apart from such highly specialized modelling, most applications can be undertaken by CPS and DPS professionals, and we recommend that the CPS and DPS staff be increased by six man years for the purpose of research application.

15. Parallel to this strengthening of the central staff, the Bank should stress the role of regional Chief Economists in articulating regional research needs and in the application of research results in country economic, project and sector work. The Chief Economist should be provided with additional staff for this purpose. For the immediate future, we would suggest appointing one senior economist, with wide interests and some research experience, in the office of each Chief Economist, to concern himself primarily with the promotion of research and the application and dissemination of research results. This number could be increased as the work expands.

16. Dissemination and Training in the Bank. Apart from the existing modes of research dissemination, we would suggest that brief reports be prepared on the findings of individual research projects, which should highlight the relevance of the results for the operational and policy work of the Bank. Furthermore, we recommend that state-of-art papers be prepared to review research areas that are of particular interest to operational staff and that seminars be organized, using these papers as inputs. A further spur to assimilation of research by operating staff would be provided if researchers participated in reviewing the output of operational economists in their field. Dissemination and training objectives would also be served by extending the rotation of Bank staff, expanding the scope of sabbaticals and instituting advanced training programs.

17. Research Application and Dissemination Outside the Bank. We suggest that the Bank extend the distribution of its research related documents to all research institutions of some standing in the developing countries, as well as to institutions working on development problems in the developed countries. More frequent arrangements should be made for authors to visit developing countries and hold seminars on their results. To the extent possible, research results should be introduced into courses at the Economic Development Institute. The Bank's resident offices should play a role in ensuring proper linkage with the local research community.

18. In cases where Bank research has produced new techniques, there is sometimes demand for training in such techniques from member countries, beyond what can be accommodated through the use of existing staff and budgets. The use of specialized consultants may be the most effective answer to this problem, and the Bank should be willing to share the cost involved, not out of the research budget, but as a part of its general budget.

V. Size and Priorities

19. Size. We begin with the observation that the development process in most of its aspects is still insufficiently understood and that more research on development issues is strongly warranted. While there are other individuals and organizations able to contribute to the needed research, there is a prima facie case for additional Bank-financed social science research on development. The need for such research is urgent and the Bank is well equipped to undertake it. We find the Bank's research record to be good in terms of quality and usefulness and an increase in Bank research seems both desirable and feasible.

20. Our recommendations for increased collaboration and institution building, data management and retrieval, and expanded dissemination and application would by themselves cost about $3.5 million a year at today's prices. In addition we recommend that aggregate expenditure on socioeconomic research (as defined in the administrative budget) be expanded at a rate up to ten percent a year in real terms, which cumulates to about 60 percent over 5 years. The ten percent expansion of the research program would be about $1.0 million in the first year. Another review like the present one might be commissioned in about five years to make recommendations for the subsequent period.

21. Priorities. We endorse the past movement in Bank research to such newer fields of Bank emphasis as agriculture and rural development, urbanization, and income distribution and suggest that over the coming period still further shifts in emphasis may be warranted. We have also reviewed the list of research topics proposed by the various specialized panels and selected those which appear to us to be of particular importance for the Bank to undertake. We have further suggested topics of our own in areas not covered by the specialized panels. However, precise allocations among types and fields of research, and the choice of particular projects, are matters which the panel considers can only be decided by the Bank, taking into account such factors as the availability of particular skills in the staff, and the comparative quality of research proposals. We would not wish any list of ours to inhibit the Bank in these managerial functions, whether by pushing topics of which the Bank is doubtful, or excluding topics it might otherwise favor.

Chapter I

Introduction

1.01 Research has been an established part of the World Bank's economic work for over twenty years. In the latter half of the 1960's, Bank research activities began to expand in response to the diversification in Bank operations. In 1971, the Bank created a separate, centralized budget for research. At the same time a standing committee known as the Research Committee was established to formulate guidelines for Bank research and to administer the Bank's research budget. Four objectives for Bank research were enunciated in 1971. They are:

(i) To support all aspects of the World Bank's operations including the assessment of development progress in member countries;

(ii) To broaden understanding of the development process;

(iii) To improve the Bank's capacity to give policy advice to its members;

(iv) To assist in developing indigenous research capacity in member countries.

1.02 Most individual research projects are reviewed on completion, but since 1977 the Bank has sought more comprehensive reviews of its research activities in a number of areas. Specialized Research Advisory Panels (SRAPs) of outside experts have evaluated the Bank's research in agriculture and rural development; commodities; income distribution and employment; industrial development and trade; energy, water and telecommunications; and transportation. These panels have assessed the quality and relevance of past Bank research in their respective fields, identified important areas for further research and provided their views on the special contribution that the Bank could make in such research. The reports of the SRAPs also make valuable comments on such general issues as the organization of Bank research, relationships between producers and users of research within the Bank, and the Bank's relationships with research institutions in member countries. In addition to the SRAPs, external advisory panels have examined and reported on the entire range of Bank activities in population and education. These panels have commented on Bank research activities in these fields, and we have read these observations with care. 1/

1/ Annex I lists the members of these panels and their reports.

1.03 The present General Research Advisory Panel was established to provide judgments on general issues concerning the Bank's research posture in the future. Our draft terms of reference defined this task as follows:

> "The primary objective of this review will be to assess the size of the overall research program and its character and balance. This should be done in the light of the Bank's operations in, and advisory services to its member countries, with consideration given to the role it should play in increasing worldwide understanding of the development process."

The panel was also asked to assess whether the objectives presently enunciated for the program (cited in para 1.01 above), needed reformulation or ordering in terms of priority.

1.04 The panel was invited to establish its own final terms of reference after its meetings with Bank staff. Our deliberations have been concerned with the following broad questions:

(i) What should be the objectives of the Bank's research program?

(ii) What are the Bank's research needs?

(iii) What kinds of research is the Bank most suited to undertake?

(iv) What policies should guide the Bank in its relationships with other research institutions and other researchers? What responsibilities should the Bank assume toward the development of indigenous research capacity in member developing countries? How best can it sponsor such development, given its own needs and abilities?

(v) What role and responsibility are appropriate for the Bank in the generation, management and dissemination of data?

(vi) What improvements are needed in the organization and execution of research by the Bank?

(vii) How can the Bank better assimilate the fruits of its research in its operations? How can it better propagate these results to its other chosen audiences?

(viii) What has been the quality of Bank research, and how useful has it been to Bank operations?

(ix) What is the appropriate size for the Bank's research program?

(x) What are important areas of research for the Bank to enter in the future, given its objectives, aptitudes and needs?

We have been conscious of the intrinsic difficulty of answering many of these questions, especially for a group of outsiders not very familiar with the internal workings of the Bank.

1.05 The Panel has had some difficulty obtaining a complete picture of the Bank's contributions to research on development. This is in part because the Bank's activities are naturally diverse; the Bank is involved through its projects, its policy advice, and otherwise, in seeking to apply what is known and what is being learned about the entire complex, sprawling subject called "development." Our difficulty also stems in part, in our opinion, from the fact that the Bank has become increasingly involved in research over the past decade in a variety of ways and from several different sources of initiative, without bringing the whole picture together in any publication or organizational unit. At present, the Bank's contribution to research breaks into at least five parts:

(i) Research funded in part from a central research budget (known as the "external research budget") and allocated through the Research Committee, amounting in fiscal year (FY) 1978 (including the value of staff time) to about $6.2 million (in FY79 dollars). Of this amount, roughly $600,000 was spent on contracts with researchers and research institutions in developing countries.

(ii) Research financed from departmental budgets and not subject to central review, amounting in FY78 to about $4.1 million, also in FY79 dollars.

(iii) The Bank's contribution to international collaborative research in agriculture (the Consultative Group for International Agricultural Research (CGIAR)) amounting in calendar year 1978 to about $8.7 million, and the Bank's involvement in the Tropical Diseases Research and Training Program for which it currently acts as co-sponsor and fiscal agent.

(iv) Studies financed by member countries under loans and credits from the Bank; though accurate data using consistent definitions are not available, the research element in these studies may in recent years have been as much as $20-$30 million.

(v) Loans and credits for building up research capacity in developing countries (perhaps $120 million in FY78, most of it for agricultural research and extension projects).

1.06 Several comments should be made about these categories. The Bank as an operating organization is primarily concerned with applied research, both in its own staff work and by its member countries, and in any such situation the line between research and its application is inevitably uncertain. The preceding paragraph does not take account of some of the work

undertaken in the Bank's regular program of economic and sector analysis, which could as well be called applied research. We are not troubled by this lack of precision in accounting, and have not tried to pursue unduly fine distinctions.

1.07 As is evident from para 1.05, the Bank has in the past funded both technological research and socioeconomic research. This report is limited to the Bank's activities in the area of social science research, particularly research in economics. We nonetheless believe technological research is important, and that technological and socioeconomic research can often reinforce each other in very important ways—as shown, for example, by the international agricultural research centers. We are aware of the Bank's desire to review its position as far as technological research is concerned, and we believe there may be important opportunities for additional Bank financing. Accordingly, we suggest that a further panel (or panels) focusing on technological research might be helpful.

1.08 The deliberations of our panel have been conducted in Washington, where we have had the opportunity to obtain the views and assistance of Bank staff. In addition, individual members of the panel have consulted with the chairmen of the specialized panels.

1.09 The report is organized as follows. Chapter II discusses what kinds of research the Bank should finance and the nature of its relationships with other research institutions. It also discusses the Bank's role and responsibilities in the area of data collection and management. Chapter III examines the organization of research within the Bank. Chapter IV is concerned with the dissemination and absorption of the results of the Bank's research. Chapter V assesses the appropriate scale of the research program, research priorities for the future, and balance between major sectors. An appendix summarizes the recommendations regarding future research priorities presented in the reports of the eight advisory panels.

Chapter II

The Bank's Role in Research

2.01 The Bank plays a variety of roles in development research. Over the last decade it has emerged as a major producer of such research and is now a significant part of the world-wide community of researchers on development issues. The Bank is also a major consumer of research results, whether generated by its own research activity, or by the work of other persons and institutions. To some extent the Bank is a contributor to the development of research capacity in developing countries. The objectives of the Bank's research program, the criteria for choice of topics, and the method of execution should reflect this diversity of interests. In this chapter we articulate some general principles that might assist in defining research strategy for the future, taking due account of research capability elsewhere, and the abilities and needs of the Bank. As noted already (para 1.07) we discuss here only the Bank's role in social science research.

2.02 We begin by discussing the objectives of Bank research and the criteria that should guide the Bank's choice of research areas. We then sketch the appropriate relationship between the Bank and the other major participants in development research: research institutions and scholars in both developing and developed countries, and other international organizations. We conclude with a discussion of the role that the Bank should play in the collection, management and dissemination of data. We have chosen to examine this issue both because data are a prerequisite for research, and because the Bank's responsibilities for primary data collection were discussed in the reports of several of the specialized panels.

Objectives and Criteria

2.03 Since the Bank has become one of the largest sources of research on economic development, its research objectives must be stated in relation to the policy needs of its member countries as well as its own analytical requirements. While focusing on its operational needs, the Bank should not neglect the contribution to the understanding of the development process that its central position makes it particularly qualified to provide. In this context we endorse the four objectives of Bank research stated in paragraph 1.01. In the remainder of this chapter we suggest ways in which the Bank may be able to achieve a better balance among these objectives.

2.04 At the most immediate level the Bank's own use of social science research can be considered under two heads: research whose results are likely to influence the design of projects for which the Bank lends, and research that is of importance for the Bank's more general policy functions. These functions include the Bank's role as an advisor to developing countries and the development of the Bank's own policies.

2.05 These lending and policy activities require the Bank to use the results of a wide range of analytical techniques. The Bank's larger role as a leading development institution requires it to keep abreast of current thinking on development processes, and to use the results of both theoretical and empirical research. The Bank should not be content with the state of the art as it finds it, but has an obligation to advance the debate on the objectives and instruments of development.

2.06 To say that the Bank has these varied research needs is not to say that it should attempt to meet all of its needs through its own staff, nor that it should necessarily finance the entire spectrum of research. Nonetheless we feel that a balanced research program for the Bank must necessarily continue to be a blend of project and sector level research, applied country and international analysis and more fundamental analysis of the structure of developing countries and of the development process.

2.07 There remains a need to define more specifically the kinds of research the Bank should seek to undertake. Here we think the decisions should be guided by the special advantages that the Bank enjoys as a research organization, and by its own requirements. As a general proposition the Bank should not embark upon topics that are already adequately covered by other institutions. It should on the contrary rely on exploiting its comparative advantages which may be defined as follows:

(i) The Bank should capitalize on the skills and information that it acquires through its activities as a lender and as an adviser. Particular attention should be given to making use of the Bank's project experience and the data generated in Bank projects. However, fruitful analysis of data presupposes an adequate conceptual framework, so we would expect some part of the Bank's research program to be concerned with theory.

(ii) The Bank should excel in comparative studies, though its program should not be confined to work of this kind.

(iii) The Bank is exceptionally well placed to handle larger investigations involving several manyears of research time, and we would expect rather more than half of its research manpower to be engaged on projects of this kind. However, many medium and small projects will also qualify for inclusion in the program, because they exploit the Bank's comparative advantage in data and experience, and because of their relevance to the Bank's immediate needs. However, the Bank should not embark upon an investigation unless it is willing to commit sufficient resources to see it through.

2.08 Despite the advantage that the Bank ought to enjoy in project-related research, it has not made much use of its project experience or project data for research purposes. This side of Bank research needs to be strengthened. In recent years the Bank has begun regular evaluations of completed lending projects and has increasingly built monitoring and evaluation components into the projects it finances. These should provide additional material and ideas for project-oriented research.

Relations with Other Research Organizations

2.09 As a major actor in the field of development research, the Bank has a responsibility to consider the impact of its activities and decisions on other organizations active in the field. We discuss in subsequent paragraphs the relationships of the Bank's research program to research institutions in developing countries, research institutions in developed countries, and international institutions. Before taking up those relationships, we mention two more general points.

2.10 The first is that the Bank's approach to its research activities has been primarily self-contained. Bank staff have identified research objectives; prepared, executed, and supervised research projects; and interpreted and disseminated the results. Researchers outside the Bank have been involved as consultants employed to assist in Bank-defined work. The principal reason for this tightly controlled research process has been the desire to establish research as a high-quality and useful activity in an organization mainly directed toward operational objectives.

2.11 There have plainly been benefits from this approach to research. It is our impression that research has been accepted and is now solidly established as an appropriate part of the Bank's normal activities. But there have been costs as well. To the outside research community the Bank has appeared somewhat aloof and unresponsive to opportunities for interchange and collaboration. While this impression appears to be exaggerated, we believe there would be much gained by the Bank adopting a more collaborative research stance, and we recommend that the Bank do so. In particular, we suggest that the Bank move toward other methods of research collaboration in addition to hiring outside consultants, and toward greater involvement of outside researchers in the assessment and guidance of Bank research. None of these ideas is new, and in a few instances the Bank has moved in these directions.

2.12 Second, we believe the Bank ought to play a larger role in promoting stronger and more coherent research efforts by the development research community. Research on development is conducted in many places around the world, by many individuals and groups. There can be great strength in this diversity, and we would not want to see any inhibition on the independence of researchers. But there is clearly need for initiative in arranging for periodic assessments of research progress in different fields; in encouraging the rapid spread and adoption of new research methodologies; in identifying promising new areas for future research; in doing many other things which will contribute to informing researchers and supporting them in doing better research. The Bank is in a

strong position to help with these objectives, as it has shown in some instances already, and we think it should do more along these lines. We are not, we emphasize, recommending that the Bank seek to lead or dominate the development research community - only that it play a part, along with many other organizations, in improving communications, assessments, and supporting arrangements among the world's researchers in the development field. We believe the increased openness by the Bank to external influences on its own research program, which we have earlier recommended, will put the Bank in a much better position to play a more constructive role in the larger research community.

(i) Researchers in Developing Countries

2.13 As was noted in paragraph 1.01, the Bank in 1971 declared one of its research objectives to be "to assist in developing research capacity in member countries." There are many ways in which this might be accomplished: by collaboration on research projects, by grants and loans to institutions, by award of fellowships and so on - and we have reviewed the Bank's activities in this area.

2.14 Thus far, the Bank has related to scholars in developing countries largely through collaboration on joint studies. There have been many such projects under the external research program, and the scope of association has been wide, ranging from assistance in data collection to subcontracting complete studies. The Bank has avoided making block grants to socioeconomic research institutions, or to individual scholars.

2.15 We endorse the general idea of collaborative research as the major instrument of interaction between the Bank's research program and developing country researchers and recommend that the number of such collaborative projects be increased. We feel that research capacity in a growing number of developing countries is now sufficiently sophisticated to make a valuable contribution to the substance of Bank research. Moreover, important institution building objectives can be achieved as a byproduct of research collaboration.

2.16 Certain difficulties however limit the opportunities for such partnership at present. First, the number of LDC research institutions adequately staffed for such research is still limited, and most of these have research priorities of their own which should be respected. The constraints faced by LDC institutions are only partly financial, and insofar as they are financial they can be alleviated by the Bank contributing to both direct and indirect costs in its collaboration agreements. A more fundamental obstacle in some countries is the limited number of trained personnel, and the demands for their services from universities, government departments and the private sector. Elimination of this deficit will take time.

2.17 Another limitation is imposed by lack of data. Countries differ substantially in the quantity and quality of available data; understandably, research has tended to concentrate on those countries where the data are

better. In a number of cases the Bank has sought to overcome this problem by financing the collection of data as a part of the research. This practice should continue in the future as appears appropriate in specific research contexts.

2.18 Collaborative research undoubtedly has certain costs. The structuring of collaborative agreements, the supervision of work plans, the monitoring of output and of quality are time consuming tasks. Collaboration with the weaker institutions may also involve higher risks of failure.

2.19 While we recognize that additional managerial overhead may be required, we regard collaboration as beneficial to both parties. The Bank should be prepared, within reasonable limits, to make the additional investment in time and money needed to foster research collaboration.

2.20 The Bank should, moreover, deepen the involvement of developing country institutions in its research projects and should strengthen the institution building aspects of its collaborative efforts. Collaboration agreements should provide time and budget allocations to cover the costs of bringing overseas researchers to Washington for limited periods. The technical expertise so gained would facilitate the future application of innovative techniques in the country concerned. This would be particularly desirable in comparative projects, where at present the country participants often are not involved in the later stages of the analysis.

2.21 It should be noted that the opportunities for collaborative analytic work with developing country researchers are not restricted to the external research program. There are often sizable research components financed by borrowing countries under project loans and credits. While the disposition of these funds is governed by the loan agreement between the Bank and the borrower, the Bank could play a more active role in providing technical support.

2.22 Similar opportunities are presented by the Bank's country and sector work program, which is several times larger than what is defined as research. A number of studies now carried out in connection with country economic work on income distribution, employment, exports, migration, etc., would constitute suitable topics for collaborative studies with local institutions. There are already examples of such collaboration and its expansion is under active discussion at the Bank.

2.23 We recommend two additional steps which we think will allow the Bank to combine its interest in high-quality research with its interest in strengthening research capacity in developing countries. The first is to join with other funding institutions in channelling research grants through regional research organizations which would assume responsibility for selection of qualified individuals and institutions as recipients of the assistance. Several organizations of this kind have been created in recent years. Such arrangements provide assurance of quality control to the Bank and other funding organizations without necessitating the staffing and

other costs that would otherwise be necessary to distribute research grants effectively. We would encourage the Bank to move cautiously but positively in the direction of making such research grants, in collaboration with other institutions. Experience suggests that supporting researchers in what they wish to do, in areas that coincide with the general research objectives of a funding organization like the Bank, can be a highly efficient form of research support.

2.24 The second step we recommend is for the Bank to offer a number of postdoctoral fellowships for research to be undertaken at the Bank. Each fellowship would be tenable for a limited period (say, two years at a maximum), and would be restricted to researchers who are firmly associated with institutions in developing countries. The Bank would have to decide the number of such fellowships it could have at any time: up to twelve persons might be feasible. Even a relatively small program could make a significant difference over time in stimulating local research activity in developing countries.

2.25 We further feel that the development of socioeconomic research institutions in developing countries could be a priority field for Bank action. Research in such institutions can be as important to development as a successful agricultural or transportation project. The Bank should therefore consider the feasibility of financing projects for building socioeconomic research institutions in the same way in which it selects projects for financing in other sectors. Institution building requires sustained efforts at creating or strengthening an organization, training staff at the predoctoral and postdoctoral levels, providing library and data management facilities, establishing strong contacts with leading research institutions elsewhere and promoting a generally stimulating environment. These requirements are best fulfilled within the context of self-contained projects covering a period of several years and financed with a proper balance of Bank loans, credits and grants. Temporary secondment of Bank staff may be helpful in some cases. Existing regional research institutions in developing countries merit special consideration, and in some places the Bank may wish to be associated with the establishment of new regional research institutions.

2.26 We feel it important that the Bank take an integrated view of the research work being undertaken in any given country with Bank finance, something which does not adequately happen at present.

2.27 To summarize, our view is that the Bank's main relationship with developing country researchers should be through collaborative research projects. The scale of such collaboration should be expanded, and the involvement of collaborating institutions deepened. In addition, such collaboration should be encouraged in contexts wider than the external research program. The Bank should take steps to strengthen the institution building aspects of such collaboration. Furthermore, we feel that institution building could be undertaken in a project context, and we would encourage the Bank to consider the feasibility of financing projects for the development of socioeconomic research institutions.

(ii) Researchers in Developed Countries

2.28 We turn next to the appropriate relationship between Bank research and research institutions in developed countries. Past contacts between the Bank and such institutions have been characterized by some of the same problems as those encountered with developing country institutions. In certain respects the situation is easier because developed country institutions are better staffed than their developing country counterparts. However, the managerial effort associated with such collaboration is only marginally less than that associated with developing country institutions. The Bank has on the whole avoided entering into formal collaborative arrangements with developed country research institutions, although such arrangements have characterized a few of the larger projects.

2.29 We endorse the general reluctance of the Bank to finance research in institutions in developed countries. We recognize that cases may arise where a special expertise is needed, which is only available from institutions or consultants in developed countries. There will also be cases where tripartite collaboration between the Bank, a developing country institution and a developed country institution is the best procedure.

2.30 The Bank's contribution to research on development undertaken in developed countries is not restricted to its financial contribution however. The Bank can sometimes undertake a coordinating role while only making a minimal financial contribution itself, and should continue to do so.

(iii) Other International Organizations

2.31 The Bank is not of course the only international institution doing research into the socioeconomic aspects of development. The greatest potential overlap is with the work of FAO, ILO and UNIDO; there is a smaller overlap with UNESCO and the UN itself. Collaboration is obviously necessary. In some cases--research on income distribution is probably a good example--there has been good communication and a series of valuable meetings among researchers from ILO, the Bank, and other agencies interested in research in that field. It is the panel's impression that a good deal more can and should be done to ensure effective collaboration on research among the Bank and other international agencies. At the same time, we do not mean to recommend a constraining effort toward uniformity: research flourishes on competition and the challenging of one researcher's results by other researchers. The guiding principle for the Bank to follow, in our judgement, is to be very active in communicating its research plans and inviting comments on them, while continuing of course to make its own decisions on what research to undertake.

Data Collection

2.32 As regards data required for research, the situation is somewhat paradoxical. Although the inadequacy of data is often cited as a major constraint on empirical research, it is also the case that the analytical potential of such data as exist is not usually fully exploited. While there are

several reasons for this state of affairs, an important one is the generally inadequate provision for the documentation and dissemination of research generated data beyond the initial researchers. Our impression is that within the Bank as well the arrangements for the documentation, archiving and subsequent retrieval of research-generated data are ad hoc, and fall upon the analysts rather than on specialized staff.

2.33 Bank-financed project operations also generate a wealth of data for research. These opportunities are only beginning to be exploited. The increasing practice of having monitoring and evaluation arrangements set up as part of the project is a promising development in this regard. We commend recent efforts to strengthen the technical support provided for monitoring and evaluation analysis. We strongly support this line of activity and are convinced of its potentially high value for research purposes.

2.34 The Bank should recognize that the data generated are an important output of its research and operational efforts. We recommend that the Bank make a systematic effort at documenting and inventorying such data (as has recently been initiated in the Education sector), and that it establish the necessary in-house capability in the specialist functions that statistical expertise and data management involve. Apart from making existing data available for further research such an effort would help to identify particular instances where new statistical investigation would be fruitful.

2.35 The institutionalized generation of time series data raises issues far wider than the research needs for such data. The Bank presently has official responsibility within the U.N. system for data on the debt of developing countries and it has moved to discharge this obligation conscientiously. The maintenance of such capacity is an expensive business; but, as far as we can judge, it is a precondition for providing good information to lenders, and ultimately, therefore, serves to widen access by developing countries to international loans. In considering any extension of such responsibilities to other fields, three questions arise: the Bank's needs for regularly updated data in particular areas; the Bank's comparative advantage in filling these needs by itself; and jurisdictional issues within the U.N. system.

2.36 On the question of Bank needs for data, a distinction should be drawn between the need for country-specific information using country concepts and definitions, and the need for internationally comparable data series. As the Bank itself has come to realize, these are two distinct needs. Country-specific data are useful in the conduct of country policy dialogue. Internationally comparable statistics are essential for both aggregate and cross-sectional analyses. Commentaries on world trends, assessment of development progress and documents such as the World Development Report make use of such cross-country data.

2.37 Despite these different needs, the basic sources for such data remain national statistical offices and country data systems. The Bank should endorse attempts to improve the capabilities of these offices, and to upgrade the quality of these systems. However, direct responsibility for such efforts

has to be assumed primarily by the countries themselves with the help of the U.N. Statistical Office. Expenditures by the Bank should be related mainly to specific data collection needed for its research or lending operations or to specific topics for which it feels a particularly urgent need. Any resources the Bank might allocate for the specific purpose of [illegible] capabilities in LDCs should not compete with its research program.

2.38 A major area where there may be a role for the Bank to play was identified by the Research Advisory Panel on Income Distribution and Employment, which recommended that considerably greater initiative and expenditure be devoted by the Bank to assure collection and dissemination of reliable, consistent and continuous data in the areas of income distribution and employment. We have examined this recommendation.

2.39 We are conscious that the panel's concern was not to collect and disseminate data on income per se, which in poor rural communities is difficult even to define, but rather to collect better information on living conditions. Household surveys provide the proper technique for such an investigation. But setting up suitable surveys, analyzing their results and using them for evaluating changes in welfare levels over time is a major operation which still raises problems, even in countries with developed statistical services. The Bank should not expect quick return from efforts in this area; in particular a long period will be required before any conclusive inference can be drawn from survey data as to the aggregate distributional impact of Bank lending.

2.40 A better knowledge of conditions of living within any country is, however, an important element in any assessment of its social and cultural development, and for this reason we feel that the Bank would be justified in promoting such knowledge. We recommend that this matter be studied by specialists in the area who would examine: (i) how a program in this area might be organized from a few pilot surveys to a more systematic effort aimed at producing international comparability, and (ii) in what ways the Bank might contribute to organizing and supporting such an undertaking.

2.41 There do not seem to us to be other major areas in which the Bank should assume the responsibility for coordinating routine international data collection. We do not in any case feel that the Bank has any particular comparative advantage in undertaking such work, nor that it should supplant existing arrangements within the U.N. system. Rather, in areas where it feels the current efforts to be deficient it should seek to operate through the responsible U.N. agency to improve matters.

2.42 Apart from its role as a producer of data, the Bank has become a major disseminator of cross-country data via documents such as the World Bank Atlas, the recent Atlas of the Child, or the 'World Development Indicators' appended to the World Development Report. These documents are widely used as standard reference sources by those interested in development. Such a role as propagator imposes responsibilities on the Bank for accuracy, consistency and comparability.

Chapter III

The Organization of Research Within the Bank

3.01 We deal in this chapter with issues internal to the Bank: the structure of its research establishment, the influence of this structure on the research program and the machinery for generating and approving research proposals. Our discussion is restricted to social science research financed either through the external research budget or from departmental budgets.

3.02 We begin with the present structure of the research community within the Bank. The Bank's staff of researchers is currently scattered across a wide variety of organizational units. Only one department, the Development Research Center (DRC) in the Development Policy Staff (DPS), is exclusively concerned with research. This unit of twenty professionals has accounted for roughly a quarter of Bank staff time devoted to research. The bulk of the research undertaken by the DRC is in the form of projects approved by the Research Committee. The major areas of concentration for the DRC have been sector planning models, economy-wide models, income distribution studies and effective protection studies. While the DRC has been a leader in these areas, such work has also been undertaken by other departments.

3.03 In contrast to the DRC, the remaining units engaged in research at the Bank are functionally specialized, and they also have responsibilities other than research. The Development Economics Department (DED) in the DPS and four departments in the Central Projects Staff (CPS) have a significant mandate for research. The Development Economics Department (which is the largest research producing department in the Bank) is divided into five functionally specialized divisions. In addition to research, these divisions have responsibility for policy work and they also support regional economic staff in their country and sector economic work. In the CPS the relevant departments have responsibility for functional review and support of projects, for sector work and for development of sectoral policy papers. What is of interest is not only the diversity of tasks mandated to the unit, but also the tasks assigned to the individuals who constitute the unit. Our impression is that CPS staff concerned with research typically undertake a somewhat wider range of activities than is the case in the DED.

3.04 Apart from these units in the CPS and the DPS, the Research Committee funding mechanism makes it possible for some operational units (such as country program departments) to undertake some research as well, supervised if not always executed by operational staff.

3.05 Expenditure on research as provided for in the administrative budget is of two sorts. First there are the funds allocated by the Research Committee. This is referred to as the "external research budget", and it is used to finance "external" expenditure (on consulting, travel, and data processing) on projects approved by the Research Committee (so-called RPO

projects). The size of the external research budget has remained frozen in real terms since 1973, at a level corresponding to roughly $2.5 million in 1979 dollars. Research projects come before the Research Committee only if they need "external" finance in excess of $10,000; if not, the expenditure is carried on departmental budgets, and the work is called departmental research (as distinct from RPO research.) Departmental research projects tend to be smaller projects, but they are numerous: about 60 out of the 140 research projects listed by the Bank in 1978.

3.06 The other budget item is the imputed cost of professional time spent on research. The cost of a professional manyear in research as calculated by the Bank includes an allocation of overhead costs, such as the cost of management and the prorata cost of support departments. About sixty-six professional manyears were devoted to research in FY78; this includes both the time of Bank staff and the time of consultants not financed from the external research budget. Half this total was directly spent on RPO projects, and the remaining half on departmental studies and other research related activities, including research preparation. In FY79 professional time devoted to research is estimated to have declined by fifteen percent as staff were diverted to operational and policy work; the reduction was wholly in non-RPO research and resulted in almost two-thirds of professional time being devoted to projects approved by the Research Committee. As Table 2 indicates, the total cost of research in FY78 was around $10.3 million in FY79 dollars; the cost in FY79 is estimated to have declined to $9.2 million. The share of RPO research has ranged from roughly two-thirds to three-quarters of this total.

Table 2: Resources Devoted to Research: FY77-79 (in millions of FY79 dollars)

	RPO Research			Departmental Studies & Other[a/]			Total		
	FY77	FY78	FY79	FY77	FY78	FY79	FY77	FY78	FY79
"External" Expenditure	2.4	2.4	2.5	-	-	-	2.4	2.4	2.5
Manpower Costs	4.3	3.8	4.3	3.9	4.1	2.4	8.2	7.9	6.7
Total	6.7	6.2	6.8	3.9	4.1	2.4	10.6	10.3	9.2

a/ Including research preparation and other miscellaneous research related tasks.

The Research Community

3.07 This fragmented structure of research units, and the variety of other tasks undertaken by staff engaged in research must have some adverse effects on the efficiency with which they can conduct research. We have therefore considered whether it might not be better to concentrate all major research activity in two or three specialized units like the DRC.

3.08 A unified research community would have some advantages. First, its staff would be more highly specialized in research, having no operational responsibilities; its research capability would therefore on average be higher than that of units where operational skills rank equally with or higher than research skills. Second, it would probably make management of the research program easier. Third, concentrating researchers into two or three units would help to generate a stronger research culture with benefits in the form of better morale, mutual aid, cross-fertilization and achievement.

3.09 The disadvantage of consolidating all research into specialized units is that it would remove research still further from operations, reducing interactions between researchers and operational staff. Also, a large research department is likely to develop a life of its own, independent of the central concerns of the institution.

3.10 While it is difficult for us to enter deeply into organizational questions, it seems possible to suggest some criteria for improving the present organization. Among them would be the following:

(i) The need for a minimum critical mass, which is now lacking in certain areas. This has been stressed by several of the specialized panels.

(ii) The need to balance the requirement of continuity of research by individuals against operational involvement. The Bank currently seems to have a number of staff who devote too little time to research to be wholly effective as researchers.

(iii) The need for a balance to be struck between the use of staff and of consultants on individual projects.

3.11 These criteria suggest firstly that, as much as possible, staff engaged in research should be grouped in units of a certain minimum size whose primary focus is research, even if the parent department has other responsibilities. Second, while it is desirable that some staff rotate between operational and research assignments to promote cross-fertilization, care must be taken to ensure that staff, when engaged in research rather than operational support, have adequate blocks of uninterrupted time for effective research. Third, while involvement in research implies corresponding time commitments on research collaboration, application and dissemination, care should be taken to allow time for genuine intellectual involvement by Bank researchers in the substance of the research; otherwise the Bank will be unable to retain its best research staff. Fourth, for the Bank to ensure that the research undertaken is responsive to its needs, as well as for better absorption by the Bank of research techniques and results, it is important that there be an adequate ratio of staff to consultants on individual projects.

Influence of Operations on Research

3.12 Our next concern is the relationship between research and operations in the formulation of the research program and in its overall management.

3.13 Currently the operational staff complain that the Bank's research program is not sufficiently geared to its operations. While much useful research runs ahead of operations, it is also true that the Bank's operations do reveal problems which could be the basis of relevant and interesting research. The question is to make sure that these problems get on the research agenda.

3.14 The answer is in part that the operational staff must have opportunities to affect the Bank's research program, and in part that they should have the chance to articulate their concerns through the generation of particular research projects.

3.15 As regards the development of research programs, there is need for additional machinery to bring research and operational staff closer together. We suggest that for major areas of research the Bank create 'Steering Groups', consisting in about equal numbers of suppliers and consumers of research in that area.

3.16 Such a Steering Group was established recently in the industry and trade area, with the participation of DPS, CPS, IFC and the Regional Offices. The Group has produced a draft report that makes proposals on research for the next several years, with additional recommendations on staff levels in research and in research applications. We would recommend the Steering Groups consult outsiders, particularly from developing countries, in the preparation of such programs.

3.17 The research programs of the Steering Groups, once established, would be reviewed by the Research Committee and the Steering Groups would also advise the committee on projects in their area. It might additionally be possible to grant the Steering Groups authority for approving research proposals that are part of an agreed-upon program and do not exceed predetermined budgetary limits. The Steering Groups could review the progress of ongoing research within their areas of responsibility and participate in the evaluation of completed activities. This would enable the Research Committee to devote more time and effort to setting priorities among research areas and to consider general issues relating to Bank research, such as research application and dissemination.

3.18 To ensure coordination we recommend making the Research Advisor an ex-officio member of each of the Steering Groups. He would be responsible for coordinating programs in the various areas. The Research Advisor would also play a promotional role in research application and dissemination efforts, and in relationships with developing country institutions. He would need additional staff to discharge these duties adequately, and his position would need to be upgraded.

3.19 Whereas the Steering Groups would develop research programs in particular areas and advise the Research Committee on individual proposals, there remains the need for assistance to operational departments in translating their ideas into research projects. We propose below (para 4.06) that the office of the Regional Chief Economist be strengthened to permit better articulation of regional research interests. Assistance to this office (and to other operating units) should be provided by the Bank's research staff, given their familiarity with Bank operations. Assistance may take a variety of forms: development of joint projects, technical advice in the formulation of proposals, and generation of research based on ideas received from operational departments. Whenever appropriate, operating staff may also obtain the assistance of consultants in formulating proposals.

Other Social Scientists

3.20 One final issue relating to the organization of the research community within the Bank is the role of non-economists. As noted earlier, Bank research has been dominated by economics. However, with the Bank's more purposive attempts to encourage and support social change and to comment on such change, the Bank will need social scientists in other fields, particularly sociology and political science.

3.21 The number of such staff will probably grow even faster in the operational departments than in research, as the Bank expands its lending programs in such areas as rural development, population, education, health services or urban development. We doubt that it would be appropriate to create within the Bank a separate division for social scientists other than economists. Presumably they will be integrated into existing units in much the same way as economists and engineers now serve together. Our immediate concern is that the point of view of social scientists other than economists should be given consideration by the proposed Steering Groups.

Chapter IV

Research Applications, Dissemination and Training

Introduction

4.01 Thus far, we have dealt with the Bank's role in development research, and the organization of research within the Bank. In order for research to influence the Bank's larger program of country economic, sector and project analysis, it is important for the Bank to organize an effective application and dissemination effort internally. Since much Bank research is of direct interest to the developing countries, the Bank needs to have an effective dissemination effort externally as well. In turn, the effective absorption of research techniques and ideas carries implications for the Bank's training activities for its own staff and for officials in developing countries. We discuss these issues in this chapter.

Application of Research Within the Bank

4.02 As the research program has come of age, there are increasing demands for research applications from the operational departments. Such applications usually involve the use of innovative or more sophisticated techniques (whether developed in the Bank or elsewhere) to the problems encountered in country economic, sector or project work.

4.03 The external research budget does not presently provide for such research applications, and there are no explicit budgetary and staff allotments for this purpose in DPS and CPS or in the Regional Offices. The problem is therefore, one of promoting the application of new analytical techniques in the Bank. While it is not our purpose to make detailed recommendations on organizational questions, we wish to outline some possibilities.

4.04 In the first place, if researchers in the CPS and the DPS are to spend time on applications and dissemination, more staff must be made available for this purpose. The work involves different degrees of specialization. For example the construction of industrial programming or of agricultural sector models calls for highly specialized skills. It would be worthwhile to establish a special unit for assisting in work of this kind, using the services of say three persons who gave it all their time.

4.05 Apart from such highly specialized modelling, most of the work to be done by way of application and dissemination can be done by CPS and DPS professionals, if enough time is made available. We are told that the current pent-up demand for help with research applications from operational units would support up to six additional manyears, and we recommend that the CPS and DPS staff be increased by some such number for this purpose. We locate this staff in the CPS and DPS rather than in the Regional Offices for two

reasons. First because we intend that many more than six persons should be involved in applications and dissemination, thus making available a wider range of expertise than would result from giving each region an applications officer of its own. And second because we consider that in general applications should be a part-time function of persons who are also engaged in research, since this will help to maintain the quality and relevance of both research and applications.

4.06 At the same time we sense the need for a closer link between regional staff and research. The Steering Groups that we have recommended in Chapter III would contribute to this, but a focus of administrative responsibility is also needed in the regions. This could be provided by stressing that the Bank looks to the Chief Economist in each region to give leadership in the articulation of regional research needs and in the application of research results in country economic, project and sector work; and by providing him with additional staff for this purpose. For the immediate future we would suggest appointing one senior economist, with wide interests and some research experience, in the office of each Chief Economist, to concern himself primarily with the promotion of research and the application and dissemination of research results. This number could be increased as the work expands.

Dissemination and Training in the Bank

4.07 We have made repeated references to the importance of dissemination. While there are several existing channels for research dissemination, we feel that an increased dissemination effort is required to ensure that country, sector and project work at the Bank takes full account of research results, including important research developments occurring outside the Bank. At the same time, dissemination and training are intimately related since it is through exposure to research results and techniques and their absorption that the intellectual capital of operational staff can be maintained and upgraded.

4.08 One additional channel for the dissemination of research results is the preparation of brief reports on the findings of individual research projects. Such reports should include the results of 'field trials' or other examples of applications which show the relevance of the results for the operational and policy work of the Bank. Their preparation should be an integral part of the research project.

4.09 More generally, state-of-art papers should be prepared to review research areas that are of particular interest to operational staff. The reviews should survey research done both inside and outside the Bank and should focus on establishing generalizations that are relevant for the Bank's work, indicating how the research results and techniques may find operational applications. We understand that such a program of papers is about to commence, and we endorse this development.

4.10 Research reports and state-of-art papers are likely to remain unread in the absence of face-to-face communication between operational and research staff. Such communication could occur in seminars and workshops, for which research reports and state-of-arts papers become an input. Seminars may be organized on a particular subject on a regional, sectoral or Bank-wide basis, as appropriate.

4.11 Apart from seminars and workshops, there would be need for more informal individual contact between operational and research staff to discuss research-related problems arising from operations. All these avenues would sensitize operational staff to research ideas and results and would make research staff aware of operational issues. A further spur to assimilation of research by operating staff would be provided if researchers participated in reviewing the output of operational economists in their field. We would encourage the use of researchers' time for such review, and feel that in the long run this dialogue would create a more receptive climate for research amongst the operational staff.

4.12 Dissemination and training objectives would also be served by extending the rotation of Bank staff, expanding the scope of sabbaticals, and instituting advanced training programs. As regards the last, the Bank could develop courses for its own staff of the kind designed by the Economic Development Institute (EDI).

4.13 An increased dissemination and training effort would require modifying existing priorities in the Bank. For operational staff, this would mean setting aside time and resources for such activities on a regular basis. For research staff, participation in dissemination and training efforts, including informal contacts, should be made an integral part of their work program.

Research Application and Dissemination Outside the Bank

4.14 The standard vehicles for the dissemination of research results outside the Bank are the Staff Working Paper Series, Reprint Series, and the Bank's Research Publications. These are sent to a large number of research institutes in the developing countries, but far from all. We suggest extending the distribution of these documents to all research institutions of some standing in the developing countries as well as to institutions working on development problems in the developed countries. A wider distribution of the Bank's Catalog of Publications would further help the results of Bank research reach interested scholars in developing countries.

4.15 It is particularly desirable that research results reach the officials and consultants in LDCs who are designing projects into which the results could be incorporated. More frequent arrangements should be made for authors to visit such countries and hold seminars on their results as has already been done in a few cases. To the extent possible, research results should be introduced into courses at the EDI. The Bank's resident offices should also play a role in ensuring proper linkages with the local research community.

4.16 Finally in some cases where Bank research has produced new techniques that have aroused wide interest (such as programming methods in large scale industry, effective protection analysis, project evaluation techniques, or the use of social accounting matrices) the Bank may be asked by member countries to send staff to train local staff in the new technique. The research budget makes no provision for this; nor do departmental budgets. The particular staff members who have worked on the research project are not likely to be available for much work of this kind. In some cases it might be possible to use consultants conversant with the research in question. For this purpose, the Bank could pay the cost, not out of the research budget but as a part of its general budget.

Chapter V

Size and Priorities

5.01 In this chapter we address the question of how large the Bank's program for social science research should be, and of what areas of social science research should be given priority.

Size

5.02 We begin with the observation that the development process in most of its aspects is still insufficiently understood, and that social science research can help substantially to increase the world's knowledge about obstacles to development and how to overcome them. Considering the urgency of dealing more effectively with the stubborn problems of poverty, hunger, unemployment, and ill-health in the world's developing countries, we consider that more research aimed at these issues as well as at the development process in general is strongly warranted.

5.03 How much should the Bank do? There are other individuals and organizations that are able to contribute to the needed research. But at the same time, the Bank has the ability to select its own research objectives with flexibility, and the Bank is able to mobilize the best resources, wherever they may be found, to conduct the research it undertakes. There is thus a *prima facie* case for additional Bank-financed social science research on development: the need for such research is urgent and the Bank is well equipped to undertake it. Before reaching a firm conclusion, however, we have asked several further questions.

5.04 First is the test of performance: has the Bank's research been of high quality? This question was addressed by the Specialized Research Advisory Panels, on whose judgement we mainly rely. They were reviewing the results of over one hundred studies, so they found a mixture of good and bad. One can only ask how this mixture compares with results in other research institutions. The answer is that the quality of Bank research ranks as high as that of any other development research institution in the world. In a number of fields the Bank is the leader, and in all its work is highly respected.

5.05 Second is the test of utility: has the Bank's research been useful? In part, the answer is clear. The results of Bank research are plainly contributing to the Bank's analytical work, and that of member countries, in such fields as trade policy and sector analyses. There are also some good examples of the use of research results in project design: the Bank's projects in transportation and in public utilities would not be the same but for the results of Bank research. In some other sectors (education, population, small scale industry) however, research is still in the early stages and its results are only beginning to be seen. It remains true that the Bank's comparative advantage in project-related research has not been fully exploited, but we have proposed measures to remedy this.

5.06 Third is the test of feasibility: can the Bank manage well an increased amount of research? We have expressed some doubts about the current administrative arrangements, which produce too much fragmentation, but in our view these defects are easily remediable. The program is not in sight of becoming too large to manage. On the contrary, in some fields of study the number of Bank staff engaged in research may be below the critical minimum. We have also expressed concern that not enough attention is being given to disseminating research results both within the Bank and in developing countries. We do recommend that high priority be given to the measures for improving research application and dissemination outlined in Chapter IV, but we do not believe that the resources needed for this purpose preclude an increase in research activity.

5.07 In summary, then, we find the Bank's research record to be good in terms of quality and usefulness, and an increase in Bank research seems both desirable and feasible. By how much should Bank research be increased?

5.08 First note that we have already made some recommendations which are intended to be put into effect even if the program stays at its present size, and which have a substantial cost: (a) our recommendations for increased collaboration with LDC research institutions and for other ways of building LDC research capacity might cost $750,000 a year in the first instance; (b) better management of the Bank's own data and retrieval systems might cost $150,000; (c) additional staff for dissemination and application of research results within the Bank might come to 15 manyears (9 attached to the research departments, six in the regions) or $1,800,000; (d) additional staff for dissemination and applications outside the Bank would require say three manyears or $350,000; (e) travel and other expenses associated with (c) and (d) might add another $500,000 to the external research budget. These figures add up to $3,550,000 a year at today's prices. They are intended as orders of magnitude rather than as precise dollar amounts. But this should not obscure the importance we attach to them. In our view the additional functions that they will finance have a higher priority than an increase in the existing research program, since we consider that, having successfully completed a large number of projects, the Bank's next task is to make the arrangements to ensure utilization of results.

5.09 Fortunately it is not necessary to choose between spending more on application and dissemination and more on research because the total research budget is in any case such a small proportion of the income of the Bank. This budget has been held constant in real terms for the past six years or so, and should in our view now be expanded, because of the need to fill gaps in our knowledge of the development process, combined with proven capacity. Having in mind the need for orderly progress, we recommend a rate of expansion of up to ten percent a year in real terms, which cumulates to about 60 percent over 5 years. This ten percent applies to research funded through the Research Committee and departmental budgets (items (i) and (ii) in paragraph 1.05) and would therefore be about $1,030,000 in the first year. Another review like the present one might be commissioned in about five years to make recommendations for the subsequent period.

5.10 Taken together the increases recommended in paras. 5.08 and 5.09 are quite sizable, and apart from their cost to the Bank we have also considered their effect on other development research organizations, which might find it more difficult to recruit professional staff. This should not be much of a problem for institutions in developed countries; as for the developing countries, the program will itself strengthen their research capacity if our recommendations in Chapter II are carried out.

Priorities

5.11 The question of priorities for the selection of research to be financed by the Bank is a difficult one. Precise allocations among types and fields of research, and the choice of particular projects, are matters which the panel considers can only be decided by the Bank, taking into account such factors as the availability of particular skills in the staff and the comparative quality of research proposals. We have three general comments on priorities, however, based on our review.

5.12 First, we have considered how well the Bank's research program accords with our view of the Bank's comparative advantages for conducting research which were outlined in paragraph 2.07. In general, we believe the research projects that have been selected for Bank support stand up well by these standards: they have made good use of the Bank's comparative advantages of scale, international scope, and ability to mobilize high quality talent. We have noted three qualifications which suggest directions for possible improvement:

(i) The Bank's research has not drawn upon the Bank's own extensive experience with development projects as much as it might have.

(ii) There are clearly areas of the Bank's interest—education is an example—in which a somewhat larger and more coherent research effort is needed to achieve a minimum critical size.

(iii) While we endorse the importance of small projects and individual research in appropriate cases (the initial exploration of new areas, for example), we think some Bank research projects have not had sufficient scale and continuity to yield the results that were hoped for.

5.13 Second, we have looked at the fields of research chosen by the Bank and the relative emphasis given them. A convenient although imperfect indication of relative allocations is given in Table 3, which shows both the intended allocations among fields of research for the four years FY75 through FY78 and the actual allocations as they worked out over that period. These allocations are noteworthy for the substantial weight given to such newer fields of Bank emphasis as agriculture and rural development, urbanization, and income distribution, in comparison to such older fields as transportation, public utilities, and growth/comparative economic analysis. The panel considers these allocations desirable, and suggests that over the coming period still further shifts in emphasis may be warranted.

Table 3: External Expenditure FY75 to FY78, Compared to Guidelines
(Percentage of external expenditure, in constant dollars)

Category		Guidelines 1/	Actual
IA.	Growth/Comparative Economic Analysis	11.0	13.4
IB.	Income Distribution	14.0	11.4
II.	International Finance and Trade	4.0	6.4
III.	Agriculture and Rural Development	20.0	17.9
IV.	Industry	10.0	6.5
V.	Transportation	10.0	11.0
VI.	Energy, Water and Telecommunications	5.0	8.1
VII.	Urbanization	10.0	7.8
VIIIA.	Education	5.0	4.6
VIIIB.	Labor and Employment	5.0	7.5
VIIIC.	Population, Nutrition, and Health	5.0	4.7
	Other	1.0	0.7
		100.0	100.0

1/ Guidelines established by the Research Committee in 1975.

5.14 Third, we have reviewed the reports of the specialized panels, whose views of past research and of research priorities in their respective fields are summarized in the Appendix. These reports vary widely in the degree to which they make specific recommendations concerning research objectives and priorities within the fields they studied. They are all, in our view, conscientious and valuable reviews, and several of them make specific suggestions for reallocations of research resources which we commend for careful attention by the Bank. We note, however, that the priorities listed by these panels add up to a much larger program than the resources likely to be available, and we have been asked by the Bank to indicate our own preferences among these topics, as well as topics which have not been reviewed by specialized panels.

5.15 This we do with great reluctance. None of us is familiar with the state of research in the whole of this area. Moreover the choice of a topic depends to some extent on the persons who happen to be available, the data opportunities that may open up, and the changing urgency of issues. The making of a program is essentially a management task, and we would not wish any list of ours to inhibit this function, whether by pushing topics of which the Bank is doubtful or by excluding topics which it might otherwise favor. We have however been urged by the Bank to indicate our preferences, and this we now do.

5.16 Our list does not include topics where the current level of Bank research is adequate; it covers only topics where the research effort should be increased. It also excludes monetary and fiscal policy, which are studied by the Research department of the I.M.F.

1. Rural institutions. Relations between techniques and institutions; land tenure, marketing, credit, the labor market, the division of labor within the family.

2. Incentives in agriculture. Pricing policies, rural taxation, the flow of resources between town and country, the terms of trade between industry and agriculture.

3. Women in economic development. The changing economic roles of women. Absorption of women into the labor force.

4. Comparative costs in production of some major agricultural commodities and minerals. Differences in factor productivity and in the elasticity of supply.

5. Location of plants for processing primary products, as between developed and developing countries.

6. Technical education and the demand for labor. The match between demand and supply in the market for skills.

7. Energy policy in developing countries, with special reference to non-fossil sources of energy.

8. Water use and management; reduction of waste in both urban and rural uses.

9. Development policies and income distribution; changes in the relative positions of different socioeconomic groups; study of the consequences of different policy interventions pursued by countries to alleviate poverty or improve the distribution of income.

10. Labor markets, urban and rural migration. The structure of wages. Unemployment.

11. Low cost delivery systems for education, health and other public services.

12. Small scale industry, urban and rural. Evaluation of policies pursued in these areas since 1950.

13. Appropriate technology. The process of innovation and adaptation in developing countries; incentives for choosing appropriate technology.

14. The policies of public enterprises. Price policy; externalities; profits; labor policy; relationship to wider socioeconomic objectives.

15. Industrial entrepreneurship. Sources of indigenous entrepreneurship; policies designed to promote such enterprise.

16. Changing structure of world trade. Policy implications; inter-LDC trade; access to MDC markets.

17. Development and human fertility. Operational and evaluational study of family planning and related projects.

18. Development policies and nutrition; effects of different policy combinations on nutrition levels.

19. Coordination of various modes of transportation; cost comparisons, price policies; coordination of routes.

20. Geographical polarization of development; measures to spread development more widely and prevent excessive concentration in a few large cities.

5.17 Integration of these or other topics into the Bank's research programme would take some time, as existing research projects are completed, and as additional funds become available over the next five years. Over this period the Steering Committees whose creation we have recommended in Chapter III should be reviewing their fields from time to time and making judgments more closely linked to the immediate needs and opportunities of the Bank.

Appendix

Bank Research: Evaluation and Future Priorities

1. This appendix is based on the findings of the various panels (listed in para 1.02) which have reviewed Bank research in the following areas: Agriculture and Rural Development; Commodities; Education; Energy, Water, and Telecommunications; Income Distribution and Employment; Industrial Development and Trade; Population; and Transportation. Each panel has provided an evaluation of past research and has made recommendations on future research directions. Several of the panels have also indicated the implications of the recommendations for the size of the research program.

2. The Research Advisory Panel on Agriculture and Rural Development (RAPARD) concluded: "The overall impression which emerges from a review of research in the field of ARD [Agriculture and Rural Development] in the Bank is that of a large output of high quality" (p. 5) and that it "has quite likely contributed to the shift in lending policy toward the small farmer and the rural poor" (p. 6). At the same time, RAPARD criticized the lack of a "comprehensive research program" (ibid.) and noted that "the direct impact of ARD research on agricultural lending has been marginal" (p. 21).

3. RAPARD recommended that "the Bank's research support for ARD studies should be expanded considerably—roughly in proportion to the increase in Bank lending for the ARD sector" (p. 21). It further listed a number of new and expanded areas of research, classified under five major headings (pp. 23-24):

(i) Nature and magnitude of emerging tasks: links between demography and ARD; water and energy balance; new production techniques.

(ii) Options in key areas: relationships between techniques and institutions; risk, uncertainty, and finance; food and nutrition; factor shares.

(iii) Making the lending program more effective: taxes, subsidies, and other incentives; rural infrastructure; 'software' aspects of projects; learning from prior lending projects; strategies for reaching the rural poor.

(iv) Methodology for projects.

(v) The process of change: intersectoral models; rural industries, insertion of traditional farmers in the market economy; public utilities.

4. The Advisory Panel on Commodities reviewed Bank operational work on commodities, including commodity forecasts, commodity studies, and papers on lending policy, as well as the few research projects the Bank has financed in the commodities area. The Panel concluded that "the forecasting methods used compare favorably with those in use for comparable purposes in other organizations" (p. 2) adding that "continuous improvement of the commodity forecasting should be based more on acquiring a deeper understanding of markets and their functioning than on further sophistication and formal modelling" (ibid.).

5. In turn, "the commodity research conducted under Research Committee auspices, though highly professional and sophisticated, has not yet made any significant contribution to the operational and the policy roles of the Bank, and the direction of the work still underway is hardly more promising in this respect" (p. 3). The Panel recommended undertaking four research projects:

(i) The linking of cyclical and trend elements in forecasting the behavior of commodity markets.

(ii) The treatment of inflation in price forecasting.

(iii) The study of comparative costs and productivity in production of selected agricultural and mineral commodities, starting from available Bank information and research projects, with a view to feeding into Bank commodity policy and international commodity policy.

(iv) The study of factors affecting the location of processing of selected agricultural and mineral commodities, with view to orienting Bank and international development policies.

"It is estimated that the above research projects, which should be monitored by the proposed Bank commodity steering group, would require one additional staff research coordinator in the Commodities Division, plus some $70,000 of external funds per year for five years" (p. 5).

6. The External Advisory Panel on Education noted that "much of the research done is of high quality and quite useful, but for an organization committing several hundred million dollars a year to the expansion and improvement of education and training, the scale is clearly inadequate" (p. 28). The Panel proposed "a steady increase over several years in the scale of Bank-supported research on education and training, and the development of a stronger research strategy" (p. 29). The Panel suggested that research "should be aimed directly at important issues faced by the Bank and borrowers in pursuing major educational and training objectives, and it should reflect the comparative advantage of the Bank in doing research: for example, cases in which the Bank can conduct comparative study of activities in several countries, or cases in which the Bank has access to data not generally available" (p. 29).

7. The Research Review Panel on Energy, Water and Telecommunications (EWT) stated that its "overall view about EWT research is a favorable one" (p. 6); it commended the researchers for their "ability to focus on the aspects most relevant for Bank purposes" (ibid.); and it further noted that "a number of examples testify to the infiltration of economic analysis into Bank lending to public utility organizations" (ibid.). The EWT panel proposed a wide-ranging program, including the following subjects, some of which involve practical applications (pp. 9-10):

(i) Energy--general: LDC energy outlook, energy and development; energy demand management and conservation; non-conventional energy.

(ii) Energy--oil and gas: natural gas issues; inter-fuel substitution.

(iii) Energy--electric power: standards of rural electrification; power pricing seminars; power energy pricing; autogeneration, cogeneration, technology monitoring, methods of power supply to remote areas; energy transmission costs.

(iv) Water supply and sewerage: technology dissemination; follow-up research on technology; resource recovery; multi-city and multi-purpose projects.

(v) Telecommunications: nature and characteristics of telephone usage; pricing policy.

(vi) Multisector studies: alternative financing of infrastructure; asset revaluation; appropriate institutions for decentralized services.

8. For purposes of carrying out the research program, the panel suggested an immediate increase in staff from 2.5 manyears to 6.5 manyears and of consultants from 6.5 to 9.5 manyears. This increase is supported by the statement, "we feel that the research activities of the department have been understaffed in the past, its previous record shows that it is very capable of turning out very worthwhile research, and that there is a whole string of important projects awaiting investigation in the near future" (p. 16). The recommendations also aim at increasing the staff-consultant ratio in EWT research (p. 15).

9. The Research Advisory Panel on Income Distribution and Employment (RAPIDE) noted that the "Bank program of research upon income distribution emerged in a self-conscious, planned fashion and the Bank has been prominent in stimulating and pursuing research on income distribution." (Appendix B, p. 1). It further concluded: "this review makes apparent both the high quality and varied character of Bank income distribution research. It has been technically proficient and directed to a wide range of problems...

The productivity of Bank staff compared with academic environments in which research is a large and regular component of responsibility is quite high [and] the deficiencies of the research have related less to its execution than to the erosion of policy orientation and focus as it has been conducted" (Appendix B, p. 8). Finally, "the initial Bellagio conference and subsequent publication of Redistribution with Growth has meant a leadership role that has not been characteristic of all fields of Bank research" (Appendix B, p. 1). RAPIDE identified "four priorities for future income distribution research. These include research upon the characteristics of socio-economic groups; study of the consequences of different policy interventions pursued by countries to alleviate poverty or improve the distribution of income; specific analysis of the effectiveness and implications of a basic needs approach to the problem of inequality; and comparative case studies of the relationship between national strategies of development and evolution of the distribution of income" (Main Report, p. 12).

10. In regard to research on employment, RAPIDE noted that "an evident feature is the generally high quality of the output ... and many of the authors have been acknowledged as experts in the field ... On the other hand, the relationship between the subject matter of research and the operational needs of the Bank has not always been evident" (Appendix C, p. 1).

11. The panel's priorities for research on employment are stated as being closely related to its priorities for income distribution research. "They may be grouped under three categories: careful study of developing country labor markets; analysis of the characteristics of the unemployed; and research on direct and indirect employment creation resulting from different kinds of projects. These generally conform to the emphasis of the Employment and Rural Development Division" (Main Report, pp. 14-15).

12. RAPIDE did not explicitly deal with the question of the size of the research program in income distribution and employment. As regards research on employment, it suggested the need "better to concentrate the efforts of the small number of staff in the [Employment and Rural Development] Division, and better to coordinate the research underway in other divisions" (p. 17). However, its recommendations on research in income distribution may be assumed to imply an increase in staff. Finally, RAPIDE proposed undertaking a large-scale data collection effort in income distribution and employment that cannot but be rather costly.

13. The report of the Research Advisory Panel on Industrial Development and Trade stated: "by and large, we are impressed by the overall high quality of Bank research on industry and trade in economic development" (p. 24), adding that "it is our impression that a number of different strands of Bank research have influenced, directly and indirectly, Bank operations" (p. 26). The panel further noted "that there are certain lines of research at the Bank which in the past have been forceful and productive, but which now are running into diminishing returns" (p. 29) and suggested that, in most of them, an application and dissemination phase should follow. It further proposed new lines of inquiry, together with the extension of some recent

work. The Panel's recommendations concern topics related to the trade area, to internal policies and institutions, and to innovation, entrepreneurship, and technological change (pp. 22-26).

14. In the trade area, the Panel recommended the expansion of Bank research on export promotion policies and market access and on changing international trade patterns, with special focus on inter-LDC trade. It also recommended undertaking research on "industrial growth paths, trade patterns, and policies to support them for countries that are rich in natural resources, those which are just beginning their industrialization, and those which are extremely poor and have not yet started in developing manufacturing activities" (p. 32).

15. Furthermore, it was suggested that the Bank "shift the focus from the economic effects of tariff policies to more general consideration of how policies and institutions influence resource allocation and efficiency within a country" (p. 32), with particular attention given to the effects of government policies on labor and capital markets as well as to the effects of price controls and other interventions. In addition, the Panel proposed expanding research on small scale industry and public enterprises.

16. It is further stated in the report: "We recommend that the Bank explicitly and consciously do research on mechanisms of technological transfer, adaptation of technology to better fit local economic conditions, innovation in industry in less developed countries, and the policies and institutions that support and stimulate technological progressivity" (p. 33). Having listed a number of related topics, it is added that an "examination of a set of issues relating to entrepreneurship strikes the Panel as particularly important" (p. 34).

17. As regards the size of the research program, the report concluded: "even though we have suggested that some research areas now are mature for application, that others should perhaps be phased out ... and finally that other types of research should not be 'moved into' it is obvious that our suggestions would require a somewhat larger research budget in the field of industry and trade..... Against this background, it is not unreasonable to increase the number of scholars at the Bank in this field with at least a handful (approximately five), highly competent persons" (p. 35). This number presumably does not include the staff needed for research applications in the industry and trade area.

18. According to the External Advisory Panel on Population, "the Bank's research work has been good as far as it goes but it has not gone far enough. On the whole, it is solid work, but it has been small and fragmented. The Bank's research effort needs to be changed in both magnitude and context: more substantial and more focused" (p. 30). And, again, "the research support for population is disproportionately low—especially in comparison with the Bank's disbursements to the Consultative Group for International Agricultural Research averaging $2.8 million annually [sic] in the past four years. If that sort of funding can be made available for that important

field, the Panel considers that the Bank should be able to do better by this important field" (ibid.). The Panel further recommended that "the Bank's research program should deal primarily with (i) population growth and socio-economic development; (ii) operational and evaluational study of family planning and related projects; (iii) policy possibilities beyond family planning; and (iv) demographic data systems in fertility trends and behavior, in collaboration with other agencies" (p. 31).

19. While noting that "the transport research program under review has few signs of actually being a 'program'" (p. 6), the Transport Research Review Panel concluded that "the hallmark of this research has been the bridging of the gap between theoretical models and what reality can supply in the form of empirically established relationships" (pp. 6-9). Furthermore, "the overall accomplishment has indeed been impressive, clearly placing the Bank and its collaborators in a preeminent position among the world's leading research centers on problems of transportation in developing countries" (p. 7).

20. As to the future, the Panel suggested that, in the case of research projects undertaken so far, the "emphasis should clearly shift from the research activity proper to wider application and dissemination of results" (p. 7). As regards new directions, the Panel suggested the following areas of research and research applications (p. 30):

(i) Highways: highway safety problems; road user charges; bus/trucking regulation/organization; miscellaneous technical guidelines.

(ii) Construction industry: study selected countries; review paper: construction and industrial development.

(iii) Ports and shipping: ocean shipping potential; port management problems; ports and regional development; coastal shipping potential.

(iv) Railways: comparative costs of rail/other modes; practice in service reductions/closures; electrification vs. dieselization; equipment maintenance/utilization; railway information systems; miscellaneous technical topics.

(v) Roads: Yemen Arab Republic feeder roads: Phase II; Ethiopia feeder roads: Phase II; rural transport needs; domestic regional aviation.

(vi) Multi-modal and other: process of deregulation; spatial implications of transport projects; social transfers by transport pricing; freight transport chain; risk analysis updating.

(vii) Contribution to rural infrastructure research program.

21. The Panel added that, "the recommended program of transport research implies an increase over three years of three Bank staff (from the current approximate seven man-years per year) plus one or two additional transport staff in support of the proposed broader study on socio-economic impact of rural development projects" (p. 32). This increase in staff time, and the proposed increase of consultant time by one-half (p. 30), includes work on research applications as well.

Annex I: Membership of Specialized Advisory Panels and List of Panel Reports

(a) Panel Members 1/

1. Research Advisory Panel on Agriculture and Rural Development:

Vijay Shankar Vyas, Chairman	Director Indian Institute of Management Vastrapur, Ahmedabad, India
James Boles	University of California Berkeley, California, U.S.A.
Jean Marc Boussard	Chargé de Recherches Institut National de la Recherche Agronomique Paris, France
Gelia T. Castillo	Professor of Rural Sociology University of Philippines Los Baños, Philippines
Lucio G. Reca	Buenos Aires, Argentina

2. Advisory Panel on Commodities:

Ali Ahmad Attiga	Secretary General Organization of Arab Petroleum Exporting Countries Kuwait
Alan Brown	University of Oxford England
Eric M. Ojala	Senior Research Fellow Massey University Palmerston North New Zealand
Affonso Celso Pastore	Minister of Finance State of São Paulo, Brazil

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1/ Affiliation at time of panel membership.

3. Advisory Panel on Education:

David E. Bell, Chairman	Executive Vice President The Ford Foundation New York, N.Y., U.S.A.
Mary Jean Bowman	Professor Emeritus University of Chicago Chicago, Illinois, U.S.A.
Paulo José Dutra de Castro	General Director of Industrial Relations Volkswagen do Brazil
Louis Emmerij	Rector Institute of Social Studies The Hague, Netherlands
Lameck K.H. Goma	Minister of Education Republic of Zambia
Sippanondha Ketudat	Secretary General National Education Commission Thailand
G. L. Monekosso	Director University Center for Health Sciences University of Yaoundé Cameroon
D. P. Singh	Vice-Chancellor Rajendra Agricultural University Bihar, India

4. Research Review Panel on Energy, Water and Telecommunications:

M. Boiteux, Chairman	President Electricité de France
Rómulo Furtado	Secretary General Ministry of Communications Brazil
Lambert Konan	Director General Energia Electrique de la Côte d'Ivoire, Ivory Coast
A. R. Prest	Professor of Economics London School of Economics England
A. K. Roy	Sanitary Engineer World Health Organization New Delhi, India

5. Research Advisory Panel on Income Distribution and Employment:

Albert Fishlow, Chairman	Professor of Economics and Director of the Concilium on International and Area Studies Yale University New Haven, Connecticut, U.S.A.
Simon Kuznets	Professor Emeritus of Economics Harvard University Cambridge, Massachusetts, U.S.A.
W. Arthur Lewis	Professor of Political Economy Princeton University Princeton, New Jersey, U.S.A.
Justinian Rweyemamu	Personal Economic Advisor to President Nyerere, Tanzania (on leave to the Independent Commission on International Development)
Gerardo Sicat	Director-General of the National Economic Development Authority Philippines
Leopoldo Solís	Sub-Director General Bank of Mexico

6. Research Advisory Panel on Industrial Development and Trade:

Assar Lindbeck, Chairman	Professor of International Economics University of Stockholm, Sweden
Edmar L. Bacha	Professor of Economics Pontificia Universidade Católica Rio de Janeiro, Brazil
Gerardo M. Bueno	Mexican Ambassador to the EEC Brussels, Belgium
Juergen B. Donges	Professor of Economics The Kiel Institute of World Economics, Kiel, Federal Republic of Germany
Jae-Ik Kim	Director-General Bureau of Economic Planning Seoul, Republic of Korea

(Continued)

Richard B. Nelson	Professor of Economics Yale University New Haven, Connecticut, U.S.A.
Kirit Parikh	Professor of Economics Indian Statistical Institute New Delhi, India

7. Advisory Panel on Population:

Bernard Berelson, Chairman	President Emeritus and Senior Fellow The Population Council New York, New York, U.S.A.
Ronald Freeman	Professor of Sociology Population Studies Center University of Michigan Ann Arbor, Michigan, U.S.A.
Goran Ohlin	Professor of Economics University of Uppsala, Sweden
Frederick T. Sai	Assistant Secretary-General International Planned Parenthood Federation London, England
Chandra Sekhar	Secretary Ministry of Health and Family Planning Government of India

8. Transport Research Review Panel:

Jorge Cauas, Chairman	Former Chilean Ambassador to the United States; also former Minister of Finance and Governor of the Central Bank. Previously Director of the Economic Institute at the Catholic University of Chile
Dag Björnland	Director of the Institute of Transport Economics, Oslo, Norway
Rodolfo Félix-Valdez	Sub-Secretary of the Ministry of Human Settlements and Public Works, Mexico

(Continued)

Goon Kok Loon	Secretary of Administration of the Port of Singapore Authority
S. Jagannathan	Former Financial Commissioner of the Indian Railways; also former Secretary, Ministry of Finance (Department of Economic Affairs); former Executive Director of the World Bank and Governor of the Reserve Bank of India
Daniel L'Huillier	Professor of Economics and Director of the Center for Transport Economics Research at the University of Aix-en-Provence

(Continued)

(b) List of Panel Reports

1. Report of the Research Advisory Panel on Agriculture and Rural Development, May 1979

2. Report of the Advisory Panel on Education, October 31, 1978

3. Report of the Advisory Panel on Commodities, May 1979

4. Report of the Research Review Panel on Energy, Water and Telecommunications, May 10, 1979

5. Report of the Research Advisory Panel on Income Distribution and Employment, May 1978

6. Report of the Research Advisory Panel on Industrial Development and Trade, May 1, 1979

7. Report of the Advisory Panel on Population, August 1976

8. Report of the Transport Research Review Panel, April 1979

REPORT OF THE

RESEARCH ADVISORY PANEL

ON

AGRICULTURE AND RURAL DEVELOPMENT

May 1979

TO THE WORLD BANK

FINAL REPORT OF THE EXTERNAL ADVISORY PANEL

ON AGRICULTURE AND RURAL DEVELOPMENT

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May 1979

CONTENT

I. Introduction

1.1 The Bank's lending in Agriculture and Rural Development (ARD) is a substantial activity - not only in its own right, but also for the overall impact that it can make on the development strategy of the borrowing countries through its direct and induced effects. Looking to the magnitude and complexity of the task, lending decisions in these areas need to have proper support from well designed research. As it was succinctly stated in a review document, "The Bank needs knowledge to formulate intelligent policy and research can supply that knowledge." The Bank has assembled a well qualified group of researchers that, in their respective Departments and Divisions, have acquired an excellent reputation in professional and, to an extent, operational circles. Currently, the activities of these various research groups within the Bank are being assessed.

1.2 The Research Advisory Panel on Agricultural and Rural Development (RAPARD), one of several similar Panels each reporting on a specialized area of research, has been charged with the task of evaluating the research program of the World Bank in its specialized field. While each of the Panel Reports will have intrinsic interest and value, they will also provide a partial basis for the work of the General Research Advisory Panel.

1.3 Without presuming to know completely the rationale that led to the formation of these Panels at this time, this Panel (RAPARD) interpreted its task broadly. Is the research in this field relevant to the operational needs of the Bank as well as the economic policy requirements of member countries? Is it of high quality? Does it cover all or most of the important researchable questions? Is the research mix appropriate? Are the comparative advantages of the Bank fully utilized? Is the scope for collaboration with other research institutions properly exploited? Are the research results adequately disseminated and used? To what extent should the research program be charged with the additional task of assisting the development of research institutions in developing countries?

1.4 These, at least, are some of the questions we have considered. We recognize, however, that the ability of this Panel to respond in a meaningful way to all of these questions is limited. One obvious limitation is that we have, for the most part, reviewed only "externally" funded research projects; projects funded at least in part from research funds administered by the Research Committee, though we are aware that there are other research activities not so funded. We are also limited by our consideration of only those research projects identified as Agricultural and Rural Development, even though other projects exist which address issues also relevant to Agriculture and Rural Development. Additionally, we have had direct access to only one class of potential research users, namely members of the various Regional divisions of the Bank which administer the actual lending operations. Finally, although Panel members bring to its deliberations a variety of administrative and research experiences, the Panel has no direct information on the usefulness of the Bank's research product to member countries, and especially to borrowing countries. Despite these limitations, this Panel has taken its charge seriously and will exercise its best judgement in responding to the set of questions outlined above.

II. Overview of Research in Agricultural and Rural Development

2.1 To obtain some indication of the magnitude of our evaluation problem, during the past six years there have been some 26 "external" research projects classified as ARD. Annual research expenditures on this set of projects ranged from US$225,000 to US$504,000, approximately 15% of "external" research expenditures. During this period annual "external" research expenditure has been approximately constant in real dollars even though Bank lending for Agriculture and Rural Development projects has approximately doubled, from US$957 million to US$3.2 billion. This represents an increase in lending for Agriculture and Rural Development from 12% to about one-third of the total Bank lending.

2.2 Research efforts in the Bank in the area of Agricultural and Rural Development (ARD) have, for the purpose of this review, been broadly classified as research on: (1) Project Design and Management; (2) Sectoral Policy Issues; and (3) Developmental Policy Issues. With the recent emphasis on Rural Poverty, the projects in this area can be treated as a special and separate category.

1. Studies on Project Design and Management

2.3 These studies aid in improving the current modes of designing operational lending projects. Two distinct approaches are discernable:

(i) Quantitative analysis and modelling - The objective of these studies is to develop a formal framework for delineating interrelationships between various elements of a project and evaluating the direct and induced effects of operational lending projects; the latter, generally, are discussed in terms of income generation and income distribution under different institutional settings. A good example of such research is the Indus Basin Irrigation Project which is basically an attempt to formalize the design and management of a large scale irrigation project. Analysis of rural change falls also into this category. Several "internal" projects are operated by the DRC in the same vein. Using extensively quantitative and econometric procedures, these studies are generally of high professional quality despite their obvious reliance on comparative statics at the expense of the dynamic dimension. At the same time, they are not always well received in the operational departments of the Bank, precisely because of their degree of sophistication which may limit routine use. Nevertheless, it is quite natural that the methods for tomorrow do not fit with the commonly used routines of today. The shadow pricing system itself was not better received a decade ago than, for instance, the Social Accounting Matrix of today. One problem with this type of investigation is avoiding too large a commitment in unfruitful directions, a difficulty so far avoided.

ii) Non-quantitative, comparative studies of various rural development projects; The prototype of this kind of research project is the African Rural Development Study. Several other projects such as the Study of Lilongwe Land Development Program, the Agricultural Innovation and Rural Development Study, the Study on Management and Operation of Irrigation Projects also belong to the same line of investigations. Such studies can be described as in-depth, ex-post, comparative analyses of agricultural and rural development projects. Some of these projects particularly the African Rural Development Study, resulted in outstanding pieces of analysis, and are generally appreciated by the personnel in the operational departments.

Some of these projects approach the subject in a non-conventional way. For example, the study on Agricultural Innovation and Rural Development rejects the conventional micro-economic framework for analyzing technological changes as being too restrictive and argues for a more complete systems framework.

2. Sectoral Policy Studies

2.4 Research in this category aims at an analysis of important problems at the national level. The projects in this area include two kinds of studies:

(i) Several studies of key policy variables belong to this category. The main research objective of these projects was to increase knowledge of the institutional and economic variables affecting development process. These include the studies of Smallholder Agriculture in Yugoslavia, Land Reform in Latin America and the country case studies in Agricultural Process and Subsidies. The report on Land Reforms in Latin America is one of the more balanced pieces have been of value to users within the Bank, is a systematic effort to identify particular types of agricultural projects suited to the conditions emerging in each country after land reform. The study on Agricultural Prices and Subsidies shows, basically, the same strengths and weaknesses as the Land Reform Project. However, in the latter case the time overrun was less important. The study on Yugoslavia was impeded by the difficulty of finding competent researchers sufficiently interested in the project and by difficulties in obtaining data.

(ii) National Agricultural Models: The objective of this type of research is to construct and validate sectoral models. An outstanding example of such a study is the CHAC model which was followed by similar models for Brazil and Portugal. The project on Agricultural Pricing and Storage Policy in East Africa was also attempted along the same lines. Emphasis of the researchers is on methodological issues;

i.e., how to build easy-to-construct, adaptive, and versatile models of the agricultural sector of a given economy. These studies gave rise to a great number of publications, and contributed to the reputation of the Bank within the scientific community. At the same time, they could be used for operational purposes. For these reasons, they are generally considered to be successful within the Bank. Nevertheless (and without diminishing their merits), it must be pointed out that they are examples of comparative statics. The absence of a time dimension not only deprives the user of important information, but also casts doubt on the validity of their results. In view of the importance of changes over time adding a dynamic dimension would have significantly modified the models and perhaps the conclusions drawn from them.

Similarly, a major shortcoming of many of these models is the fact that the links between the agricultural sector and the rest of the economy have been largely neglected, except through prices and quantity requirements.

More generally, these models are extremely difficult to test, therefore, one is not sure whether their predictions will be realized. For this reason extreme care needs to be exercised in interpreting their results.

3. Development Policy Issues

2.5 These studies aim at deepening our understanding of the developmental process by selecting some key determinants. Many of these studies are located in more than one country. Examples of such studies are provided by projects such as Land Reform in Latin America; Farm Mechanization in India and Pakistan; Rural and Urban Public Works; Raising Productivity on Small Farms; Price Intervention in the Agricultural Sector; Research and Productivity Changes.

2.6 Some of these projects, i.e., Farm Mechanization in India and Pakistan, and Rural and Urban Public Works, producted good results from an operational angle. On the other hand, few others, e.g., Raising Productivity on Small Farms could not serve its original purpose (although it produced a computer program which was not without value at the time it was written).

2.7 Finally, two other projects deserve a special mention. Price Interventions in Agriculture is concerned with the new distributive impact of subsidies and taxes on products and factors in the agricultural sector. It deals with an unquestionably important topic and was carried out by a consultant, within the time and resources originally committed to the project. However, the project apparently suffered from lack of supervision, the consequence of which is a lengthy final version, inappropriate for Bank use at the operational level. This is particularly painful because, as indicated above, the subject matter was adequately covered from a conceptual point of view.

2.8 The project on Agricultural Research and Productivity Changes deals with a basic question pertaining to agricultural development, and is a good example of a "successful project". Several reasons help to explain the good results: a clear specification of the topic and the choice of two highly qualified scientists to develop the subject. The study also provides some insights for research organization with the Bank: it is a good example of collaborative research as well as an indication that scale and quality of research are not necessarily correlated.

4. Poverty Issues

2.9 Although this topic may be considered as a special case of the preceeding category, it needs a separate mention owing to its importance in the Bank's lending program and the large number of research projects in this area. The latter include projects such as Agricultural Development, Rural Income and Poverty, Food Deficits in Target Groups, Distribution of Income Through the Extended Family System, Structure of Rural Employment, Income and Labor Market and Rural Savings and Investment Behavior. These projects are mainly concerned with the functioning of economic institutions in low income rural societies.

2.10 Similarly, the Bank has been increasingly involved in the area of Food and Nutrition research in the recent past, which, given the magnitude and importance of the subject is a proper field of research activity in the Bank. Several aspects of the work on these issues are worth mentioning:

(i) carefully designed studies which make excellent use of far-from-ideal data coupled with rigorous economic analysis; (ii) the methodological contributions to the analysis of probems of food and nutrition, as for example, in the case of the study on Food Deficits in Target Groups; (iii) the positive interaction among different research units in the Bank (for example DED and CPS); (iv) the production of research results which give insights in the area of direct relevance to the operational side of the Bank.

5. Summation

2.11 The overall impression which emerges from a review of research in the field of ARD in the Bank is that of a large output of high quality. These research projects have yielded a large number of interesting and excellent research publications. At least 20 research papers based on these projects were published in top level professional journals or were brought out as full-length monographs. Due recognition should also be given to the contribution of the external consultants in achieving these satisfactory results. It can be safely concluded that on purely professional grounds, the overall performance of researchers in the Bank is very satisfactory.

2.12 This is not to say that the research efforts are not capable of improvement. The collection of research projects in Agricultural and Rural

Development does not appear to constitute a comprehensive research program. There is evidence that in some cases inadequate attention has been given to other relevant research, not only from other research organizations but also from the Bank itself. One further gap is the relative absence of studies that delve into the rich lending experience of the Bank to derive a better understanding of the development process, although the problems of handling large amounts of disparate data relating to such experience is clearly a difficult and time consuming task.

2.13 Mention has been made of the reliance on comparative statics and the consequential absence of dynamic elements in the models. The Bank staff is aware, not only of this omission, but also of the difficulty and complexity of adding the time dimension in appropriate ways.

2.14 Reference has been made to problems encountered in research administration, particularly in respect to the use of outside consultants and research organizations, and their adequate supervision and coordination. Given the limited resources available, these problems suggest an explanation, if not a justification, for the virtual absence of efforts to build research capacity in the developing countries.

2.15 Nevertheless, the Bank's research has quite likely contributed to the shift in lending policy toward the small farmer and the rural poor; it has entered the debates on development issues besides providing some macro-policy indications and evidences of direct and indirect project outcomes. Small advances on the methodological front have likewise been achieved and a few more observations about the development process added. Moreover, there is promise of operational guidance from certain studies. In general, it is not that the Bank's research is poor or that it is not useful, but that it is not enough to address the multitude and magnitude of problems encountered in the lending program, nor the knowledge needed to make rural development feasible in diverse situations.

2.16 Moreover, despite the claims and counter-claims within the Bank, it cannot be truthfully said that the Bank's research has no operational significance, for research can exert influence in inconspicuous ways. In giving advice, assistance and comment and when participating in country economic missions, sector work, and project related activities, DPS and CPS most probably rely on their insights from research to carry them through. The influence is, therefore, likely to be more circuitous than straightforward and perhaps never in its particular research purity but in combination with previous knowledge and partly with speculative judgment.

2.17 In this context, however, it is notable that (as suggested in para 2.1) the resources devoted to "external" research in ARD is relatively small. While the Research Committee nominally allocates some 20% of its annual budget to ARD research, in practice only 11 to 13% is used. The total staff time allocated to ARD research may be as low as 6 manyears per year. One reason why the Research Committee funds are not used may be related to the limited in-house capability to plan and supervise their use. More importantly, when spread over 6 regions, 25 operational projects dimisions,

and some 120 developing member countries, the 26 ARD studies completed over the past 6 years looks very thin. It is perhaps not surprising that operational staff see few research results of direct relevance to their own work or to the countries with which they are concerned.

2.18 Clearly, producing good, publishable research is one thing. Coping with the many and specific needs of the Bank, fully exploiting its comparative advantages, and properly disseminating the results of research is another. The Bank's research efforts have to be evaluated in terms of their fulfilling these tasks. We will now address these questions.

III. RESEARCH NEEDS AND STRATEGY

3.1 The research program in the Bank should be directly basically to fulfill three tasks: (1) having a good general understanding of the developmental process; (2) having reliable tools for designing and appraising development projects; and (3) being able to take a position in national policy issues whenever the Bank is involved. A significant amount of research in the Bank is conducted with these purposes in view, although is is not always clear as to which one of the objectives listed above is sought to be fulfilled, and sometimes the studies made may have "fallen between stools".

3.2 The Bank will derive obvious advantages if the research is more consciously focused on major issues and subject areas. At present the impression given is that of highly diffused research efforts without a clear focus. In an institution whose main task is to promote research, such multiplicity is permissible, probably desirable. But in an institution like the Bank, research efforts should have an operationally relevant focus and be somewhat more structured. Such structuring does not mean a lack of innovative work in different areas; it only pleads for periodic efforts to develop major themes that will serve to guide the research program.

3.3 A more purposeful research program has the following important elements. First, a conscious effort is necessary to identify the next set of problems that developing countries are likely to face. At present international organizations as well as national governments merely react to the problems. The Bank is not an exception to this rule. A group in the Bank with the assistance of a few knowledgeable outsiders may be able to identify the set of problems that are likely to acquire serious dimensions in, say, the next decade. This will enable the Bank as well as the member countries to be better prepared to cope with these problems. Such a group may be constituted every 3 to 5 years, to assist in identifying the emerging tasks. This group will not indulge in merely a speculative exercise. It will review the current situation, identify emerging trends and assess member governments' capabilities in coping with the problems in key areas such as population, food, non-renewable resources, technologies, and institutional factors. It should identify the nature and variety of tasks, as well as existing and emerging bottlenecks. Fortunately, a lot of work on emerging tasks and bottlenecks is being done by specialized institutions. The Bank's researchers should bring the major findings of these studies together and relate these to its principal activities. With the back drop of such studies the Bank will be able to develop major strategies for research and lending.

3.4 Second, in the context of these emerging problems in the developing world, the main thrust of Bank research should be to identify and spell out alternative strategies and policy options, keeping in view the socio-economic milieu in different countries. Obviously, such options will change over a period of time. However, without bringing in policy options in different sets of circumstances, developmental efforts are likely to remain partial and ad hoc.

3.5 The third group of research activities, which should be the kingpin of the research program in the Bank, pertains to those providing "guidelines for lending strategies". The projects in this category should aim at: a) an understanding of intersectoral and intra-sector linkages, and structural and systematic problems within the sector; b) improving behavioral aspects of research design, which at present are treated inadequately; and c) a focus on organizational and administrative issues related to project implementation.

3.6 It is not unusual to find that the social and physical infrastructure created at high cost do not serve the rural poor. This is due, inter alia, to various administrative and institutional "leakages", an aspect on which very little systematic work is done. In brief, the main purpose of this type of research is to find out how a given amount of loan or credit can be made more effective in serving the stated objectives. To an institution like the Bank such research is of immediate importance and can promise high payoff.

3.7 The fourth category of research pertains to project level research i.e., procedures and processes for identification, formulation and evaluation of lending projects. Various stages in the project cycle are carefully identified in the Bank, but the research support for undertaking tasks associated with different stages of a project is not yet firm. Systematization of knowledge in this field will also be of help to the member governments in their own economic operations.

3.8 Finally, a large developmental institution like the Bank should contribute to the understanding of the process of social and economic growth. Development is a complex phenomenon, it does not proceed along neat lines or in regular stages. The interplay of various social and economic institutions, the nature and limitations of public intervention, and the response of institutions to intervention need careful study. Equally important in this regard is understanding individual motivation and incentives. Without an understanding of the process of change, development will continue to be viewed in a mechanistic way and effectiveness in influencing its course will be marginal. For example, though there is a recognition that economic development influences population growth, there is little knowledge as to which element of development and in what manner it influences, say, fertility behavior.

3.9 To recapitulate, the Bank's need is to have well researched ideas and views on: (i) the nature and magnitude of emerging tasks in agriculture and rural development; (ii) various options available in key areas of ARD; (iii) possibilities of making lending more effective; (iv) methodologies for identifying and formulating bankable projects; and (v) understanding the process of change in order to draw useful inferences for policy options.

3.10 Not all of this research needs to be in-house. There are significant opportunities for collaborating with other research bodies. Before we give our views on the nature of such collaboration, it will be advisable to examine comparative advantages, and limitations, that the Bank has in conducting the type of research which can maximize the Bank's contributions to ARD.

IV. COMPARATIVE ADVANTAGES AND DISADVANTAGES

4.1 It is at least a debatable point that the World Bank has an absolute advantage in the field of economic research in Agricultural and Rural Development when compared to individual universities and specialized research institutions. Even if this point were to be conceded, there is still the matter of comparative advantage. To the fullest extent possible, the Bank should utilize the data and research products generated by all of the public and private institutions working in the field of economic development. Furthermore, in developing and managing a research program of its own, the Bank should consider carefully its comparative advantages and disadvantages in deciding how to proceed.

4.2 Some of the Bank's comparative advantages are:

(i) There exists in the Bank a substantial number of competent researchers, not only in the field of ARD but also in the general field of economic development. The various formal and informal communication networks existing within the Bank serve to identify problems, appraise proposed solutions, and disseminate research results. By definition, development is an integrated process. From this perspective, the position of a researcher in ARD in an institution which copes with the whole developmental process is indeed an advantageous one.

(ii) Over a period of time the Bank has developed relations with a large number of countries. It can easily set up an information network or mobilize expertise from diverse sources. Numerous contacts between the Bank, member governments and local institutions facilitate access to data and specialized knowledge. The researchers in the Bank can cope with diverse conditions by adopting a comparative approach which may expand the number of cases for analysis and insights. This is possible due to the operational involvement of the Bank in a number of countries in the same region.

(iii) The Bank has a veritable gold mine of development experience waiting to be analyzed, refined and shared. Where else can one find 363 lending projects approved during the five-year period 1974-78 with 210 of them defined as rural development projects in which more than half of the direct benefits are expected to accrue to the rural poor.

(iv) Finally, relative to most other institutions, the Bank is more capable of allocating adequate funds to important and well conceived research projects. This permits the further possibility of hiring external consultants in order to benefit from the association of outstanding specialists in a given field. It also permits effective collaboration with local institutions having access to reliable data and a stronger feel for the local scene.

4.3 There are, at the same time, several comparative disadvantages:

(i) The organizational culture of the Bank is understandably dominated by the lending project syndrome, since this is the major function of the Bank. Those who are not involved in lending activities may feel a degree of alienation. The demand for usable and timely results may place undue pressure on the researchers.

(ii) By the very nature of the Bank's relationships to the governments and institutions of the borrowing countries and the role which its projects play in these countries, the Bank is vulnerable to their political and socio-cultural sensitivities. Research results carry their own hazards.

(iii) Finally, and perhaps of greatest importance, the Bank is based in Washington while agricultural and rural development is taking place in the hinterlands of the borrowing countries. Although there is virtue in maintaining objectivity and distance from the phenomena being studied, there is a risk that Washington may be too distant to allow the intuitive and practical insights which determine the operational relevance of research findings.

4.4 These comparative advantages and disadvantages of the Bank, as well as the ultimate limitation of staff numbers should be clearly in mind when deciding whether a research task should be completed in-house or shared with outside consultants or developed in collaboration with other research organizations.

V. COLLABORATION WITH OUTSIDE RESEARCH ORGANIZATIONS

5.1 In a sense, collaboration of the Bank with outside research organizations is both implicit and explicit. It is implicit in that it should be aware of and use, where appropriate, research results obtained and published by other organizations in the field of agricultural and rural development. It is explicit when, in an effort to develop the most cost-effective and efficient research program, comparative advantages and disadvantages of the Bank relative to outside research organizations result in direct collaboration.

5.2 There is, however, another set of reasoning that would justify collaboration even at the potential risk of loss of timeliness, research quality and cost effectiveness. In its own long-term interest, the Bank should assist in the development of an institutional capacity for research particularly in the borrowing countries. Presumably, the Bank is or should be responding primarily to lending project proposals originating in the developing countries. To the extent that the quality of these proposals can be improved over time, the processes of review, approval, disbursement, monitoring and evaluation by the Bank staff would be greatly simplified. In the most recent Annual Report concern is also expressed that disbursement in certain major sectors, particularly agriculture, will be somewhat lower than expected. Reference is made to the fact that "new style" projects are technically complex and that new agencies and institutions are needed to discover ways to benefit low-income groups of people previously not reached by most governmental programs. To ensure that institution building to improve indigenous capability gets the attention it deserves, it should be included as a major element in the set of criteria used in evaluating research (as well as other) projects.

5.3 While each specific project will require particular attention to the issue of collaboration, it is possible to identify a number of general subjects that are likely to be largely in-house and others that are likely to prove more effective in a collaborative setting.

5.4 The Bank is well equipped to prepare sectoral and economy-wide models to test alternative policy options. The work is complex, time consuming and requires large resources. At the same time it is necessary that a special effort be made to simplify the techniques for easier replication of such exercises, and to assist in dissemination and training both of Bank staff and also of outside research organizations.

5.5 The projects on lending strategies have again to be essentially in-house research, although on some of the aspects, particularly on the behavioral and organizational dimensions, collaboration with others may be desirable. It is also possible to farm out some research in this category to institutions in developed countries but particularly also to those in developing countries where a suitable capability to conduct such research exists or might easily be developed.

5.6 Another category of research, aimed at lending project identification, formulation, monitoring and evaluation, should be basically an in-house

activity. Here one should particularly emphasize the effort to review and analyze the rich resource of project experience in order better to understand the process of agricultural and rural development.

5.7 A core program of research in all these areas should be located in the Bank yet in each of these areas, barring the research on lending strategy and on project related methodology, the Bank should encourage involvement of outside expert groups to a significant extent. There are several aspects of research under each of these categories where collaboration with other knowledgeable individuals or specialized institutions would be useful.

5.8 There are at least four situations where a project might be executed outside the Bank. First, if highly specialized technical knowledge is needed, the Bank might not prove to be a suitable location. For example, in the area of ARD, there are several problems which can be better researched in the International Agricultural Research Centers than in the Bank. Since the Bank has taken a leading role in the establishment and development of these centers, and since most of them have excelled in their chosen fields, close collaboration between these institutions and the Bank is both feasible and desirable. Second, the projects necessitating a large amount of primary data collection can be profitably located in the institutions of the country concerned. Because of its geographical location, its high cost operations and the given style of functioning, the Bank is not the right agency for large scale data collection in the developing countries. With proper planning and monitoring, these tasks can easily be entrusted to the local institutions. Third, when the research needs a thorough and intimate knowledge of the local situation in a country, i.e., in case studies, the Bank has hardly any comparative advantage. The institutions located closer to the field can do this task more efficiently. Finally, the Bank may also support local institutions, particularly those with competence in behavioral, organizational, and sociological areas, even when only a part of the research effort is directly operational.

5.9 It is clear from the above that a variety of arrangements will have to be made in order to make satisfactory use of the collaborative arrangements. Carelessness in this regard can lead to failures and frustrations, as has already been pointed out. On the other hand, with the support of the Bank, and more importantly, the involvement of its staff in the research projects located in different centers, the Bank can obtain high quality results and in the process strengthen local research capabilities. There cannot be a uniform type of collaborative arrangement with outside research groups. Much depends on the strength of the collaborative organization and the nature of the project. The arrangements can range from contracting with local instituitons to collect primary data to making research grants in areas which might be of mutual interest and benefit. In between come arrangements such as visits of external collaborators as consultants, placement of the Bank's staff in local institutions and joint project teams named by the Bank staff and the collaborative institution.

VI. RESEARCH ORGANIZATION

6.1 Most of the "external" research in Agricultural and Rural Development is performed in the Development Research Center, the Development Economics Department, and the Agricultural and Rural Development Department. The first two units are in the Development Policy Staff (DPS) and are very much research oriented, while the last unit is one of the departments of the Central Projects Staff (CPS) and has other important functions in addition to research. Research in the DPS has been typically longer term and methodological and discipline oriented, while the research performed in the CPS has been typically intermediate term and more immediately "problem solving", with perhaps more emphasis on operational issues. External research is funded and projects reviewed and approved by a Research Committee that has jurisdiction over both CPS and DPS research projects seeking external funding.

6.2 The existing Research Committee is in a sense an apex organization, but by its composition, nature of functioning, and largely, due to the mandate given to it, the Research Committee concentrates mainly on the screening of projects which are proposed for funding by different units in the Bank. Its role as a priority setting, task allocating, monitoring and interacting body is conspicuous by its absence. What is needed, therefore, is not a new committee or group but a redefinition of the functions of the present Research Committee along the following lines:

(i) Reviewing periodically the research needs of the Bank, in collaboration with outside experts and with research and operational staff.

(ii) Identifying and defining major themes to be addressed in the research program.

(iii) Exercising initiative in the generation, development and implementation of research projects on the defined themes.

(iv) Monitoring and periodically reporting on both ongoing and completed research.

(v) Assisting in the task of building research capacity in the developing countries.

(vi) Reviewing and approving research proposals submitted by the panels.

In view of these redefined and expanded functions, the Research Committee should be designated as the Research Council. The Council will be able to perform these functions only if it has a regular staff, working exclusively for it, to assume routine administrative functions.

6.3 The Council should also be assisted by standing Research Panels to screen and strengthen research proposals prior to final review and decision by the Council. These Panels, organized on the basis of subject matter interest and competence, would replace the current ad hoc Panels organized for each proposed project. They would also monitor on-going research projects in their respective areas and should also facilitate communication between researchers and users, both within and outside the Bank. In order to ensure that "new blood" is brought into the process, which is an important advantage of the ad hoc Panels, a small number of ad hoc members should be added to the Panel for the review of each research proposal.

6.4 While the reorganization of the Research Committee and the redefinition of its research function will accomplish a great deal, there is also the need to delineate more clearly the categories of research activities for which the various units of the Bank are likely to have comparative advantage. Any clarification of the territorial jurisdiction of different units is likely to arouse passion. Despite this danger, and in view of the five fold classification of research activities already developed, the following correspondence is suggested:

Research Area	Unit Primarily Responsible
1) Emerging Tasks and Research Themes	Research Council
2) Alternative Strategies and Policy Options in ARD; Methodological Issues in Project Design	DRC
3) Issues Related to Bank Policy on ARD, Strategies, Behavioral and Organizational Aspects	AGR
4) Processes of Change and Intersectoral Relations	DRC/DED
5) Impact Studies, Intercountry Comparisons, Project Related Research	Regions

6.5 These suggested principal areas of responsibility do not have well defined boundaries and, in any case, should not be regarded as mutually exclusive. The rationale for this division is provided by the existing "division of labor" as well as the nature and functions of the various units. It needs to be reiterated that not all the research needs to be in-house.

6.6 The emerging tasks in ARD have to be identified and transformed into major themes for the research program (with due regard for the contributions and findings of other institutions). In our view this can be best performed by the Research Council with its several subject-matter Panels.

6.7 An examination of policy alternatives in the area of ARD requires a comprehensive and systematic look at the whole economy. Various types of modelling will be an aid to such a discussion. With its available competence and the traditions already established, DRC is the proper location for this type of work. The methodological issues in project design could be tackled by the DRC in close collaboration with operational departments. The existing practice of inviting outside experts as consultants is a welcome device and can be better utilized once the main thrust of research in the DRC is programmed.

6.8 Guidelines for lending strategies and project design involve issues which are of direct relevance to the Bank's operation, and therefore should be considered the king-pin of the research program. AGR, in close touch with operations and, at the same time, having key staff functions, has a comparative advantage for this research area.

6.9 The aspects of agricultural and rural development that can be better understood in an intersectoral context (urban-rural linkages) and which may also involve in-depth study of the development process may be better pursued by DED. Here also there are opportunities for collaborative research with other institutions.

6.10 Research on issues of immediate need and operational relevance should be addressed more actively by the operational departments concerned. They should be provided with resources to permit them to review, interpret and execute studies of project impact and program strategies, as well as those involving inter-country comparisons within their Regions. The purpose here should be to facilitate the additional work that would make the overall research program more relevant to operational concerns.

6.11 An organization provides only a formal structure. If it is to achieve its objectives, certain procedures should be followed. Procedures which are relevant for research objectives include:

(i) development and phasing of research projects;
(ii) research collaboration with outside agencies;
(iii) monitoring and evaluation of on-going research.

6.12 The initiative for research projects can come from a variety of sources, both within and outside the Bank. Regardless of its source all research will be more meaningful if the sponsoring units are clear about the particular clientele, either inside or outside the Bank, who will benefit from the research product.

6.13 The research initiative of the staff in the operating departments is dampened because the scrutiny to which research proposals are subjected is excessive and dilatory. In these circumstances it is suggested that the operational departments should be encouraged to develop acceptable proposals jointly with, or with help from, DRC, DED, AGR or consultants.

6.14 In the previous section, we have outlined the scope for collaborative research. There are, however, certain pre-conditions for the success of any such arrangement. These are: (i) there should be a mutual respect and trust, (ii) the institution should already have some competence in the area where the Bank's interests lie, (iii) the research design is clear and explicit and there is a provision for periodical consultation and monitoring and, (iv) both institutions are assured of some mutual gain from the collaborative activities.

6.15 There are, however, some situations where even when these conditions are fulfilled, collaboration may be neither possible nor desirable. If the research problem is considered "sensitive" by the host government, e.g., problems of land reform in some countries, the local institutions will be seriously handicapped in pursuing research. The credibility of the local institution vis-a-vis the member government is also an important factor to be considered, and this may vary depending on the nature of the problem.

6.16 Most research projects require a multi-stage phasing of development and implementation which makes it possible to assess intermediate output from each stage before proceeding to the next. For example, available information from past research, practice and theory may be reviewed and tentative hypotheses proposed. Either with available data, or following collection of new data, the hypothesis is tested. This in turn may lead a reformulation and retesting or to a successful conclusion.

6.17 As in the case of initiating new projects, the Research Council and its subject-matter Panels will have a major role in monitoring the successive phases of on-going research, at present one of the weakest links in the research process.

VII. DISSEMINATION OF RESEARCH RESULTS

7.1 Even if the research program generates results that are of high quality, are relevant, cover the field, and are potentially usable, there still remains the problem of dissemination and utilization.

7.2 It is easy to say that good research will be its own salesman and that no special effort is needed. If this were true, nothing further would need to be said. As evidenced by publications in referred journals, books, and working papers, there is a large quantity of high-quality research from the Bank available to each of the several groups of potential users. These publications have been supplemented by seminars and workshops both in-house and in the relevant geographical regions.

7.3 This problem of dissemination and utilization of research results is not unique to the Bank. Particularly in Agriculture and Rural Development, the problem has been approached by extension programs of various types. Even within the Bank, some additional effort is needed. For the potential user in the developing countries, an even greater effort is required. From the producer-user dialogues, two general views were expressed:

(i) that operational needs of the Regions are not being addressed by the Bank's research;

(ii) that important policy papers and research reports are not being read by operational people within the Bank, and by developing country officials outside.

7.4 This occurs despite the fact that the research product is considered as "interesting", even by those people who complain that they do not benefit from it. In fact, the research program is interesting because it addresses questions that they, themselves, have addressed. It is not of much help because they cannot usually read the research reports at the time they need it. Research reports are always either forgotten or forthcoming. Besides, the language of the researchers seems often tedious and too abstract.

7.5 Difficulties of this kind are not specific to the research in the Bank. They are encountered in any research service associated with an operational institution. They are painful, but probably unavoidable. Even so, conflicts arising between researchers and operational departments do not seem to occur in the Bank with the same frequency as in many other institutions. To a certain extent, some credit for this must be given to the formal and informal communication links inside the institution, and to the professionalism and relative open-mindness of both research and operational staff members. What can be done, nevertheless, to improve the situation?

1. Inside the Bank

7.6 The first thing to do is to avoid destroying what is already functioning. The dispersion of the research activities within several

departments is certainly regretable from the point of view of research management. It is nevertheless a very efficient way to take account of a wide spectrum of needs and should be maintained. Similarly, the facilities and opportunities which already exist for a researcher to participate in an operational mission, and for a member of the operational staff to stay for a while in a research unit should be encouraged and expanded. Obviously, the great majority of the operational staff members will never be interested in research, and some researchers will never be attracted by operational tasks. But even if only a few professionals can be exchanged between the two sides, this will facilitate personal acquaintances and communication.

7.7 The documentation within the Bank could probably be improved by a computerized document retrieval system, which would allow, for instance, somebody having tractorization problems in the Ivory Coast to know about other studies on similar questions done, for example, in Pakistan and in India, without painful bibliographical investigation. Such a system gives researchers access to unpublished documents and data from the project files. To some extent this would compensate for the difficulty of setting up a full data bank in agricultural and rural development. Such a documentation system, limited to documents internal to the Bank, would probably not have an undue cost, and could significantly improve the informal communication system, by enabling all staff members to identify colleagues sharing the same interest in a particular field.

7.8 Finally, as noted earlier, it must be pointed out that communication between research staff and operational staff members of the Bank is not necessarily direct. Indirect ways of communication may involve, for instance, the identification of a problem by a researcher belonging to the Bank's staff, its solution found outside the Bank, and the application of the solution being done by the operational staff on the basis of information provided by a third source. In such cases, the Bank's research will never be credited for the results, although it may have played a very significant role in their attainment. This must not be forgotten when judging research efficiency. From this point of view, information flowing from the Bank's researchers to the whole scientific community are just as essential as connections inside the Bank. The best way of ensuring communication in that respect is through the normal channels of research result dissemination, i.e., professional papers, participation in scientific meeting and congresses. This is in effect one of the better ways of maximizing the multiplier effect of the Bank's research expenditures. At the same time, this implies that a continuous flow of information proceeds from the operational staff to the research staff. In practice, this is perhaps the most difficult thing to achieve, for the operational staff is (probably rightly) convinced that a new problem submitted to research will never be solved in due time. Thus, nobody in the operational staff is tempted to lose time discussing with the researchers. It is therefore essential to create communication patterns which would enable the researchers to forecast the needs likely to appear within the operational staff. Again, informal links are probably more efficient in this respect than formal ones.

2. Outside the Bank

7.9 A major obstacle to research dissemination outside the Bank, in the developing countries, is the lack of cooperating institutions capable of interacting with the Bank in identifying problems, translating them into research projects, in contributing to them and receiving research results from them that can then be adopted or adapted in the particular context of each country.

7.10 Consequently, major effort -- even at the potential expense of some reduction in research quality and cost effectivenss -- should be made to incorporate some element of institution building in virtually every research project. Particularly where implementation implies a time-frame longer than the research project, and even longer than the disbursement period, institutions must be developed to guarantee continuous and effective work. In the design and review of research and lending projects as currently practiced, an additional criteria should be used, namely the project's contribution toward the building of research and analytical capacity in the country being studied.

7.11 Possible actions to achieve this objective include the following:

(i) increased participation of research staff in EDI activities;

(ii) the identification of a cooperating institution, wherever possible, for each project processed through the Research Council;

(iii) the involvement of researchers from the Bank staff in lending operations aimed at creating research institutions; and

(iv) the sending of some researchers of the Bank staff on sabbatical leave for one or two years in LDC universities.

VIII. SUMMING UP

8.1 The above review has indicated that we have found the Bank's ARD research to generally be of high quality, and that it is widely acclaimed in professional circles. At the same time we have identified a number of problems, or potential problems, which it may be useful to enumerate specifically:

i) Despite the major importance of agricultural lending for the Bank, it seems to us that the direct impact of ARD research on this activity has been marginal.

ii) Despite the quality of research projects, it has been difficult for us to perceive any internal logic or clear focus to the ARD research portfolio.

iii) Despite a number of interesting and stimulating projects, we do not feel that the Bank's ARD research has yet contributed a significant break-through in our understanding of the development process.

iv) Despite individual examples of close cooperation by specific people at the working level, we feel that the current organization of the Bank's research does not bridge the gap between research and operations; it does not close the loop from perceived need, through research to implementation of results.

8.2 Despite these perceived shortcomings, which cannot be fixed by any one simple recommendation, our overall recommendation is that the Bank's research support for ARD studies should be expanded considerably - roughly in proportion to the increase in Bank lending for the ARD sector. This is partly due to our perception that much of the transfer of research results to operational practice is indirect. (As an example, the Nairobi speech and consequent "new style" projects, cannot be traced to any one study, but did reflect, to some extent, the cumulative impact of a number of individual studies and memos, which had the effect of questioning the impact of the previous pattern of Bank lending.) As the Bank's lending for ARD continues to increase while maintaining the slant towards the rural poor, the problem of designing useful projects and suggesting appropriate implementation strategies will become more complex. Without proper research support, it will be very difficult to cope with the emerging tasks in an effective and innovative manner. In this context, the resources available for ARD research in the past seem relatively inadequate.

8.3 Despite the specific concerns mentioned above, the existence of the Bank's research establishment, manned as it is by qualified and sensitive researchers, is a major asset. In view of the increasing needs for well researched ideas in the field of ARD, we sugggest the Bank should strengthen and systematize the related research activities and such to improve

the bi-directional flow of useful information between researchers and research users both within and outside the Bank. It is in this spirit that we have developed the following set of recommendations.

(i) The administration of research in the Bank should be strengthened by modifying the form and function of its existing machinery as follows: (a) a Research Council should replace the present Research Committee with a much wider mandate to strengthen research activities in the Bank; (b) the Council should identify major themes for research in ARD (and other sectors) and the research units in the Bank should give due importance to these themes while planning their activities; (c) the screening and monitoring of projects should be entrusted to the subject matter research panels.

(ii) The organization of research should be improved to take full advantage of available resources. Toward this end: (a) each unit in the Bank entrusted with the task of research in ARD should have a well-defined mission in the context of a total research program; (b) given a mission orientation, each unit should then be encouraged to form research groups across jurisdictional lines in order to gain the critical mass needed for larger studies, and to take advantage of existing staff skills; (c) increased use should be made of joint studies with outside institutions, especially those in developing countries.

(iii) The dissemination and utilization of research results should be given further attention. Toward this end: (a) the operational departments should be given resources to enable them to carry out certain research activities, especially to enable the Regions to make better use of research results; (b) a computerized information storage and retreival system should be considered; (c) specific responsibility should be placed on researchers to develop their output to the stage where it is readily accessible and relevant to operational staff.

(iv) The Bank should make conscious effort to build up research capabilities in its developing member countries through its lending and research programs.

(v) In developing the overall research program for ARD, gaps in research identified earlier in this report should be given priority attention, within the broader agenda suggested in Appendix I.

(vi) In the light of increasing complexities in ARD, the growing magnitude of the lending operations in this area, and the need to develop a critical mass of researchers, Bank support for research in ARD should be expanded considerably.

Appendix I. Research Agenda

According to the previously defined categories of need, possible topics of new or expanded areas of research follow.

I. Nature and Magnitude of Emerging Tasks.

(a) Links between demography and ARD.

The various research projects that we have examined were, for the most part, without any explicit links with demographic studies conducted elsewhere in the Bank or in the scientific community. However, demography, rural and urban balance, labor migration, age and sex composition of the rural household and rural sector, are all of importance to ARD, and warrant expanded efforts.

(b) Water and Energy Balance.

These agricultural inputs should be given particular attention in view of their limited supply and their critical importance.

(c) New production techniques.

The "book of blueprints" is a familiar idea for any production economist. Nevertheless, it is yet a theoretical idea, with little empirical content. Now, in relation to agricultural development, the set of new techniques available is continuously changing, often with dramatic consequences, even for the poor traditional farmers. Research in this area might involve (1) a continuous review of new techniques as they emerge, particularly with close collaboration with the international centers such as IRRI and CIMMYT, and (2) the building of a set of models aiming at forecasting the dynamic consequences of their adoption on income distribution, crop location, and on national as well as international trade patterns. These studies must also incorporate social and behavioral elements.

II. Options in Key Areas.

(a) Relationships between techniques and institutions.

A prototype for this type of research is the study of sharecropping recently done in DRC and DED. It should be expanded toward analogous investigation of other forms of institutions. Especially, socialist economic patterns should be examined from this point of view, in order to assess the conditions under which they are efficient.

(b) Risk, uncertainty and finance. ← Crop insurance?

Outstanding research has been done on the effects of uncertainty on the level of production and the adoption of new techniques. But these studies

have not been related to a complementary aspect, namely finance. In effect, the financial status of a production unit is one of the major determinants of its risk aversion, however the latter is measured. A clarification of this issue would cast important light on a major determinant of loan demand in rural areas and consequently on the adoption of capital intensive techniques.

(c) Food and nutrition.

Nutritional needs for low income populations is obviously an important question. The recent pilot study in AGR should be expanded and its conclusions checked, in order to assess the efficiency of alternative forms of public intervention.

(d) Factor shares.

Technical progress in agriculture often involves three separate effects, an increase in production, a substitution of capital for labor and/or land, and economies of scale. What are the long term relative importance and consequences of these effects? This question is of major importance in evaluating the cost of helping the rural poor.

III. Making the Lending Program More Effective.

For the most part, the Bank's lending program in ARD aims at promoting new techniques. But the rationale for the adoption of these techniques by farmers and people living in rural areas is far from being clear. Research in this area might include:

(a) Incentives of all kinds including both taxes and subsidies.

In relation to household behavior the sociological and anthropological points of view are likely to beome important supplements to the neoclassical economic framework. All approaches require a theoretical basis, to be verified by appropriate surveys and model experiments. These attempts need to be expanded, particularly in a variety of institutional and cultural contexts.

(b) Rural infrastructure.

The improvement of rural infrastructure, including rural roads and water supplies is an important concomitant of development, and may often greatly influence the dynamics of regional development. The impact of such infrastructure needs to be evaluated in a dynamic context.

(c) "Software" aspects of projects.

In most rural areas, the success or failure of a project depends on individual decisions which, in turn, depend on cultural patterns and institutions. So far this problem has often been neglected by the research as well as by the operational staff. Sociological and anthropological considerations would be of assistance.

(d) Learning from prior lending projects.

A primary research priority for the Bank is to learn from its own experience, to mine the "gold mine" that it has in its past history, not only in an informal way by each staff member, but with serious application of methodological sophistication, conceptual talents, analytical expertise, plus time (lots of time) and resources.

(e) Strategies for reaching the rural poor.

There are two aspects of this, program content and methods of implementation, particularly to minimize the "leakages" that may divert a significant part of the impact away from the targeted population.

IV. Methodology for Projects.

Computer programs for data handling and management have been developed in various services of the Bank in an almost clandestine fashion, without it being recognized as a research problem. Nevertheless, the conceptual difficulties associated with these endeavours are far from being trivial. At the same time, quick procedures for processing the large amount of data used by the various services could improve their efficiency in a very significant way. Moreover, if, in the future, the Bank were to enter into the business of using these data for other purposes than their immediate uses for projects, these problems should be solved. Therefore, it is recommended that some identified research projects in that field should be added to the research project portfolio.

V. The Process of Change.

The major question here is to elaborate a theory of rural development. One cannot imagine a better place than the Bank for the elaboration of this needed theory.

(a) Intersectoral models.

A prototype of these is the Social Accounting Matrix. It should be developed and expanded, especially in a more dynamic direction. Many other sorts of such models could be imagined, at the regional level, casting light on regional development issues, and their effects on production and income distribution.

(b) Rural industries.

Agricultural and Rural Development relies not only on farmers' behaviour, but also on the possibility of developing a significant industrial sector in the rural areas involved in projects. What kinds of industries are suitable in a variety of contexts? What are the obstacles against their birth and survival? Such are some of the questions that a rural industries research program could address itself.

(c) Insertion of traditional farmers in the market economy. This topic should be investigated in the light of the results drawn from the "incentive" topic already referred to.

(d) Public utilities.

The role of public utilities in ARD is self evident. Nevertheless, few studies have attempted a quantitative approach to measure its impact. If successful, such studies would make an invaluable contribution to project appraisal.

REPORT OF THE

RESEARCH ADVISORY PANEL

ON

INDUSTRIAL DEVELOPMENT AND TRADE

May 1, 1979

Table of Contents

Foreword

We hereby submit our Report on World Bank research in the areas of industrial development and trade. Though we are collectively responsible for the entire report, the drafting of the various sections of Appendix I has been undertaken by individual members of the group.

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CHAPTER I

BANK RESEARCH: A GENERAL DISCUSSION

The overall general quality of research in the field of industry and trade in the World Bank is, in our view, very high--compared both to university research and to research activities of non-university organizations, including organizations connected with the UN system. Thus, the basic problem of research within the Bank in this field is usually not the quality but rather the type of research produced by the Bank, in particular when looking ahead, and the use of research within the Bank.

When addressing the issue of Bank research in this field, it is important to remember that "research", as defined by the Bank, is only a small fraction of general analytical work going on in the Bank, and that research on industry and trade is only a small part of the total research effort. Broadly speaking, analytical work of various kinds--including the assessment of economic trends and policies in various countries, sectors and markets--comprises approximately one sixth of the administrative budget of the Bank. About one quarter of this analytical work seems to be formally classified as "research", of which approximately one seventh, covering about 10-11 manyears of research annually, is on industry and trade.

It is useful to start the discussion in the report with some general principles that may be applied when planning research activities within the Bank. In particular, we shall take up the issues of the reasons for Bank research and the audience for the research. We shall thereafter make some suggestions as to how the efficiency of various research activities of the Bank - import, production, application and dissemination of research - may be improved. In the final section of the chapter, the implications for Bank research of alternative strategies for economic development are considered and applied to research on industrialization and trade. Against the background of the discussion in this chapter we shall, in Chapter II, give a general review and evaluation of Bank research on industrialization and trade, as well as some recommendations for future research priorities of the Bank in this field. More detailed discussion on these issues are presented in Appendix I, where the previous and present research of the Bank on industry and trade are classified into six main areas.

A. Why and for whom?

An important point of departure when assessing Bank research is why the Bank is, and should be, engaged in research, and who the audience for Bank research is supposed to be. A hint on these issues is provided by a formulation in our terms of reference according to which research objectives of the Bank include the task "to support all aspects of Bank operations ..." and "to broaden our understanding of the development process". According to these statements, the audience for Bank research should be both Bank representatives

who are responsible for general policy issues and staff members who are engaged in more narrowly defined Bank operations. It is obvious, however, that Bank research may also be of great relevance for people outside the Bank. In fact, the Bank has regarded it as a duty to stimulate research in the less developed countries as well as to produce research knowledge for people outside the Bank, including the community of scholars around the world in the field of development economics. In addition, research may be stimulated in these countries when the Bank fulfills its advisary role.

But before looking into the implications for the research policy of the Bank of the need to support Bank operations, and to stimulate research in the less developed countries, it is interesting to explore the implications for Bank research of its comparative advantage as a research unit, ignoring for the moment the issue of for whom Bank research is supposed to be performed. It would seem that this approach to the issue of research priorities would follow from a research philosophy according to which the Bank, in the most efficient way possible, tries to contribute to the accumulation of research knowledge in the world as a whole. Thus, the Bank would be regarded as a producer of "public goods" in the form of scholarly knowledge in the field of development economics. The choice of research priorities of the Bank would then be determined solely by the Bank's ability to produce research, and not by its internal demand for (use of) research knowledge.

By applying the notion of comparative advantage we could conceive of a ranking list of research areas in terms of falling relative advantage of the Bank as a producer of research - a list to be cut off at the point where the research budget of the Bank is used up. In other words, for what kinds of research is the Bank, given its lending and policy advising activities, a particularly good location when the Bank is looked upon solely as a producer of research?

On the basis of this approach, the following aspects of Bank research seem to be particularly relevant:

(a) The research of the Bank should concentrate on fields where particularly competent researchers are available within the Bank, or (in a longer perspective) can be hired by the Bank. Thus, the skills currently available amongst Bank research staff should perhaps be regarded as a short-term constraint rather than as a factor that determines the long-term comparative advantage of the Bank.

(b) Bank research should exploit the skills and information that are acquired within the Bank in its operational activities as lender and adviser.

(c) The Bank should concentrate on large projects and projects where a continuity of research effort is important.

(d) The Bank should exploit its information and understanding of facts and problems in a large number of countries to make comparative studies of national economies.

(e) The Bank should try to provide statistical data and other information, where such information is more readily (cheaply) available to the Bank than to other organizations. (It is a somewhat controversial issue whether this should be called "research".)

Aspect (a) would imply that the Bank continue with roughly the same kind of research as it has successfully pursued so far (assuming that leading researchers within the Bank are not likely to leave), but also that it moves into areas that are suitable for highly competent researchers who can be hired by the Bank. In some cases, of course, senior researchers of the Bank may be willing to change their research fields. The choice of the Bank's research topics will probably always reflect the interests and background of dominating researchers in the Bank. This has certainly been the case in its previous research activities, as illustrated by the research on trade policy incentives, with emphasis on effective protection and domestic resource costs (the field of Bela Balassa), growth patterns and sources of growth (the field of Hollis Chenery), and investment programming and the range of technical choice (the fields of Ardy Stoutjesdijk and Larry Westphal). In fact, a good forecast could have been made some years ago about research priorities of the Bank revealed in the recent past simply by looking at the background of the economists who had been hired by the Bank! But it is equally true that the interests of researchers have evolved in response to their experience in the Bank.

By contrast, the Bank has not been very successful in contracting outside scholars and in doing research in circumstances where there has not been strong leadership exerted by researchers within the Bank. It would seem that projects dominated by outsiders have usually not succeeded as well as those dominated by leading researchers permanently employed by the Bank. Perhaps open competition when choosing outside researchers could, in some cases, improve the quality of such research. In this connection we would also recommend that the Bank avoids getting into a straightjacket by subjecting the appointment of researchers to nationality criteria. The experience of other international organizations points to the dangers of such practices.

Aspect (b) suggests research in fields where knowledge is acquired (rather automatically) by the Bank in connection with its operational and policy advisary activities. In fact, the Bank offers to researchers the incentive to work in fields close to policy formulation and implementation, which makes it a natural location for policy-oriented research. One example is research on investment project evaluation. Studies of investment projects that are relevant for several countries at the same time may be particularly suitable for the Bank, as compared to other (usually national) organizations. Another example of a field where knowledge useful for research activities is acquired by Bank operations is probably the study of government policies and regulations. A third, related example are studies of incentive systems and the consequences of alternative institutional arrangements in markets for products, services, credit and labor - and the consequences of these circumstances for rates and patterns of economic development.

Considerable research capacity has in fact already been built up within the Bank in several of these areas. The research efforts on investment programming and trade policy incentives have already been mentioned. However, a potential for research on institutional and policy-oriented problems has also been acquired by way of various research efforts of the Bank in a large number of countries with different institutional arrangement and policies. A modest start in studies of this kind has also been made in sector reviews and studies of small-scale enterprises, state enterprises and financial intermediaries. Ongoing analytical work within the Bank on trends and problems in international markets and in national economies could also be a foundation for more systematic research efforts within the Bank.

Aspect (c) would suggest a heavy emphasis on large projects and research activities where updating and follow-up research is important.

Aspect (d) is a recommendation concerning the mode of carrying out research rather than about a research area (or problem area). The fact that both the research activities of the Bank and its operations refer to a great number of different countries is a strong reason why the Bank should have a comparative advantage in comparative studies.

Aspect (e), finally, might suggest increased efforts by the Bank to collect, process and publish data that come out naturally from the operational activities of the Bank. However, to make this activity a main task of the Bank - i.e. to turn the Bank into a predominant data bank in the development field - would require truly enormous resources. We know how great such a task is even on a national scale. To do the same thing for about 100 countries would therefore require a formidable effort. It may thus be reasonable to limit demands on the Bank in this field to taking greater responsibility for the data it collects for use in its own research and surveys, and to making these data to a large extent available to outsiders. It may be noted that this is not only an issue about publication of research results, but also about improving and controlling the quality of data, which may very well be regarded as a research task. However, resource constraints make it necessary, we believe, to take a rather selective approach to the collecting, improving, controlling, processing and publication of data.

A rather special reason why the World Bank may have a comparative advantage relative to universities in research activities such as (b)-(d) is that these pursuits, to be efficient, often require a rather strong and permanent research organization, which the social science departments of universities often lack.

A strict adherence to the principle of comparative advantage in the production of research would imply that types of research knowledge that are not effectively produced by the Bank - regardless of how important they are for Bank operations - should be imported. It is obvious, however, that such a comparative advantage approach is not a sufficient criterion upon which to base Bank research strategies.

Firstly, the need (demand) for scholarly knowledge in Bank operations cannot always be satisfied by importing research results (in fields where the Bank has not a comparative advantage in the production of research knowledge). One reason is the specificity of required knowledge, another is the lack of interest outside the Bank in certain types of research knowledge that the Bank needs (demands). Thus, in order to satisfy its own requirements for research knowledge, the Bank certainly has to carry out research that is particularly useful for Bank operational needs, and that is not done elsewhere. In other words, the Bank has also to perform a role as residual supplier of research in some fields.

What would be the concrete consequences of following this principle rather than the principle of comparative advantage in production? In the light of the roles of the Bank as lender, investor and policy adviser - and considering the often highly distorted relative prices in many LDCs - the heavy emphasis on studies by the Bank of effective protection and domestic resource costs could certainly be defended on the basis of the principle of residual supplier of research. In fact, it would seem that these studies have been regarded as particularly useful by operations people, though some complaint emerges to the effect that the techniques may have been somewhat "overelaborated" for the purpose of Bank operations.

Another implication of the principle of residual supplier of research is probably to justify research concerning large investment projects (in particular where the Bank is involved as a lender), projects with considerable externalities (such as learning by doing), returns to scale and/or great linkages between sectors. The research within the Bank on investment planning and programming is an example of this type of research for which an important point of contact seems to be the Industrial Projects Department (IDP). It would appear, however, that the operational staff of the Bank, particularly in the regional offices, have not yet made a thorough assessment of the usefulness of this work; nor have they undertaken to manage its application in an efficient way. In fact, it would seem that they usually do not believe that research done in this field is very helpful.

However, we would argue that an adherence to the principle of the residual supplier of research also motivates studies concerning problems in which Bank research so far has not been strongly involved, but which in our judgement reflect severe bottlenecks for economic development in many LDCs. This brings us back to an earlier recommendation that was motivated by the comparative advantage principle, i.e. research on the consequences of (i) government policies and regulations, (ii) institutional arrangements and (iii) incentive regimes not only in product markets (where the Bank has made considerable research efforts) but also in factor markets. The argument for more research efforts of this type would probably be particularly strong should the Bank choose to concentrate research more than before on the least developed countries and on the consequences of industrialization for the least favored group within individual countries.

Thus, the residual supply of research approach and the comparative advantage approach both seem to imply a strong emphasis on studies of the

consequences of alternative institutional arrangements, technologies, government policies and incentive regimes. The reason is that both approaches to research are based on the assumption that research should be related to the fields of Bank operations - either because the Bank acquires special competence in such issues by way of its practical activities or because knowledge is needed in these fields in the lending and advisory work of the Bank (and is not easily imported). A rather special reason why the Bank may be a suitable place for such types of research, relative to universities, is that research on the importance of alternative institutional setups for economic development in the LDCs has so far not become a high priority field in the academic world. There are many relevant types of research on the importance of institutions in the developed countries, such as in the field of industrial organizations, labor economics, and money and banking. Important research on related issues is also pursued in Law Schools (such as research on law and economics), Business Schools (studies of organizations and regulations of enterprises) and Political Science Departments (for instance studies of bureaucracies and budgetary processes). However, in spite of their relevance for economic policy making, these contributions so far have not gained a very high status in the world of academic economics. Moreover, they have only to a very small extent been applied to LDCs. As it may be a long time before they make their way towards application in the LDCs, we believe that the World Bank could help speed up the process by taking a leading position in this research field.

We shall later on (Chapter II and Appendix I) discuss in some detail both why more research of these types is important and how it may be conducted.

A second reason why Bank research cannot be based only on the principle of comparative advantage in the production of research is that there are important "externalities" of having researchers within an organization. For instance, research done within the Bank may be more visible to the operations people than research done elsewhere in the same field. Also, operations people can be made aware of what researchers outside the organization are doing by having a number of in-house researchers (a point to be developed in the next section). More generally, the presence of researchers in an organization such as the Bank helps the development and propagation of new ideas that ultimately affect operations. This important role should be borne in mind in allocating nonresearch time to researchers.

It may also be argued that the general level of sophistication of an organization, for instance in the field of policy advising and economic surveying, is influenced by the general quality of the researchers within the organization. Good researchers help to set performance standards within an organization, which is particularly important in one that, like the Bank, performs the role of policy adviser. Moreover, the reputation of the Bank as an institution of high competence may be boosted by a high research capacity. Such a reputation may help the Bank to hire talented people in general, and hence improve the Bank's operational efficiency.

In other words, the research of the Bank should not only be subordinate to the (relative) efficiency of the Bank as a producer of the research, or to the immediate demands of the operational units of the Bank, but also to long-run considerations of the general competence of the Bank in various respects. In particular, it is important to stimulate the ability of the Bank to translate policy objectives into instruments and actions. An application of this principle suggests that the Bank should build up research competence in many fields where it operates - for the purpose of raising the general level of sophistication within the Bank.

Thirdly, the Bank should also be interested in the externalities of its research on the research potential of the LDCs. The adherence to this principle is an argument for (i) choosing fields of research of interest to scholars and research institutions in LDCs, and (ii) adopting research procedures that facilitate the participation of such scholars and institutions in the research program of the Bank. However, there is a risk that when following these procedures the Bank will not always get the most competent researcher for a specific project. Moreover, the Bank may also be criticized for distorting research in the LDCs ("research imperialism"). To maximize the possibility of LDC researchers' participation while minimizing its associated risks, some of the Panel members are attracted to the idea of instituting a second tier of research sponsorship by the Bank. Rather than first preparing detailed research projects and then seeking LDC consultants to help implement the research in the first stage, the Bank researchers would design only broad terms of reference. These would then be submitted to open international competition, including some preference schemes favoring the participation of LDC scholars and research institutions. These schemes naturally must take into account that the quality of the research is an overriding aim which should not be jeopardized.

Thus, our discussion suggests four different principles for the choice of research topics in the Bank in the field of industry and trade:

(a) a comparative advantage approach;

(b) an ambition to function as a residual supplier of research;

(c) an attempt to create externalities within the Bank in the form of "sophistication" among Bank staff; and

(d) an ambition to help generate research knowledge and research capacity in the LDCs.

In reality, it would appear that all these four principles for research do prevail within the Bank. For instance, past Bank research has certainly been addressed both to the community of scholars outside the Bank and to staff members who are responsible for policy advising and lending within the Bank - in proportions that we cannot really pin down. Controversies over research strategies within the Bank probably often stem just from the different weights that various staff members put on these various principles.

Disappointments over the research activities of the Bank are bound to be felt by those who evaluate the research efforts of the Bank on the basis of one of these principles only.

It would seem that the management of the Bank should clarify, to itself as well as to others, which of these principles (motives), or possibly others, should be emphasized. A similar attempt has, of course, been made by us.

Also the procedure of the research within the Bank should to some extent be influenced by the principles (motives) that govern Bank research. If the Bank is simply trying to make the best possible contribution to the research knowledge of the world, a rather concentrated research portfolio is suggested. It is then also important to allow a very broad freedom for the researchers to choose topics themselves, and hence "to do their own thing". Moreover, the more successful the Bank is in hiring competent scholars, the smaller the need for strictly formal organization and bureaucratic administration of research within the Bank.

On the other hand, if the Bank follows the residual supply of research strategy, it is necessary to see to it, by way of organizing and monitoring research, that the research becomes relevant to the operations in the Bank. A more dispersed research portfolio would then probably follow than by adhering strictly to the principle of comparative advantage in the production of research.

If instead a heavy emphasis is put on the idea of creating high sophistication among the Bank staff in general, an even more dispersed research portfolio would probably follow, perhaps with some risk of not achieving the critical mass of resources that is necessary for a breakthrough on the international research frontier. Thus, this research principle comes into some conflict with the others, in particular with the principle of comparative advantage, because of returns to scale in research.

Finally, if a high priority is given to the ambition to improve the research capacity in the LDCs, participation of researchers from these countries becomes a crucial criterion in the design of research projects. Research would then often have to be organized as joint ventures, with a rather concentrated Bank research portfolio to assure reasonable efficiency.

B. Making the import, production, application and dissemination of research more effective

It may be useful, when discussing the role of research within the Bank, to make a distinction between import, production, application and dissemination of research. It is important that all of these research-related activities are pursued in an efficient way. How that may be achieved is the topic of this section.

Import of research knowledge

The issue of the appropriate use of research within the Bank refers to research knowledge in general rather than only to research knowledge that is produced by the Bank itself. The bulk of research knowledge that is potentially useful within the Bank will always be produced outside it. We therefore suggest that the Bank strengthen its capability to import research knowledge in a systematic way. One of the most efficient ways of doing this is probably to place people with a research background in operational positions in various units within the Bank, so that research knowledge can be imported not only via researchers of the Bank, but also "directly" by the operations staff. This can be achieved in several different ways: people outside the Bank with a research background may be hired to operational positions in the Bank; researchers of the Bank may move over permanently to operational positions; and researchers of the Bank may take operational positions temporarily (for instance one or a few years). Of course, operational ability is a prerequisite for such appointments. In fact, the biggest import of research knowledge probably occurs when someone with a background as researcher is hired by the Bank as researcher or operations officer; knowledge is often most effectively imported "in the heads" of people at the time of their employment rather than by reading research documents or doing research later on.

If the Bank is anxious that its own research is highly relevant for its operations, i.e. if strong emphasis is put on the principle of the residual supply of research, it is important to rely heavily on permanent staff members. Moreover, when relying on outside consultants it may be a good idea not only to use the most outstanding consultants who are available, but also to build up a network of more or less permanently Bank-affiliated outside researchers. These could then acquire some knowledge about the use, and usefulness, of research within the Bank. It also becomes important to choose consultants who are able to communicate with Bank staff members. In order to avoid inbreeding and one-sideness of Bank research this network of consultants should include scholars with different philosophical outlooks, skills and methodological preferences.

Another obvious way of importing research is to commission people - inside or outside the Bank - to prepare state of the arts papers, an issue to be discussed in connection with the problem of dissemination of research (pp. 15-19).

Production of research

The most important aspect of the production of good research is, of course, to have highly competent researchers both within the Bank and as consultants. If the Bank, as we suggest, shifts its emphasis somewhat to new fields of research, it is crucial that the Bank hires outstanding specialists in fields relevant to such research. What this means in concrete terms has to be considered carefully by the Bank. Our view is that the best research is usually done by scholars with a strong theoretical and methodological background in a broad field of research. Suppose for instance that the Bank is going to study problems of incentives and imperfections in factor markets,

or alternative institutions and government policies - and the consequences of these rates and patterns of growth. What is then required is not mainly narrowly defined specialists on policies and institutions in specific markets and countries, but rather outstanding scholars in fields such as industrial organization (including the issue of competition and entry), technical development, financial intermediation, public expenditures and taxation, and labor economics. If the Bank, as we believe, should analyze success and failure stories of various countries, there is also perhaps a case for hiring some scholars with high competence in the analysis of economic systems and modern economic history. Again, there are good reasons for emphasizing the importance of using scholars with some dispersion of philosophical outlook on the issue of economic development.

An important question in these fields is under what general conditions (policies, institutions, systems of incentives and perhaps also cultural characteristics such as the work ethic), entrepreneurship, technological advance and innovations are likely to flourish. It is possible that research in these new fields would sometimes benefit from the application of rather interdisciplinary knowledge and methodology.

However, it is well known that interdisciplinary studies are extremely difficult to pursue successfully. For some research questions something can be gained merely by adding the understanding of a sociologist or a political scientist to that of an economist, but for most interesting questions not much is gained by such a simple addition of viewpoints. To achieve a truly integrated approach to a problem requires a much more intricate intertwining of ideas and knowledge. For a group of interacting scholars to attain the capability of doing research together in an integrated way requires a lot of time learning each others perceptions, and learning how to pose and answer questions in a way that transcends the disciplinary boxes. This is not an endeavor to be initiated with expectations of a quick payoff, and the fact that there are so few examples of successful interdisciplinary research suggests that even with patience the endeavor is highly risky. It is also noteworthy that several of the more successful efforts involved integration from different fields in the head of an individual scholar rather than through the joint thinking of a group of scholars. The important interdisciplinary questions raised above call for an effort at the Bank to broaden the analysis to go beyond conventional economics. But such a broadening should be entered upon cautiously and with patience. Much more is required than merely putting together an interdisciplinary research team.

Application of research

In some measure, Bank research has been applied in operations, with the modality of application depending upon the particular type of research. However, at present the in-house demand for application of Bank research through case studies seems to far exceed the supply. This is partly a reflection of the Bank only recently having established a research program. Most effort to date has been devoted to doing research. It is only as research results have been forthcoming that the demand for application has materialized in concrete form. Consequently, the time is at hand when the Bank should

begin seriously to consider how its research should be applied to problems identified by operational staff.

The formulation of a problem so that it can be addressed by research requires the joint effort of operational staff, familiar with the specific circumstances in which the problem arises, and of researchers, familiar with comparative merits and the feasibility of alternative approaches. In some cases, the actual case study could be carried out equally well by operational as by research staff, provided that adequate assistance is provided by the other. But there will also be cases in which the specialized expertise required to conduct the case study implies that it must be undertaken by a researcher.

It is questionable whether Bank staff should typically be expected to carry out the case studies that replicate past research in an operational setting. It may be preferable to employ researchers from the country in question to carry out the analysis, which would simultaneously increase awareness of the issues in the particular country and further develop its research capacity. Bank staff nonetheless should undertake some case studies, in order to maintain their competence and further refine the approaches, which may often result in simplification and shortcut procedures to further reduce the cost of such undertakings.

Good arguments can be made for decentralizing the research staff involved primarily in application, particularly those concerned with incentive policies and industrialization strategies. By being decentralized, staff would gain greater familiarity with the particular circumstances of the individual countries within the different regions. There is thus a strong case for the establishment of small research units within the regional offices. A different organization may be called for where operations remain centralized, as in the case of large-scale investment projects. Here it is probbly best to locate an applications unit within the department concerned.

In either case, the decentralized units would function as vehicles for application of research methods that have been developed within the Bank.

Dissemination of research knowledge

Our next issue is how to improve the dissemination and assimilation of research knowledge within the Bank - imported as well as produced. When discussing this problem, it is important not to take too mechanical a view of the issue. The task is not mainly to bring over some specific tools and actual information to operational staff and policy advisers, but rather to spread around a certain way of looking at things.

Good interaction between researchers and operational staff is not easy to achieve. In fact, our interviews among people in the Bank revealed a certain tension between the two. This is nothing peculiar to the World Bank, however. Researchers within an organization that deals with practical affairs almost inevitably will be viewed by operational staff as being somewhat distant

and academic - a kind of research enclave. But it is important that research maintain a certain distance from the pressures bearing on the operational staff. Research is a highly specialized time-consuming job, which has to be protected to a large extent from demands of practical and administrative duties. And research needs to be organized in such a way that there is a sense of community of scholarship. If an organization like the Bank wants to acquire and keep competent researchers, it is necessary to let them do their own thing to a large extent, without too many disturbances from other activities within the organization.

Another reason for tension between researchers and operational staff is that the former are usually concerned with a much longer time perspective than the latter. The production period of research is, moreover, often so long that when results do emerge, operational staff may have lost interest in the question. And sometimes the empirical data used in research projects may no longer reflect existing conditions. (To some extent the studies of effective protection and domestic resource costs have suffered from this dilemma.)

Moreover, whereas researchers are usually interested in the accumulation of generalized knowledge, operational staff are often more interested in drawing on knowledge, in particular in obtaining knowledge that pertains to a particular situation. The researchers often regards the search for the latter type of knowledge as "information gathering" rather than research. Operational staff, by contrast, are frequently disappointed by research results because these do not always give concrete, easily accessible, ready-made and unambiguous conclusions about immediately relevant operational and policy issues.

Besides, operational staff are often not aware of, interested in, or able to absorb results or paradigms supplied by the researchers. In fact, usually they cannot possibly know in advance the type of research that could help them in their work.

On the other hand, every one of the criteria discussed above signals that research at the Bank should be guided by a thorough understanding of what operational staff know and need to know. Good two-way communication is necessary if research at the Bank is to be fruitful. It is our impression that at present communications are not as good as they should be. It would be fruitful if researchers were better informed about the usefulness of research knowledge to operational staff, and if researchers could communicate more effectively with them. Obviously it is a delicate business balancing the two organizational requirements for successful Bank research - a considerable degree of shielding from short-run interests and pressures on one hand and strong interaction between researchers and operations people on the other. We have to be satisfied with compromises between these conflicting objectives. Keeping this in mind, several reforms suggest themselves:

(a) That the researchers write, and circulate within the Bank, popularly written reports on research--concerning research produced both inside and outside the Bank. To the extent that the reports summarize outside research, the processes of import and dissemination are of course combined. In some cases, it may be a good idea to ask outside consultants

rather than researchers inside the Bank, to make the surveys. However, when outside research results and paradigms are summarized, it is probably important not only to summarize outstanding academic contributions but also to try to find out what types of research have been successfully used in other operating organizations.

(b) That joint seminars are organized by researchers and operational staff--preferably at some distance from Washington (with disconnected telephones!) to make undisturbed discussions possible.

(c) That more circulation of people between research and operational activities is brought about. Sabbatical leave for research, within or outside the Bank, for the operational staff may be one method of achieving this. Such circulation may be difficult to achieve in the field of methodological and highly technical (model-oriented) research, where the rate of skill depreciation is often very high. However, in more applied fields--where experience, empirical knowledge and common sense are important--circulation may be both possible and highly useful.

(d) That more systematic attempts are made among the researchers of the Bank to try to understand what applied people need to know in their work. It is not easy to say what is the most efficient way of achieving this. One possibility would be to undertake joint ventures between researchers and operational staff - certainly in operational activities, but sometimes perhaps also in the design and to some extent in the execution of research. It is likely that dissemination of methodological knowledge is more efficiently achieved if researchers and operational staff jointly apply suggested methodologies to concrete issues in the operational departments--in studies of projects, sectors, markets or countries. Perhaps it would also be possible to induce operational staff to make more research suggestions. More informal--i.e. less bureaucratic--procedures when drafting and planning new research projects might increase the possibilities of operational staff to contribute to the initiation of and participation in research.

Apart from individual research projects, cooperation between researchers and operational staff can contribute to the development of a research program in the area of industry and trade. A beginning in this direction has been made through the establishment of the internal panel on research on industry and trade.

The suggested research units within the operational offices (see the section on application of research) probably also could help the dissemination and assimilation of research results among the operational staff.

Moreover, the earlier suggested employment of people with research background in operational positions would not only facilitate the import of research knowledge to the Bank; it would also be a way of disseminating and assimilating research knowledge within the Bank from researchers to operational staff. This is potentially important, as imported research may be more difficult to disseminate and assimilate than in-house research. Both

the suggestion to let researchers circulate between research and operational positions, and the suggestion to recruit (more permanently) people with a research background to operational positions means that some bridges would be built between research and operations. Thus, we suggest in fact that the Bank tries more systematically to build up a staff of "bridge people" in the operational departments.

It is important to realize that the limits to using more research knowledge within the Bank are probably determined more by the absorptive capacity of research among the operational staff--limited time as well as limited ability and interest to absorb research knowledge--than by the capacity of researchers within the Bank to produce and summarize research. This means that a larger volume of research within the Bank should perhaps not be expected to have much effect on the operational side of the Bank, as long as the deficiencies of the systems of dissemination and assimilation of research within the Bank have not been removed.

The role of researchers

Bank researchers have separate roles to play in the import, production, application, and dissemination of research. It is very easy, in an operational institution, for the production of research to be sacrificed to the objective of better utilizing existing research. This is particularly true in an institution like the Bank, where significant efforts to apply research are just beginning to be made. Continued production of high quality, innovative research within the Bank will require a strong commitment to protecting the time of research staff for the production of research. This should of course not be interpreted as an endorsement on our part of an "ivory-tower" attitude among researchers. This said, it must be considered whether the time spent by researchers in their non-research roles is being put to its potentially most effective use. Direct involvement in operational missions by research staff consumes much of their non-research time, with the result that they are not as available for consultation in regard to application as might be desirable. We equally suspect that dissemination could be much better organized were more resources devoted to it.

C. Research implications of alternative development strategies

A major task of our report is to discuss future research priorities of the Bank in the field of industry and trade. An important background to such a discussion is (i) a specification of what types of countries we are talking about and (ii) some kind of "vision" of what the mechanisms and driving forces of economic development are supposed to be in these countries.

For instance, if we talk about countries with a strong emphasis on central planning, nationwide planning models and empirical studies of the process of central planning in various countries would probably be top priorities. It is important to remember that there are substantial elements of decentralization of information, decision making and initiative

also in centrally planned economies. Thus, it is of great interest to study incentives and constraints on behavior at various levels in centrally planned systems.

The reason why the Bank has not put any great amount of research resources into the acquisition of research knowledge in this field is most likely that very few member countries of the Bank are centrally planned economies. Elements of planning exist everywhere, however, because of the great role of government decision making in economic matters in all countries today. This means, of course, that the process of government decision making is an important area of research for all countries.

(The macro models that have been developed by the Bank for entire economies, or even for the whole world, should probably not be regarded as tools of central planning, but rather as descriptive models or forecasting models, and to some extent also tools of analyzing the effects of alternative policies.)

A more modest version of planning would be sectoral planning or programming of investment decisions, for instance, in sectors where there are huge returns to scale, externalities, or (direct) intersectoral linkages. Then it may also be possible to consider aspects that are usually not well caught, if at all, in conventional static microeconomic investment calculations. With this approach, studies of investment planning in some sectors would be of rather high priority. It has been the case for the Bank in recent years (see Appendix I.1), in the sense of investment programming - mainly in cases where the optimum size of a firm is of about the same magnitude (or larger) than the entire national market. Another reason why sector planning models, or at least investment programming models, may be of interest is that in many countries, perhaps particularly in less developed countries, a number of infrastructure and processing industries, for which investment programming models may be particularly useful, are in fact under rather detailed central government control.

It is important to emphasize that microeconomic investment planning models of this type, which are really tools of management decision making, are quite consistent with either a market-oriented or a centrally-planned macroeconomic system. In reality the Bank's research in this field has not been framed in the context of nationwide central planning models, but rather as a means of exploring the range of choice of firms operating on markets. The same holds for Bank research on capacity utilization, capital--labor substitution and technological change (see Appendix I.5)--a research field of great interest in the case of both centralized and decentralized versions of the development process.

Research knowledge of patterns of growth of production and trade (see Appendix I.2) is also of considerable importance--especially perhaps for acquiring a broad understanding of the development process--in the case of both rather centralized and more decentralized strategies of economic development. In particular, Bank research in this field has helped provide norms of growth patterns of industry and trade, against which developments in individual countries can be judged.

In most of the member countries of the Bank, the bulk of the development process is no doubt guided by decisions of decentralized units motivated by profits and stimulated and constrained by markets. However, it is important to realize that the adherence to a pronounced decentralized strategy of economic development, in the context of a market economy, does not imply the absence of central policies and planning, but rather more reliance on different types of policy and planning operations from those in centrally planned economies. Obvious examples, besides general monetary and budgetary policies, are institutional reforms and improvements in the systems of incentives. In countries of this type it would therefore be of interest to find out how conducive alternative institutional arrangements and incentive systems are to releasing efficient decentralized initiatives.

While research on incentives in product markets, in particular trade policy incentives for firms, has been given a high priority in Bank research (see Appendix I.1), it is only recently that research has been launched on incentives and imperfections in factor markets and incentives for employees (households). By this we mean for instance the structure of interest rates, the performance of credit rationing and the mobility, and flexibility in general of the credit and capital markets, but also the structure of wage rates, the incentives and possibilities for labor to move, acquire skills, and advance.

Nor has there been much emphasis in Bank research on the importance for economic development of institutional arrangements (see Appendix I.4), though an increased interest in institutional factors can be detected in various research efforts of the Bank in recent years.

The only aspect (dimension) of development strategies mentioned so far has been types and degrees of centralization of economic decision making, which concerns the mode of economic organization. Development strategies also differ with respect to resource allocation strategies, such as the choice between export-oriented (outward-looking) and import subsitution-oriented (inward-looking) strategies of economic development--a topic highlighted by Bank research on trade policy incentives and patterns of growth.

Another important dichotomy is between strategies that rely on the assumption that incomes and employment opportunities will rather automatically "trickle down" to poor groups of the population, and strategies that more actively promote employment opportunities and income redistribution at an early stage of economic development, which according to experience (for instance, South Korea and Taiwan) is not inconsistent with an export-oriented and market-oriented strategy of economic development.

A third dichotomy concerning resource allocation, finally, is between countries that rely on a rather passive attitude towards what particular consumer goods are supplied to the domestic population, and strategies that rely more actively on the provision of some basic needs of food, shelter, health, etc.

In reality, the economic systems of the LDCs that are members of the World Bank are characterized by various combinations of centralized and decentralized decision making, and with different strategies for employment creation, redistribution and the provision of basic needs. The development strategies are also heavily dependent on a number of other features of individual countries, such as the initial condition of the stocks of physical and human capital, natural resources and historical traditions. Bank research should reflect this diversity concerning possibilities and strategies of economic development. So, of course, should our recommendations.

D. Final Comment

The purpose of this chapter has been to formulate certain general principles when pursuing and using research within the Bank, in particular as it relates to industrialization and trade. Two of the principles discussed--the ones concerning comparative advantage and residual supply of research--suggest strong emphasis on studies of the consequences for economic development of alternative institutional arrangements, technologies, government policies and incentive regimes. The reason is that both approaches are based on the assumption that research should be related to the fields of Bank operations, either because the Bank acquires special competence in such issues through its operations, or because knowledge is needed in these fields for successful Bank operations (and is not easy to import). Though there are good reasons for emphasizing empirical and applied research for an organization like the Bank, there is also a need for theoretical and methodological studies. One reason is that the competence of the Bank as a research unit is thereby enhanced, another, that the Bank can play a major role in advancing the frontier in these fields. The third principle considered--to create externalities within the Bank in the sense of high general competence--suggests a much more diversified research portfolio, with various types of research that contribute to broad knowledge about the development process. Finally, the fourth general principle--to increase the research competence in the LDCs--suggests that the fields of research chosen are those of interest to scholars and research institutions in the LDCs, and that research procedures are adopted that facilitate the participation of such scholars and institutions in the research program of the Bank.

We have also discussed various ways of improving the ability of the Bank to make import, production, application and dissemination of research more efficient. In particular, we have emphasized the potential usefulness of increasing the awareness of both researchers and operational staff in the Bank of each other's interests, needs, and competence. We have also suggested a number of arrangements to help achieve this.

In general, both researchers and operational staff are quite aware not only of the potential importance of Bank research but also of the difficulties involved in choosing efficient research procedures and useful research fields. We would in fact be inclined to argue that the Bank worries too much

about its research, in the sense that perhaps too much time and energy are devoted to planning and reviewing the research activities of the Bank. A slightly sanguine, but perhaps useful, recommendation would be: "Worry less about your research, do it instead!" Or more concretely: discuss carefully the general direction of research efforts, i.e. the research fields, employ the best senior and junior scholars you can get in these fields, and then give them considerable autonomy to do their work--in contact with operational staff but without too frequent demands as to the reporting and evaluation of their work.

CHAPTER II

PAST AND FUTURE RESEARCH ACTIVITIES ON INDUSTRY AND TRADE: A SUMMARY

A. Review of Past Research

Over the past years the World Bank has dedicated a considerable volume of resources to research on industry and trade in economic development. This research has explored a wide range of topics, a number of these in considerable depth. A list of projects and papers is given in Appendix II. For convenience, the Panel divided up past research into six broad clusters. These are briefly described below. The six sections of Appendix I provide more detailed descriptions of the research in each of the areas. Research reviewed included projects approved by the Bank's Research Committee (so-called RPO projects) as well as other research output financed by the Bank.

Incentive Policies and Economic Integration

One cluster of Bank projects has been concerned with incentive regimes and development strategies of LDCs; we also placed in this group studies on economic integration among developing countries. Included here are RPOs 670-01 (Development Strategies in Semi-Industrial Countries), 670-22 (Economies of Scale and Tariff Levels), 670-87 (Industrial Policies and Economic Integration in West Africa), 671-10 (Promotion of Non-Traditional Exports) and, now under way, 671-75 (International Trade Policy for the Development of Bangladesh), and several other (non-RPO) projects as well.

Most of these projects deal with the role of incentive systems in economic development, from both a theoretical and an empirical standpoint, and explore the resource-allocational, growth, employment and balance-of-payments effects of various government policies, particularly of those aiming at import protection, export promotion, and economic integration. Considerable use has been made of the concepts of effective protection and domestic resource costs, appropriately improved in theoretical and computational terms. While the incentive structures have been analyzed in a number of different countries on a comparative basis, there have also been attempts to evaluate incentives on a firm-by-firm basis and to appraise the impact of protection at the level of the individual investment project.

With the exception of the small study on economies of scale and tariff levels, the projects in this cluster were completed successfully or are in good progress. A definitive evaluation of the Bangladesh study is not possible at this stage; completion is scheduled for end-1979. Its relevance, however, is beyond any doubt, particularly so as it is a case of application of the more basic research on incentive regimes.

The major findings of the research referred to in this cluster, especially those of RPOs 670-01 and 670-87, are of considerable interest for policy-making purposes as they support the efficacy of promoting rather than protecting industries, and of avoiding discrimination against exports rather than overemphasizing import substitution, if efficient and rapid economic growth in LDCs is to be achieved. The conclusions and policy recommendations rest upon firm theoretical foundations and a sound factual basis. Moreover, they are timely, because reliance on and use of import controls by developing countries, combined with an array of additional government interventions in the domestic markets, still persist and guidelines for policy reforms are therefore needful. Finally, this research has been found quite helpful by the Bank's operational staff, who are applying both the findings and the methodology in country economic reports, policy analyses and in-house estimates of incentives, let alone its initiatives for additional research in this field.

Comparative Advantage, Trade Patterns and Economic Growth

This cluster of projects includes RPOs 670-07 (International Model), 670-19 (Expansion in Manufacturing for Exports in Developing Countries), 670-79 (Economic Development of East and Southeast Asia), 671-05 (Patterns of Industrial Development), 671-32 (A Comparative Study of the Sources of Industrial Growth and Structural Change), and 671-79 (Sources of Growth and Productivity Change), and two non-RPO studies as well.

The unifying theme is the objective of explaining the pattern of resource allocation within and between countries, economic growth, and changes in industrial and trade structures as a function of various country characteristics, including policies employed. While two projects (670-19 and 670-79) were clear failures, the other ones met, by and large, the research objectives. The failures are regrettable, indeed, since the projects addressed the questions of how to shift manufacturing activities from developed to developing countries and of how to plan competitive export industries in the developing countries; information on these matters would have been an extremely important ingredient of any effort to shape rational industrialization strategies.

The projects included in this cluster differ greatly in their methodologies. Some of them involve modeling that is very simple or indeed primitive; others involve attempts to empirically implement a very complex general equilibrium methodology. In the view of the Panel the most successful of the projects is RPO 671-32 that employed and elaborated an accounting framework based on sector-specific supply-demand identities for analyzing the nature of modern industrial growth. The research guided by the more ambitious general equilibrium conception (RPO 670-07) has not yet added much to existing knowledge about the development process. Moreover, skepticism about the usefulness of this type of research predominates within the operational staff. It still remains to be seen whether the computable general equilibrium model applied in the "Sources II" project, RPO 671-79, which has only just started, will produce significant empirical results to an extent which could not have been obtained at lower cost from simpler macroeconomic formulations.

Export Promotion Policies in the less developed countries and Access to Markets in the developed countries

Included here are RPO projects 670-20 (Industrialization and Trade Policies for the 1970's), 670-21 (Export Promotion and Preferences: A Case Study of India), 671-35 (Export Incentives in Developing Countries), 671-56 (Marketing Manufactured Exports), 671-66 and 671-67 (Effects of Increased Imports of Manufactured Goods from Developing Countries in Western Europe and in the United States, respectively), and 671-68 (Key Institutions and the Expansion of Manufacturing Exports), as well as a number of non-RPO analyses and reports. All but the first two RPO projects are still under way.

The major thrust of the research included in this cluster is the hypothesis, prominent at the Bank, that successful expansion and diversification of exports is a key characteristic of many recent development experiences. The work done so far includes data compilations, surveys of selected industries, analyses of commodity markets, studies of problems of import restrictions by the developed countries on the LDC manufactured exports, consideration of national policies and institutions for trade promotion in the developing countries, and analyses of the overall environment for the exports of these countries. Some of the reports on these subjects can be characterized as reports aimed at guiding Bank operating departments and serving as background material for Bank projections and policy work in this area.

As was the case with the projects discussed under the second cluster, the research methodologies and styles of the projects considered here have differed widely. The more descriptive studies on selected industries provide a great deal of empirical information which, however, will be useful only if they are kept up to date. Among the completed RPO projects the one on India, while a priori important in itself as a case study of a less successful country, was not well conceived and has not achieved publishable standards. But some of its conclusions fed discussions in India on the Government's export policies. By contrast, the project on industrialization and trade policies deserves high praise. It made an important contribution in documenting the impact on industrial countries of the manufactured export expansion by developing countries and it has stimulated further research on adjustment problems in developed countries.

Of great relevance to the operational staff and policy makers might be the ongoing project on export incentives in developing countries, which is designed to yield practical information for countries that contemplate the effective promotion of export activities. The ongoing research on key institutions holds also good promise and might fill a large gap in existing information on the marketing of exportables. The two ongoing projects on import market penetration in (twelve) developed countries reflect a serious attempt at understanding the political economy of trade protectionism. The topic is important from the export-oriented developing countries' point of view and the Bank plays the role of a residual supplier of research in this field.

Small Enterprises, Credit Markets and Public Enterprises

The projects here include RPOs 670-77 (Financing of Small-Scale Industries), 671-59 (Small-Scale Enterprise Development), 671-65 (Small Enterprise Financing: Role of Informal Credit), 671-69 (Capital Market Imperfections and Economic Development), and 671-11 (Managerial Structure and Practices in Public Manufacturing Enterprises), in addition to a number of non-RPO studies relating to social security institutions. The importance of these topics for the Bank's lending operations and advisory role in developing countries is obvious.

Many of the projects have not yet been finished and, in some cases, they are still at the conceptual stage. It is therefore difficult to provide for a definite evaluation of the research in this field. In contrast with the work on incentive regimes and export promotion policies which proceeded under the presumption that the objective was clear enough and that the task was to find the appropriate instruments, the projects included in this cluster have faced much more uncertainty regarding what ought to be achieved. This is an explanation for the explicitly exploratory character of projects in this field. The difficulties for the researchers were compounded by the fact that the existing literature is mainly descriptive rather than analytical.

While applauding the willingness to enter this important area, the Panel recognizes that research on these topics is still groping for appropriate methodologies. We also notice that research on capital market imperfections and public enterprises put too much emphasis on the experience of one country, namely India. Moreover, the available papers on capital market imperfections suggest that analytical improvement is still possible in this field. And while the quality of the research done so far is reasonably good by international standards, particularly in the case of small scale enterprises, it is still below the quality levels achieved in the other areas reviewed in this report. It may be some time before the Bank develops the capability to do first rate studies of small-scale industries, credit markets and public enterprises and their influence on development.

Capital Utilization, Capital-Labor Substitution, and Technological Change

Like the work on incentive regimes, trade patterns, and export promotion, and unlike the research on small enterprises, credit markets, and public enterprises, Bank research on capital utilization, capital-labor substitution, and technological change has proceeded within standardized methodologies and has attempted to test concrete hypotheses. Included here are the RPO projects 670-23 (Scope for Capital-Labor Substitution in the Mechanical Engineering Industry), 670-25 (Industrial Capacity Utilization in Selected Latin American Countries), 670-54 (Employment and Capital-Labor Substitution), 670-95 (Industrial Capacity Utilization), and 671-51 (Appropriate Industrial Technology). Most of them are completed, the RPO 671-51 being behind schedule. Three non-RPO studies are completed as well.

The policy thrust of the research has been provided by the observation that in developing countries labor is cheap and capital expensive relative to developed ones, and that this ought to be reflected in the use of more labor-intensive techniques. But while this has occurred to some degree it is still possible and desirable that the techniques employed be more frugal in the use of capital. At the same time, it is shown that despite the relative scarcity of capital in developing countries, productive capacity is not used very intensively in many cases.

Research has been concerned with market and other forces that explain the prevailing situation, and with policies that could improve the environment so that the choice of technique could be made more appropriately, techniques used more efficiently, and appropriate adaptation and learning proceed more effectively. On the whole, the research has been of varying quality. The most serious effort was RPO 670-23, where much care was taken in translating a well-designed theoretical framework into an empirical analysis. At the other extreme was RPO 670-54, which had to be abandoned as hopeless. RPOs 670-25 and 670-95 were in between and they did not add much to existing knowledge. The research on the design capabilities of domestic capital goods producers (RPO 671-51) is promising, and may lead to important further research. In general, however, the Panel finds the recent work exploring in great detail the scope of capital labor substitution in particular technologies important, but running into diminishing returns as a research endeavor.

Programming in the Manufacturing Sector

The sixth cluster of Bank research is on investment programming and has many connections with research on the topic considered in the section above, although the emphasis so far has been placed on optimization. The major project included here is RPO 670-24 (Programming in the Manufacturing Sector). The analytical work has been concerned with optimal programming of investment where there are significant economies of scale, or strong interdependence among manufacturing activities as for example the sharing of machinery. Empirical studies have been done of several process industries and one non-process industry (mechanical engineering).

The research has estimated the size of scale economies in certain process industries such as fertilizers, cement, pulp and paper, forest products, energy, etc. (such scale economies are shown to be important in some cases). Moreover, it has tested the utility and feasibility of using formal programming models in guiding investment decisions under economies of scale (with encouraging results). The research also has considered some of the implications of economies of scale and strong inter-activity interdependence for regional cooperation.

So far, the research has been successfully carried out. It has made a significant methodological contribution, it has the potential to improve the rationality of government decisions in developing large process industries, and it may prove useful in guiding Bank lending operations (provided the research results are available in time and can be understood by the operational staff).

The work complies with both the comparative advantage and the residual supply of research approaches. It is our impression, however, that a stage has been reached where the methodology developed for process industries has to be disseminated effectively for application in both the Bank and LDCs. The Manuals currently under preparation constitute an important contribution to these extension attempts. A conduct of a case study for a sector, which is carried out with an active participation of a LDC team or institution, would support the extension work. As far as research on nonprocess industries (namely the study of the Korean Mechanical Engineering Sector) is concerned, its generalizability has not yet been proved, and the majority of the Panel is skeptical whether an expansion of the project is worthwhile.

B. Overall Evaluation

The Panel attempted to evaluate Bank research on industry and trade along a range of dimensions, reflecting the multiple purposes of research at the Bank. Some of our criteria related to the Bank as a research producer and as a member of the scholarly research community. Here we attempted to assess the contribution of Bank research to the understanding of the economic development process and policy issues relating thereto. What was the absolute quality (in some sense) of the research output of the Bank? To what extent did Bank research reflect its comparative advantage? To what extent did Bank research proceed in conscious awareness of past and ongoing research elsewhere? Other criteria related to Bank research as it contributes to the applied objectives of the Bank. How useful has the research been in guiding Bank decision making, either regarding lending operations or regarding policy advice? How useful has the research of the Bank been to policy makers in LDCs? What contribution has the Bank research program made to the building up of indigenous research capabilities within the developing countries?

Finally, we attempted to identify the factors that seemed to explain why certain areas or styles of Bank research were more valuable or important than others. Were there certain styles of research that the Bank did well? Could one identify certain confluences of factors associated with particularly good and useful research, or poor and not-so-useful research? Were there certain distinguishing administrative arrangements associated with good and poor research?

Appendix I treats these questions field by field. The Panel noted significant differences in the overall quality and relevance of Bank research in the various fields, and the more fine-grained evaluations also differ from field to field. However, there were certain general and common judgments that we made. These we recount below.

By and large, we are impressed by the overall high quality of Bank research on industry and trade in economic development. Viewed solely in terms of its research output (much of which has been published), the Bank clearly ranks as one of the most distinguished development research centers

in the world and certainly the leading one among international organizations. In many cases, the researchers have made a remarkable effort to improve the methodology for policy analysis and investment appraisal. The work has been to a large extent creative rather than imitative and, in its applied aspects, generally complementary to the research in the field undertaken elsewhere.

Bank research on industry and trade, being mainly empirically oriented, has made outstanding contributions to knowledge about the structure of incentives bearing on business firm decision making about import substitution and export expansion in developing countries, particularly regarding the effects of tariff and non-tariff devices. Bank research has been in the forefront of scholarship positing and supporting the view that outward looking development policies are feasible and have been highly effective. More recently, research at the Bank has contributed importantly to the understanding of changing patterns of LDC exports. Work at the Bank has shown how resource allocation patterns within a country relate to country characteristics such as its income level, market size, and policy orientation. Research at the Bank on intensity and efficiency of use of capital and labor has documented the wide range of choice of techniques available, and also the informational and institutional aspects of an economy that bear on choice of technique. Bank research on programming methods, while not yet bearing much operational fruit, has explored and pushed forth the state of the art. Bank research on small enterprises, credit markets and public enterprises, while just beginning and still floundering somewhat, has a chance of providing leadership for a kind of research that has been sadly neglected by the academic research community, provided high capacity resources are made available.

By and large, Bank research on industrialization and trade has reflected its comparative advantage; but in a number of cases the residual supply of research approach was pursued. As the research in this field places high demands on data, much of what was done could not have been done at all, or would have been very difficult to do, in a university setting. This is the more so as Bank research in this area has been concentrated on comparative studies, which allow for generalizable policy prescriptions, rather than on specific cases, which would be of limited value only. With very few exceptions, Bank research has been undertaken in good awareness of the state of the art and of what was being done elsewhere.

It has proved much harder for the Panel to evaluate the influence of Bank research on Bank decision making, or on policy making in the developing countries, or upon the strength of the research communities in these countries, than it has been for us to judge the scholarship on its own terms. Our discussions with operating personnel within the Bank have helped us to understand these issues to some extent. The basic problem we had in those discussions was the tendency for operating people at the Bank (this we believe is a tendency of operating people everywhere) not to talk about the influence on their own thinking of the basic ideas and understanding that emanate from a research tradition regarding the applied problems they face, but to discuss

the contribution of research in terms of detailed pieces of analysis, or data, that are of use concretely and specifically in decision-making. In our judgment, the influence of ideas and concepts on policy-making usually is much more important than the influence of particular facts that might come from research.

With these caveats in mind, it is our impression that a number of different strands of Bank research have influenced, directly and indirectly, Bank operations. The influence probably has been stronger on operations aimed at influencing overall policy within countries, than with respect to specific lending decisions, although there are a number of instances of the latter where Bank research clearly has had an impact.

The concept and the quantification of effective protection rates together with the arguments, as well as the evidence, that protection often leads to uneconomic use of resources clearly was in the heads of the Bank officials with whom we talked. Similarly, there appeared to be widespread adherence to the proposition that an export-oriented development strategy was an attractive alternative to excessive import-substitution policies for countries to consider. Both of these notions seemed to be mentally connected with the view that decision makers did face a choice of techniques, that the highly capital-intensive techniques of U.S. manufacturing were often uneconomic in the context of less developed countries, but that uneconomically rigged factor markets and import protection regimes often encouraged and supported unnecessarily capital-intensive investments.

In their statements about the kind of research that they found useful, and not so useful, Bank personnel tended to laud studies which provided data, or examined particular institutions, let alone the whole field of incentive regimes. It is our conjecture that this very policy-oriented research may in fact have been more influential regarding decisions on particular loans than the more general analyses done by Bank researchers. However, it is the provision of the more sweeping ideas that has influenced the way Bank officials view appropriate economic development policies and set their positions in bargaining with LDC officials.

We feel in an even weaker position to judge the impact of Bank research on policy making in the developing countries. A real impact could be recognized with regard to the studies on incentives and domestic resource costs in industrial and agricultural activities. For the other projects, we would conjecture that all of our remarks above obtain. Where the research done at the Bank has had influence, we suspect this has been largely through affecting the general climate of thinking, and through its effects on dialogue between the Bank and government officials of developing countries. But we were unable to acquire much direct confirmation of these conjectures. On the other hand, we noticed that some shifts in Bank policy thinking (as the growing interest in the "basic needs" approach) have not (yet) influenced research either.

Research projects at the Bank have differed significantly in the extent to which they have contributed to the building up of research capabilities in the less developed countries. There has been very little effort to work with research institutions in these countries specifically with the purpose of helping these to develop. Our conversations with researchers at the Bank indicate a considerable reluctance to do this, on the grounds that it is very difficult, and would tend to interfere with the task of getting on with the research. Some of the Bank's projects have been done almost exclusively in-house, and have not involved LDC researchers at all. But a number of the projects, particularly those involving primary data collection in developing countries, or case studies of particular industries or policies, have involved researchers in the countries concerned. These projects, therefore, have helped to bring these researchers into the mainstream of development research, and to establish or reinforce contacts with the scholars at the Bank.

Though we have no way of assessing the overall importance of the contributions to the growth of research capabilities in developing countries due to the participation of local scholars and research institutions in completed or ongoing Bank projects, we found some cases in which further research was stimulated in the countries concerned. Generally speaking, the Bank policy of working with researchers and institutions of developing countries should also be recognized as a means of enhancing the research capabilities in this part of the world.

Our relative assessments of the research projects that have been undertaken by the Bank in the industry and trade field suggest two strong correlates of research quality. One is strong interest and leadership by a senior researcher on the Bank staff; by and large, Bank research has not been particularly successful when it has been farmed out to consultants. The second is a confluence of strong conceptual or methodological elements in the project and a set of broadly but clearly defined questions. In general, we have not been impressed with the success of Bank projects that have been motivated largely by either "pure" interests devoid of clear-cut connections with important policy questions, or a particular policy interest involving little analytical structuring.

We recognize that the Bank's research portfolio should contain a diverse mix of projects, involving different degrees of farming out. We would point out, however, to the fact that quite detailed attention and involvement of first-rate senior Bank researchers in a project has in the past been almost a pre-requisite for research success. We also recognize that in the pulling and tugging between the intellectual interests of the research staff and the more applied interests of Bank operating officials the outcome should be a spectrum of projects ranging from relatively basic to quite applied. But we propose that the Bank's research successes in the past have not been at the extremes of that spectrum, but rather on projects where intellectual interests and policy concerns in terms of issues and usable methodologies have come together. As research in the industry and trade field

was mainly applied rather than "pure", policy recommendations made by the Bank to governments in developing countries were consistent with the most recent body of knowledge generated in this area.

C. Future Research Priorities

We have argued in Chapter I that the choice of future research priorities has to be based both on the development strategies actually pursued by member countries and on some assumptions ("vision") about what are the most important forces and mechanisms of economic development in these countries. The diversity of the economic institutions and policies of member countries, and of the views about the development process, suggests a rather "pluralistic" research program for the Bank.

We have suggested four general principles, or motives, for Bank research:

(a) To contribute to the fund of research knowledge in the world about the development process; a "comparative advantage approach" is then appropriate.

(b) To improve upon the research knowledge that is needed for Bank operations and policies; a "residual supply of research approach" is then appropriate.

(c) To create externalities within the Bank for its operational and policy formulating staff in generating a more analytical view of the problems and an increased level of "sophistication."

(d) To help generate research knowledge and research capacity in the LDCs.

This means that recommendations regarding future research priorities must rest on subjective judgments regarding a number of matters, including the importance of different kinds of research in enhancing general understanding of development processes, the comparative advantage of the Bank in different kinds of research, Bank needs and LDC needs for certain kinds of studies to enhance their decision-making ability, the kind of research that is likely to attract and hold excellent scholars at the Bank, and the kind of research most amenable to cooperative endeavors between the Bank and LDC institutions.

As we have seen in Chapter I, the different principles suggest somewhat different priorities, strategies and procedures of research within the Bank. However, in reality, it is not advisable to choose one of them but rather to make compromises between them. If we would emphasize some aspects of such a compromise more than others, we would suggest that research is concentrated in fields where:

(a) knowledge is particularly strongly needed for Bank lending and policy advising;

(b) the Bank in its operations acquires research competence that is unique;

(c) a strong research organization and a system of follow-up research, mainly in the case of large projects, are required.

A basic requirement in all three cases is that the Bank have, or be able to hire, highly competent researchers.

Appendix I presents rather detailed views about the kinds of research that, according to our view, ought to be cut back and the kinds that ought to be augmented, for each of the six broad fields of evaluation. Here we attempt only a general statement of research priorities. It may be useful to consider first the possibilities of freeing research resources from previous research areas, and thereafter to consider areas into which we recommend the Bank to put more resources.

We think that there are certain lines of research at the Bank which in the past have been forceful and productive, but which now are running into diminishing returns. These include such traditional and successful Bank research fields as research on rates of effective protection or subsidy, and on patterns of growth and development. In both of these fields Bank research has broken new ground, but the ground is now well broken.

In the case of research on trade policy incentives, it is reasonable to argue that the research phase is now largely over, and that what remains to be done are further applications - by including more countries, and by updating previous calculations. However, the resources for these activities should, in our judgment, not be taken from the research budget, but either from the budget for operations and policy formation of the Bank, or from a special (separate) budget to be allocated to the regions and the other operating units for application of research after the "pure" research phase is over. Otherwise the suggested research units for application would perhaps not be able to shield their resources from the demand of operational work.

Similarly, while Bank research on patterns and sources of growth, based on regression and input-output analysis, has been useful and illuminating, it is unlikely that much new will be learned from doing more of these studies, or from doing them in a slightly different and more sophisticated way. Thus, the studies of patterns and sources of growth are also mainly completed, or near completion. However, it is not clear whether these studies lend themselves to application for the use by operational staff; the studies have perhaps mainly served to improve rather general knowledge about the development process.

We also propose that Bank research exploring the range of technical choice and opportunities for capital-labor substitution has run into diminishing returns. The basic points have been well documented. It is unlikely that doing more studies would add much to the ability to persuade people that in fact the range of choice is quite wide, and that it matters what choices are made. The Bank lending departments need to be able to do these kinds of studies themselves in the context of exploration of the range of choices available for particular investment programs they are contemplating, and to educate and persuade borrowing governments or governmental agencies about the range of choice. We propose that this body of work, like the work on effective protection rates, should be moved out of research and moved into applications.

We have the same judgment regarding Bank research on industry investment programming for process industries, though some "software" development is necessary to make applications more routinized. What is needed now is for the operating departments to develop the capability to work with the models; moreover, extension services for application to practitioners outside the Bank are also required, since this type of study will generally be undertaken in the LDCs.

In the case of both labor-capital substitution and process industry programming, the work on applications should probably not be done in the regions but rather in some more centrally located unit in the Bank - considering the size and complexity of analysis of this type.

The Panel is somewhat divided regarding whether or not the Bank should cut back on its research on programming models for non-process industries, and economy-wide models based on a computable general equilibrium framework. We recognize that professional research on quantitative general equilibrium analysis and modelling is proceeding at many institutions around the world and that it is considered a challenging and potentially rewarding field of activity. Most of us doubt that these bodies of research will contribute much directly to understanding relevant to policy-making. We believe that an understanding of the development process requires mainly other types of knowledge than is likely to be acquired by these projects. Thus, the majority of us are rather skeptical about the fruitfulness of this type of research, relative to some other fields.

It may be useful if we spelled out the grounds for our skepticism. General equilibrium models have obvious and important attractions as instruments of economy-wide economic analysis. They have the advantage of making explicit the interconnections among economic activities, the complex and not always immediately obvious relations between prices and quantities, and the economy-wide impact of policies that work through the price system. But associated with these advantages, the large-scale general equilibrium models have two disadvantages, which, in the view of this Panel, weigh heavily against them.

One is that these models tend to be so complex that the mechanisms which they describe are difficult to evaluate and modify in the light of knowledge about what the model leaves out or oversimplifies while the additional insight gained by more complexity is often not worth the effort. Second, the equilibrium character of these models makes it very difficult to deal with the fact and the consequences of technical and institutional changes which, we believe, are the prime driving forces in economic development. These changes can be built into the model, but only in a rather mechanical way. Our concern is that economic development is an inherently dynamic process with constantly changing structures and this is basically what these models tend to repress.

We recognize that not all economists share our views on these matters. We also believe it important that the Bank keep up with new trends in development economics, and the use of computable general equilibrium models may be such a trend. Also some of the Bank's most highly trained economists are interested in this work. These arguments make some of the Panel reluctant to urge that the Bank withdraw from the field, and willing to urge continuing support of a low cost effort. But the Panel is unanimous that the most important research topics lie elsewhere.

We think there are several broad fields of research to which the Bank should allocate more resources. These include, first, a set of topics related to exports of the LDCs, evolving patterns of international trade, and policies of LDCs that reflect trade opportunities and constraints. Second, a set of topics concerned with economic institutions and domestic economic policies that facilitates or deters development. Third, research relating to adoption of technology, innovation, and technical change in developing countries.

Topics Related to Trade

Over the years the Bank has built up a comparative advantage in research relating to LDC trade patterns and policies. Research in these fields has contributed importantly to policy thinking within the Bank, and has been sensitive to policy questions, thus also meeting the "residual supplier" criterion. The research that we recommend below represents continuation and strengthening of work now under way at the Bank and we hope that present staff constraints in this field can be relieved soon.

We believe that Bank research on export promotion policies and market access should be continued and expanded. We think particular emphasis should be placed on studying evolving trade patterns, with special focus on inter-LDC trade. Productive areas of research include cost-benefit analyses of inter-regional trade in the framework of preferential agreements, an evaluation of different avenues of economic integration as well as an assessment of the prospects for such an integration, and the effects of common financial institutions on investment patterns. Studies in this field should not be confined to regional arrangements, but should also analyze in depth inter-LDC trade that takes place independently of such agreements.

We also feel that there is a need for a more systematic differentiation among LDCs with regard to industrial growth paths, trade patterns and policies to support them. Developing countries can be grouped in those that are rich in natural resources, those that are just beginning their industrialization, and those that are still extremely poor and have not yet started to develop manufacturing activities. An alternative distinction could be made between countries rich in mineral resources, those with substantial agricultural resources and low population density, and mineral-poor countries that are densely populated. These groups of countries are characterized by significant differences in terms of domestic saving potential, labor skills, entrepreneurship, economic structure, export orientation, the role of direct foreign investment, etc. The development model of semi-industrial economics in Latin America and East Asia, extensively studied in the past, does not cover the wide range of issues that the "late starting" LDCs must face. By analyzing them in depth, and evaluating their trade opportunities, country and sector work in the Bank will obtain firmer operational guidance. In this connection, the research should also provide insight into the prospects of these "late starters" for self-sustained economic growth and successful integration in the world economy. It should also assess the (potential) advantages and disadvantages of pursuing the "basic needs" approach combined with a strategy of (total or selective) "delinking" from the international economic system, as forcefully advocated for in some respectable quarters.

We have also seriously considered the idea that the Bank should launch a major research effort to the question of adjustment mechanisms to reallocate resources in the developed countries in response to changes in technologies, preferences and comparative advantages in the world economy. A main reason for such a research effort would be that one of the main things the developed countries could do for the LDCs is to adjust their own economies to the export efforts of the LDCs, i.e., to provide access to markets for these exports. However, we believe that the developed countries should do this type of research themselves. Our recommendation on this issue is therefore that the World Bank strongly advise the developed countries to give high priority to research on reallocation of resources and adjustment policies in the developed countries, rather than the World Bank moving heavily into that area in its own research.

Topics Related to Internal Policies and Institutions

As with research on LDC trade, the World Bank over the years has been a leading institution doing research on the distortions to economic allocation and deterrence to economic efficiency associated with protection of domestic industry from external competition. We believe the time is now ripe for the Bank to shift the focus from the economic effects of tariff policies to more general consideration of how policies and institutions influence resource allocation and efficiency within a country. We propose that such research fruitfully can exploit the comparative advantage of the Bank in doing comparative country studies.

Earlier work on capital utilization and capital-labor substitution led to a recognition that factor market conditions played an important role in influencing choices. In turn, labor and capital markets are strongly influenced by a variety of government policies. These policies, for example, labor legislation, and policies imbedded in financial institutions, warrant considerable study on a comparative basis.

We think the Bank should venture into the study of industrial organization in LDCs and exploration of the effect of different kinds of policies on organizations and on economic performance. Many LDCs engage in price control activities, and other forms of price and input regulation. It would be fruitful, we believe, to examine the effects of regulation in developed countries. Bank research on small-scale enterprise is evolving to consider the effects of controlled markets and imperfect markets more generally on the viability of small firms. We endorse research in this field and suggest considerable efforts be made to apply best available methodologies and to try to improve these. We think it would be fruitful to study more generally whether it is economies of scale or market imperfections, that support the monopolistic or tight oligopolistic structures that mark many LDC industries.

Moreover, in many LDCs, public enterprises usually provide transport, power, and a variety of other public services. Many countries are also employing public enterprises for the production of manufactured goods, particularly when significant economies of scale are involved. The question of the relationship of public enterprise with other industries and with government pricing and incentive policies, and more general issues relating to management and investment planning in public enterprises, strikes us as important to study, probably in a country- or industry-specific context. The World Bank has initiated some research in this field. We urge that the field be given quite high priority.

Topics Related to Innovation, Entrepreneurship, and Technological Change

Earlier we expressed our belief that economic development must be understood as a process involving technological advance in an essential way. Bank research on capital-labor substitution and appropriate technology increasingly is recognizing this. We recommend that the Bank explicitly and consciously do research on mechanisms of technology transfer, adaptation of technology to better fit local economic conditions, innovation in industry in less developed countries, and the policies and institutions that support and stimulate technological progressivity.

Bank research in several different areas increasingly has come to recognize that choice and implementation of technologies is a much more active and creative process than sometimes presumed. A considerable amount of redesign, adaptation and learning often is involved in "technology transfer." Several recent studies have shown domestically adapted or invented technologies to be playing a significant role in growth of productivity in manufacturing industries in certain less developed countries, and to be occurring in exports. We think that the Bank should join more actively and provide

greater support for research trying to understand and better characterize the nature of the processes involved.

A number of important policy questions are at stake. For example, it would seem to be important to know the extent to which having a number of well-trained engineers in a company facilitates their choice of techniques, adaptation, and innovation. One can go on to probe regarding the kind of training that effective engineers have had, and to ask whether this is the kind of training that is offered by the country's engineering schools.

It is important to gain a better understanding of what kinds of firms are adapting and innovating most successfully. Do they tend to be small, medium size or large? Do small innovative firms tend to grow larger? Are there differences between domestically owned firms and subsidiaries of foreign corporations? Between private and public firms? We think it of high priority that the Bank begin to study these questions.

Among the important policy and institutional topics for study, examination of a set of issues relating to entrepreneurship strikes the Panel as particularly important. This is not only a field of industrial organization - including issues such as market structure, types of competition, and the supply of equity capital - but also a sociological problem concerning attitudes to entrepreneurship in society.

To summarize our recommendations about future research priorities, we think that the three broad areas described above - international trade patterns and inter-LDC trade; studies of factor market distortions, policies and institutions (comparative studies); and study of entrepreneurship and processes of adaptation and innovation - delineate the broad areas to which the Bank should be allocating more of its research resources.

If the Bank does decide to increase significantly its research efforts in certain new fields, our observations about the kinds of research that the Bank has done well or poorly in the past might be kept in mind. The projects chosen should involve a blend of analytical and policy questions. And there must be a senior researcher at the Bank knowledgeable about and interested in the research. Some of the new departures we suggest represent natural evolution of the research and interests of researchers currently at the Bank. But we believe that to design and carry out the research well, the Bank is going to need some new research talent with skills currently not well represented at the Bank. We recommend strongly that the Bank hire some first-rate researchers with experience in analyzing questions of industrial organization and technical advance. Where senior Bank researchers are moving into these newer fields, additional appointments could be of more junior researchers. But we suspect some new senior appointments would be very helpful.

To facilitate the design of some of the new projects, the Bank might consider establishing groups of consultants to discuss with Bank researchers the existing state of research in fields that the Bank is entering, to help identify promising research opportunities and methodologies. But, while such consultative groups can help the Bank get into a field, over the long run there is no substitute for strong in-house talent.

To avoid that research in the new fields that are recommended here ends up with descriptions of institutions and policies that do not lead to generalizations, we would recommend new research departures with a wide relevance, promising reproducibility of the results.

Even though we have suggested that some research areas are now mature for application, that others should perhaps be phased out (though first brought to completion in the most promising cases), and finally that other types of research should not be "moved into", it is obvious that our suggestions would require a somewhat larger research budget in the field of industry and trade. However, we believe that this would worthwhile for the Bank, considering how important it is that the Bank has the highest possible competence in the field of its activities, among which operations related to industrialization and trade are prominent. It is, we believe, the competence of the Bank, rather than its lending volume, that will count for its contribution to the economies of the less developed countries.

Against this background, it is not unreasonable to increase the number of scholars of the Bank in this field with at least a handful (approximately five), highly competent persons. This is, in fact, a prerequisite for shifting research to the areas that in our opinion should be given higher priority in the future than in the past. It will, of course, be the number of new researchers and the scale of additional resources made available to them which determines the number of new research areas that should be entered.

APPENDICES I AND II

INDIVIDUAL FIELD EVALUATIONS AND PROPOSALS

I.1 Incentive Policies and Economic Integration

Juergen Donges

The Subject

The following evaluation is based on a sample of five RPO projects: (i) RPO 670-01, Development Strategies in Semi-Industrial Countries (directed by Bela Balassa); (ii) RPO 670-22, Economies of Scale and Tariff Levels (Garry Pursell); (iii) RPO 670-87, Industrial Policies and Economic Integration in West Africa (directed by Bela Balassa); (iv) RPO 671-10, Promotion of Nontraditional Exports (staff member responsible: David Greene); (v) 671-75, International Trade Policy for the Development of Bangladesh (staff member responsible: C. Jayarajah). Four additional non-RPO projects (three by Balassa, one by Stephen Guisinger) will be referred to where appropriate. Seven out of the nine projects under review have been completed - successfully as will be shown below. RPO 671-75 is to be completed by November 1979, whereas RPO 670-22 has never produced a final report.

Most projects deal with the role of incentive systems in economic development, from both a theoretical and an empirical standpoint. They examine primarily alternative forms of import protection, export promotion and economic integration, and they analyze the effects of these measures on the allocation of resources, the balance of payments, the generation of employment, and the overall growth in developing countries. Moreover, there have been attempts to evaluate incentives on a firm-by-firm basis and to appraise the impact of protection at the level of individual investment projects.

In view of the widespread belief among policy makers in many LDCs that their economies cannot develop (i.e. industrialize) without direct government interference in the market mechanism, the focus of these projects is essentially applied. Not only could the findings, if accurately substantiated, provide the government officials of LDCs with guidelines for appraising the allocative, growth, distributional, and balance-of-payments impact of the incentive regimes, and for reforming them where necessary, but the studies could also strengthen the ability of the Bank's operational departments to assess both the economic feasibility of individual investment projects and the overall effects of the economic policies pursued in LDCs. Most of the studies under review meet these objectives.

Project Review

From both an operational and a policy-making point of view, the main contribution in the field under review is the project on Development Strategies in Semi-Industrial Countries (henceforth "Strategies"), by which the World Bank has continued and deepened its research on (trade) incentives; work in this area has been closely connected with the Bank's main policy interests since 1967. The "Strategies" project started in mid-1971 and was

completed, with a formidable volume of over 1,200 (double-spaced) pages and tables, in mid-1978. Its purpose was to quantify and analyze relative incentives provided to import substitution and export expansion in countries that have already established a relatively diversified manufacturing sector and that are pursuing different development strategies.

For the sake of comparability, the project concentrated on six countries at a similar level of industrial development: Argentina, Colombia, Israel, Korea, Singapore, and Taiwan. Argentina and Singapore are relatively new to the group of countries studied in this field, which is an important aspect in itself, because they represent the two extremes of economic policy shaping: highly interventionistic in one case, and strongly market-oriented in the other. The six country studies followed up prior methodological and empirical research done in and outside the Bank, and so did the comparative analysis of the experience of the six. On the whole, the policy conclusions and recommendations rest upon firm theoretical foundations and a sound factual basis.

The methodological framework underlying this project is the effective protection concept. It has been extended to include export subsidies as well as credit and tax preferences, in addition to import tariffs and quotas. By doing this, fortunately at a reasonable level of sector disaggregation, the "Strategies" project has increased the knowledge inside and outside the Bank about the effects of incentive regimes. This study is an example of how analytical tools, which have proved useful in earlier analyses, can be improved in theoretical and computational terms. It is particularly noteworthy that, unlike many other studies in this field, a great effort has been made to collect comparative price data. Only price comparisons between domestic production and import values make it possible to calculate overall effective protection (i.e. including non-tariff measures) and to identify the degree of tariff redundancy, which often exists.

That the analysis in the "Strategies" project has been carried out in a partial equilibrium framework does not reduce its practical relevance, provided the estimates are taken as orders of magnitude and their economic implications are interpreted in a comparative way rather than in isolation. General equilibrium models tend to have a rather limited explanatory power, let alone their high sensitivity to the parameter specifications.

What could have been wished from the user's point of view, however, is either that the country studies had been completed earlier or that the quantitative estimates had been more up to date. Earlier completion of the project - originally planned for the second half of FY73 - was outside the realm of possibility as various researchers were unexpectedly absorbed again and again by other duties in their own countries. Thus, the delay has to be interpreted as the price to be paid for embarking on large cooperative ventures amongst Bank's researchers and external economists; the "price" has been kept as low as possible, however, by circulating draft versions of various chapters well ahead of the project's completion.

With regard to an updating of the results, one should remember that the quantification of the incentive measures refers to a single year in the end-sixties. While this helps to understand the past economic performance of the countries under study and to assess cross-section indicators of comparative advantage, most sample countries have changed these measures, especially those related to the foreign sector, significantly since then. Had the "Strategies" project included these changes in the analysis, its results would have become even more significant than they are in any case. Of course, we are aware of the practical difficulties in keeping current a comprehensive analysis such as this one; it probably would have required a full-scale re-estimation of the effective rates of incentives. We nevertheless think that there is a need for the Bank, when applying this research, to bring the results up-to-date. The Bank is, as compared to academic institutions, in a favorable position to make such an effort; it has the staff and it can try to collect at least part of the required information through the economic missions that regularly review the LDCs. We were told, however, that many such missions have attempted to collect these data, but that this effort has not contributed significantly to regular updating. In such circumstances the Bank should encourage competent organizations within the countries concerned to do this work.

The message of the research on incentive schemes is that, whenever LDCs want to industrialize and think that this objective requires government assistance, they should promote rather than protect industries. This implies, for instance, that import substitution should and could be achieved without discriminating against exports, particularly nontraditional manufactured exports, as is so often the case in reality. Neutrality between production for domestic sales and for exports is in the interest of LDCs because, as the "Strategies" research shows, it will result in faster growth, more employment creation and higher levels of efficiency than when industrialization is aided only in relation to the home market.

These are conclusions that can be used immediately by the operational staff. We were told that this work has been found quite helpful, particularly so by the country economists in the Regional Departments and by the IDF and IFC staff; skepticism with regard to the usefulness of this type of research prevails within the IPD staff, which has little to do with incentive systems, however.

It should also be noted that researchers from the countries concerned have been involved in the "Strategies" project to a large extent. This has in itself created some problems, in terms of time schedules as well as diversity of contents. But it has also contributed to stimulating policy-oriented research in the countries themselves, which otherwise would not have been undertaken and, more important for Bank purpose, it has influenced policy decisions in these countries. Argentina is one case in point. The quantification, for the first time, of effective rates of protection (taking 1969 as the base year) has influenced significantly the current thinking of the government in the direction of decreasing the high average level of protection, of narrowing the dispersion of the effective rates of

protection and of encouraging, by means of financial incentives, those industries that have, or could develop, international competitiveness. Furthermore, one of the authors of the chapter on Argentina of the "Strategies" project has recently followed up the earlier findings, taking 1977 as the year for calculating effective rates of protection. We have also found for Colombia and Israel a growing amount of research resources devoted to the empirical analysis of trade policy issues, as a follow-up to the "Strategies" project. In Korea, Singapore, and Taiwan, where the authors have moved into prominent governmental positions, economic policy making takes into account the recommendations that were formulated in the "Strategies" study. Thus, this work has contributed, directly and indirectly, to promoting applied research in the Bank's member countries, which we have suggested in Chapter I as being one of the major principles for guiding Bank research.

Studies of this type are not only worthwhile because they may assist in shaping more rational incentive policies in the countries concerned. They are also important for the lessons they can provide for many other LDCs at lower stages of economic development. As one can never take for granted, however, that these lessons will be absorbed by those other countries, it is always helpful to extend the regional coverage of the research on incentives. The project on Industrial Policies and Economic Integration in West Africa was a useful extension of Bank research in this field. Its purpose was to appraise the potential of, and obstacles to, industrial development and economic growth in small, primary-producing countries under alternative policies, including expansion of intraregional trade.

Four countries were chosen for analysis: Ghana, the Ivory Coast, Mali, and Senegal. They differ from those studied in the "Strategies" project in that they are less developed and have not yet established a diversified industrial base. The incentive systems of these countries have been analyzed with regard to their impact on import-substituting industrialization within each country, increased intraregional trade, and expansion of exports to third countries; furthermore, the structure of (long-term) comparative advantages of these countries, in both agricultural and industrial activities, has been evaluated.

In measuring the incentives, basically the same methodology was applied as in the "Strategies" project. However, the structure of effective rates of protection and subsidy does not reflect accurately the comparative advantage or disadvantage of particular activities since it is calculated ex post and on the basis of market prices rather than shadow prices. For this reason, the project has rightly applied the domestic-resource-cost concept as well as estimates of the cost-benefit ratio for foreign capital in measuring comparative advantages--using, as in the calculation of incentives, firm level data.

While the findings provide a clear indication as to substantial incentive-generated distortions and a great diversity in the domestic resource costs of foreign exchange, they should be taken as reflecting orders of magnitude rather than exact quantification. As the authors themselves admit, the

data are not completely reliable in all cases and the estimation of shadow prices, particularly of the shadow exchange rate, always involves some uncertainty. The sensitivity of the ranking of activities in domestic resource costs to the shadow price assumptions has been tested in the studies on Ghana and the Ivory Coast; such alternative estimates are still to be done for Mali and Senegal.

Research undertaken in this project, as with the "Strategies" project, met the Bank's immediate operational needs. Indicative of this is the fact that the project was carried out in close cooperation with the Western Africa Regional Office and with support from the Industrial Projects Department (of CPS). The operational staff found the methodology, the empirical results, and the policy recommendation(s) useful in evaluating government policies and programs in that area. Recently, the regional office has even undertaken the initiative to follow up the research of this project by studying the effects of trade and other incentive policies on resource allocation in Cameroon (which is the dominant member of the Union Douaniere et Economique de l'Afrique Centrale). And the Agricultural and Rural Development Department (of CPS) is now sponsoring a comparative study on the economic effects of price intervention in agriculture. The West Africa research project has also influenced policy making in the countries concerned. For instance, the analysis of Senegal has led to the preparation of a tariff and tax reform; and the Nigerian Government has invited and financed a special World Bank mission to obtain guidance on the costs and benefits of a revised system of incentives, using the same methodology as in the West Africa project.

Operational guidance with regard to an Asian nonindustrial country is expected from the ongoing project on International Trade Policy for the Development of Bangladesh, which has been initiated by the South Asia Regional Office. This project may be considered as a country application of the effective protection-domestic-resource-cost methodology.

It aims at three objectives, according to the project proposal: (i) to assess the structure and functioning of factor and product markets with a view to identifying relative resource scarcities at present and in five years from now; (ii) to determine the structure of comparative advantages for existing agricultural and industrial activities as well as for new industries that might be established in the future; (iii) to design a trade policy, properly synchronized with domestic incentives measures, conducive to a removal of the balance-of-payments constraint to Bangladesh's economic development by encouraging investment in activities in which the country has, or can develop, a comparative advantage.

Doubtlessly, these are important subjects. Whether or not the analysis can be conducted at a high rigor will crucially depend on data availability. The project proposed displays much optimism in this regard, but there is a risk that difficulties in collecting the needed information can become enormous. That the execution of the project involves researchers from Bangladeshi institutions might facilitate the access to existing, though unpublished, data.

But the real problem is that the existing stock of statistical information in Bangladesh is far from being sufficient to measure shadow prices, quantify incentives, and calculate domestic resource costs. There will be great need for additional data collection by the researchers themselves. One should therefore not be surprised if the research leads to a narrower range of permissible conclusions than is expected, or if its completion will be delayed.

The internal needs of the Bank relating to incentive policies do not exhaust themselves with country reviews. As a lending institution, it should also have a great interest in knowing the degree to which both the financial and economic profitability of individual investment projects are influenced by trade policies. In this respect, the non-RPO study on Industrial Protection in IFC Projects, assigned to a research consultant, could be useful--and has in fact been considered helpful by an internal evaluation report.

Nine IFC past investment projects were selected. They are industrial in character and located in Brazil, Colombia, Iran, Nigeria, and Pakistan (one each) as well as in Ethiopia and India (two each). Basically the same conceptual framework as in the "Strategies" project referred to above was used to examine empirically the relationship between levels of protection afforded to these projects and their economic and market rates of return (related to all assets). It is emphasized that the standard measures of effective rates of protection do not provide all the information required for making decisions at the project level. Those measures are inherently static and they are sensitive to the exchange rate used to convert foreign into domestic values. Judgment about the overall financial profitability of a new investment should also take into account the pattern of protection (and other incentives) over its lifetime. This can be accomplished, in the opinion of the researcher, by means of supplementing the current IFC technique for appraising the protection aspects of new investments (the 25-percent guideline on nominal protection) with a benchmark that indicates the extent to which, in a protection environment, the market rate of return may exceed the economic rate of return without reducing the economic net benefits of the investment to an unacceptable level (a 50-percent guideline is suggested).

This is an interesting suggestion, as has been recognized as such in the corresponding offices (including the IFC itself). We have to mention, however, that the application of the approach suggested may encounter serious data problems. The "Strategies" project shows how difficult it is to obtain adequate data for making price comparisons for past periods. It might prove even more difficult to make such a comparison for a period of years in the future, as required ideally in appraising investments under systems of protection.

These caveats notwithstanding, this project is an example of research that can probably be undertaken only in an institution like the Bank with a reasonable chance of success. Researchers in academic institutions frequently

are reluctant to embark on large data collection activities, which are regarded as not adding much to professional reputation. And in a purely academic environment they may not work hard enough on the methodology of project appraisal because they do not perceive this issue as crucial as researchers closely connected with an operational staff might do. In fact, literature on project appraisal shows that much of the pioneering work has been done by senior researchers under the sponsorship of international organizations involved in this field.

In estimating the efficiency effects of trade policies, the projects reviewed hitherto were mainly concerned with the allocation of resources within the economy. But protection not only permits domestic production that would not be able to compete with imports in a free-trade situation (allocative inefficiency), but also permits poor performance in the protected industries, which would improve if necessary for survival in a competitive environment (X-inefficiency). Comparison of these types of protection-induced inefficiencies and an assessment of the relative importance of the two is proper indeed. The small project on Economies of Scale and Tariff Levels was supposed to study this subject explicitly, taking Colombia, Indonesia, and the Ivory Coast as case studies.

The research has yielded a first conceptual draft containing a priori propositions about the X-efficiency consequences of import protection. There is also a first draft available for the case study on the Ivory Coast (including five industry analyses) and, as a by-product, a case study on Iran (with three industry analyses). The project was, however, never completed: the access to data proved to be much more difficult than anticipated; in the case of Indonesia there was no progress at all. The Colombian consultants to whom the Colombian study was commissioned reportedly did not have the experience and capability for doing good work in this field, while the responsible researcher in the Bank was absorbed by other duties. In retrospect, the project, although reporting some interesting empirical findings that are not available elsewhere, must be considered as a failure.

The project on Promotion of Nontraditional Exports is, by comparison, more descriptive. Carried out in cooperation with the UN Economic Commission for Latin America, the project aimed at evaluating export expansion policies, with particular reference to Argentina, Brazil, Colombia, and Mexico. For greater comparability, the project was extended to include also the experience of non-Latin American countries, namely India, Israel, Korea, and Yugoslavia. The initiative was taken by the Latin America and the Caribbean Regional Office.

In general, the country studies have surveyed the existing stock of knowledge in this field, rather than increasing it through new empirical investigations. Their significance for policy making is, however, great. It is shown that the economic policies of developing countries have a substantial impact on the evolution of new manufactured exports, and in this regard this project has complemented the "Strategies" one. It is an example of how Bank research findings relating to incentive policies can be diffused among both

the Bank's operational staff and local researchers as well as government officials in developing countries; dissemination could have been even wider, had the results of this work been published not only in Spanish but also in English.

General Evaluation

The studies on incentive policies and economic integration which have been completed so far show five distinct features:

(a) One is the high professional quality of the work. This has allowed various authors to have already published part of the research results in highly reputable journals (see Annex II), and thereby to contribute to the effective dissemination of the findings.

(b) Second, in most of the cases the authors have made a substantial effort to improve the methodology for policy analysis and investment appraisal. The Bank's research so far has been to a large extent creative rather than imitative.

(c) Third, the applied component of the research has been generally complementary to the research in the field undertaken elsewhere (UN organizations, OECD, academic research institutions). There has been, however, some overlapping with regard to the LDCs chosen for analysis, which is perhaps a reflection of the uneven distribution of useful statistical data among LDCs.

(d) Fourth, the studies have been designed with a view to responding to important needs of the Bank's operational departments. The research was empirically oriented, which is of paramount importance if soundly based policy prescriptions are to be achieved. In fact, the operational staff is to a large extent convinced of the relevance of this research, and has expressed this by applying the findings of the research in country economic reports and policy analyses, by using the methodology for in-house quantifications of trade incentives, and by initiating additional research in this area.

(e) And, fifth, a great deal of the studies reviewed has involved participation from developing countries--in most cases relatively successfully. This is a significant achievement since it could help strengthen the role of rational choices in the political decision-making process in the countries concerned--to the benefit of their economic and social progress.

As the research in this field makes high demands for data, the Bank has proved to be an appropriate place for undertaking it, given its experience, as well as the fact that researchers from academic institutions usually do not have a comparable access to all information required. Furthermore, while it is always difficult to sell a research result to a government, the Bank was--because of its leverage--in a good position to induce governments in developing countries to draw as many benefits as possible from the flow of thinking coming from its research units. That research on incentive policies and economic integration was also undertaken by both academic research institutions and other international organizations should not be considered as an argument for not doing it at the Bank. Research inside and outside the Bank was complementary to a significant extent; the Bank exploited its comparative advantage even more by concentrating on comparative studies. And, more fundamentally, advances in policy-oriented research, including its methodological foundations, are normally greater, the greater the competition among researchers--analogous to the productivity growth in an economy resulting from active competition in product and factor markets.

With regard to the relationship between research and operational activities, one important lesson can be drawn from the studies on incentive policies and economic integration: while it may not always be possible to provide the operational staff with ready-made solutions to pressing problems, the overall research work in this field can contribute, and in fact has contributed in the past, to sensitize the country economists in the regional offices and to influence lending operations. Particularly, this work helped them to fully understand the efficiency problem at both the macro- and micro-level and to explain its implications to governments in developing countries. It is noteworthy that, until the early seventies, the Bank's economic and sector missions to developing countries used to adopt benevolent attitudes towards (protectionist) import-substitution policies, while they now advocate vigorously for fairly neutral incentive systems combined with reasonably liberalized trade regimes and realistic exchange rates. Moreover, specific investment recommendations are based now more often on comparative advantage criteria than was the case until recently.

As the effective protection-domestic-resource-cost methodology can be considered mature, and understandable to the operational staff, the past research under review has established the conditions for entering the application phase according to concrete demands. The preparation of other (comparative) studies on incentive schemes may involve, from a purely academic point of view, diminishing returns--a point also emphasized by the DPS staff. Yet many developing countries still favor policies of import substitution, maintain high levels of effective protection, and prefer quantitative interventions over pricing incentives. And the Bank will presumably continue lending to them. A regular analysis of the incentive policies pursued by member countries could increase the advisory capacity of the operational offices. Moreover, if incentive schemes are to be changed in order to make development policies in developing countries more effective in the medium and long

run, the Bank has the authority to explain this forcefully to national governments, whereas individual researchers of academic institutions, including those of the countries concerned, frequently lack this influence. This is particularly important with regard to the question of how a successful transition to a more efficient incentive scheme could be made in practice. The answer to this question, while touched upon in past work, in any case still requires a good amount of research.

Areas for Future Research

From the above it follows that Bank research for appraising incentive policies should not be totally discontinued, though shifted in emphasis.

(a) To begin with, it would be desirable to investigate in depth the existing incentive regimes with regard to factor markets, the implications of these regimes for both the functioning of product markets and the changes in the structure of trade, and the avenues of eventual reforms.

(b) Second, analyses of the existing or required overall incentive systems should be oriented towards the following groups of countries: those rich in natural resources, those that are just beginning their industrialization, and those that have still very low per-capita incomes. Such studies are of interest for assessing these countries' prospects for self-sustained economic growth and successful integration into the world economy or, by contrast, the (potential) advantages and disadvantages of pursuing the "basic needs" approach combined with a strategy of (total or selective) "delinking" from the international economic system.

(c) Third, the continuation of research on promotion and diversification of manufactured exports, with emphasis on marketing aspects and on the contributions that export processing zones can make, is of considerable importance to developing countries and to Bank policy-advisory work as well. In this connection, the relationship between the emergence of export industries in developing countries and the implementation of discriminatory protectionist measures by developed countries should be analyzed further and then kept under review. Furthermore, policy changes in developing countries with regard to export promotion and import substitution as reaction to external shocks should deserve some investigation.

(d) Fourth, and given its importance for Bank lending, research on future world trade patterns--with particular reference to changes in export structures of developing countries according to their stage of development, the size of their domestic market, their endowment with natural resources, and skill levels in their active population--should be intensified.

(e) Fifth, in view of the data intensity of research in these fields (including applications), it seems to be worthwhile to consider the establishment of a central data bank, particularly with information on world prices.

As regards research on economic integration, there are at least three subjects where further research is of importance.

(a) One would be to follow up the work on West Africa's integration referred to above by investigating the effects of the Fonds Communautaire de Developpement on investment patterns, the impact of the monetary union on the international competitiveness of industries in the countries concerned, and the consequences of the Taxe de Cooperation Regionale for the structure and intensity of intraregional trade.

(b) Second, the different avenues of economic integration among developing countries (i.e. regional versus interregional framework; mutual trade liberalization versus product specialization agreements; customs union versus free trade area) should systematically be analyzed, taking into account the factors that have contributed to the many failures of integration schemes in developing countries in the past.

(c) Third, more insights into the costs and benefits of economic integration among developing countries should be provided for operational purposes.

These are some illustrative issues that, in our view, have important applications in the Bank's advisory, financial and industrial sector work. Obviously, the various topics are linked together at many points. As staffing constraints might not allow the expansion of research into all areas outlined above at the same time, priorities among them should be established by the Bank's Research Steering Group according to actual and potential policy and operational needs with which this Group is more familiar than we are. We strongly suggest, however, that once resources have been allocated to specific lines of investigation, the completion of the work should not be jeopardized by overloading the responsible researcher with too many duties (as was frequently the case in the past). If the in-house demand for policy work increases, and if this increase is not only temporary, we recommend that the Bank enlarges its research staff accordingly.

I.2 Comparative Advantage, Trade Patterns and Economic Growth

Edmar L. Bacha

This section addresses a set of projects on general equilibrium trade models, comparative advantage structures, patterns of industrial development, and sources of industrial growth. Six RPO projects plus two non-RPO papers by Bela Balassa are reviewed, followed by a general evaluation of the field and a brief discussion of possible future extensions of the research under review.

Project Review

670-07: International Model. This project proposes to construct an international general equilibrium model for the world economy, to study problems of interest to developing countries, such as the consequences of changes in tariff rates, varying growth rates of industrial countries, and oil price increases. There are sub-models for each of the three main developing regions (Latin America, Asia and Africa), for the developed world, and for the oil producers. The model is based on the principles of general equilibrium theory, and makes extensive use of activity analysis procedures.

The model was contracted out in September 1972, with the work being carried out by Victor Ginsburgh and Jean Waelbrock, both as outside consultants (Waelbrock was at one time a Bank staff member). The project is supervised by Bela Balassa, and it is practically completed, although the simulation results are not yet available.

The reviewer had access to two papers of a methodological nature, the first describing the computational algorithm and the second presenting the general equilibrium model of world trade. These papers appear to be very innovative. According to the authors, "this seems to be the first successful computation of a general equilibrium for a large realistic model." The use of a General Equilibrium model also "forces the model builder to incorporate only theoretically significant constraints, avoiding the use of the crude heuristics which play such a large role in development planning models."

But, once the theoreticians decide to come down to the real world, they must accept its crude rule that the test of the pudding is in the eating. From this perspective, a judgment on the project must be suspended until its empirical findings are published and its results compared with those of less sophisticated world models.

670-19: Expansion in Manufacturing for Export in Developing Countries. This project proposes to analyze which industries are moving out of industrialized countries because of high wages and labor shortages, into low wage developing countries. It intends to survey export industries that have been transferred to Singapore, Taiwan, Korea (and possibly Mexico), as

well as to sample international firms in Japan, the U.S., and Europe, on their prospects for further shifts of export manufacturing operations to LDCs.

The objective is to provide developing countries with information for planning export industries. Knowledge should be added on (i) segments of industry that have already moved to developing countries, (ii) manpower and ancillary industrial requirements, (iii) dependence on foreign partners for technology and marketing, and (iv) outlook for access to foreign markets in Japan, the U.S., and Europe.

Jack Baranson was the staff member responsible for the project, for which the first contract was drawn up late in 1971. Two papers were written by outside consultants. One was on the migration of Japanese industries to Taiwan and South Korea, and the other on the offshore activities of the Japanese electronics industry. The project was cut short by Baranson's departure from the Bank.

An internal evaluation report is available on the two papers, concluding that, while they produced some useful information, they are not of good quality. Both papers are in fact very descriptive. One at least delivers its message in a readable format, while the other paper is a poorly written dossier, with interest only to research analysts in the electronics industry. The project was a failure from its inception. In view of its poor design, most probably it would have led nowhere even if Jack Baranson had stayed in the Bank. This is a pity, as the subject matter appears extremely important to inform the discussion on industrialization strategies in developing countries.

670-79: Economic Development of East and Southeast Asia. As with the previous project, the objective of this one is to identify and analyze the industries most likely to find it advantageous to relocate their processing activities from Japan to East and Southeast Asian countries.

The study was divided into two parts. The first was macroeconomic in nature, involving projections of the Japanese economy and of the structure of trade between Japan and other main regions through 1985. In the second part, selected industries such as textiles, electrical machinery, steel and miscellaneous manufactures, were studied in more detail to identify their changing comparative advantage within the Japanese economy.

The project was supervised by Parvez Hasan, and carried out by the staff of the International Development Center of Japan (I.D.C.J.). It lasted from June 1973 to August 1974, when a final draft report was completed.

This draft was not made available to the reviewer. However, a completion report by Mr. Hasan says that the draft was disappointing because it did not provide a comprehensive or cohesive view of Japan's economic relationship with East and Southeast Asia in the coming decade, which was the main objective of the study. Apparently, the Bank requested a revised version of the paper, but the I.D.C.J. was unable to provide it.

This project, although producing some useful information, can also be considered a failure. Much was promised but only a few projection exercises without much interest were delivered.

671-05: Patterns of Industrial Development. The main purpose of this project seems to be the organization of macroeconomic industry and trade data banks at the World Bank. The economic analysis is limited to updating the Chenery-Taylor regressions on patterns of industrial growth, with some marginal methodological improvements.

The project was started in early 1976 under the responsibility of Vinod Prakash of the Development Economics Department. Completion was expected by October 1978.

We had access to two papers by Vinod Prakash, with the second, on the measurement of industrial exports, being an improved version of part of the first one, on statistical indicators of industrial development. Alternative definitions of industrial exports, commonly used by U.N. agencies and economic researchers, are tested. The conclusion is that these definitions are not interchangeable as commonly believed. Hence, there is an urgent need for a standard definition of industrial exports, if the data base for country-comparative studies is to be improved. Adoption by all countries of SITC (Rev. 2) would permit such uniformization.

This project seems most useful for the World Bank's work on industry and trade. Its conclusions should be discussed with the operational staff of the Bank, since apparently some statistical pitfalls are related to specific country procedures, which country specialists may clarify. For example, the discrepancy pointed out by Prakash in industrial value added as between the National Accounts and the Industrial Censuses in Brazil is only apparent. The problem is that the Brazilian Census Bureau used the concept of "value of industrial production", which is an intermediate concept between value of production and value added.

Users' Guides to the data bank have been written, and it is hoped that the operational staff of the Bank will be using these files in their country and sector analyses.

671-32: A Comparative Study of the Sources of Industrial Growth and Structural Change. This project is designed to contribute to an empirically based theory of industrialization which Hollis Chenery and associates have been building for the last 19 years.

For each of eight countries, the sources of industrial growth and structural change are determined using input-output data. Determination of these sources starts from an accounting proposition asserting that, for any sector of economic activity, domestic production is equal to the sum of domestic intermediate and final demand plus exports minus imports.

Observed changes in sectoral domestic production through time are attributed to changes in domestic demand, exports, and import substitution, and "growth contributions" are computed for each of these demand components.

Deviations of the sectoral production levels from a proportional expansion path (where the factor of proportionality is the growth factor of domestic income or aggregate value added) are also calculated. Such deviations from homogeneous growth are designated as "structural change". Application of formulae derived from the accounting proposition above permits the calculation of the "contributions to structural change" of each of the demand components.

The emphasis of the analysis is placed on exports and import substitution. The purpose is an evaluation of import substitution and export promotion strategies from a long-term, sector-by-sector perspective, stressing questions of sequencing as well as problems of transition to a flexible, viable industrial structure.

Parallel to the country studies, simulation exercises are developed to assess the relative importance of universal and country-specific influences in industrial structure and growth. "Normal" patterns for the relative contributions of each demand component to industrial growth and structural change are estimated from cross-country regressions. These "normal" or universal patterns are assumed to vary with per capita income, population size, and factor endowment of the country. Deviations from "normality" are country specific and hopefully may be explained by the development strategy and associated government policies of the country under consideration.

The project started in early 1976 and completion was expected by mid-1979. People responsible for the project include Sherman Robinson, Yuji Kubo, Hollis Chenery, Larry Westphal and Moises Syrquin.

Only part of one country study (on Norway, by Bela Balassa), which actually antedates the research project, was made available to the reviewer. Three other papers were read, on the cross-country simulation study, authored by Chenery and Syrquin.

This project may be considered as the end-product of a twenty-year quest to establish an empirically based paradigm for modern industrial growth. It demonstrates the importance that simple ideas may have for the development of economic knowledge. Chenery's seminal 1960 paper consisted of little more than the numerical estimation of an accounting identity. However, as his papers for the Nobel Symposium and for the Tokyo IEA Congress (the latter co-authored by Syrquin) demonstrate, within a 20-year period such simple exercises made it possible to organize a considerable body of empirical knowledge regarding the nature of modern industrial growth.

It is unlikely that this project will achieve its ambitious initial aim to develop an analytical framework to articulate the connections between individual policy instruments, changes in industrial structure, and economic performance. We must wait for the final report to see how close Chenery and associates have come towards this goal. However, the papers reviewed are indicative of the high quality of the work done, including the important methodological contributions by Balassa and Syrquin to the measure of import substitution.

671-79: Sources of Growth and Productivity Change. This project is a follow-up to the previous one, with specific focus on Korea, Turkey and (possibly) Yugoslavia.

Three levels of study are contemplated: (i) linking the demand oriented analysis of the previous project to the study of sources of growth from the supply side; (ii) constructing general equilibrium models for the countries in the study, aiming at a "consistent, comparable and rigorous" analysis of different policy packages, and (iii) complementing the previous analysis with a micro-focused study of two common industrial sectors in the three countries.

The project started last summer and completion is expected by late 1980. Sherman Robinson and Kermal Dervis direct this research project, with Larry Westphal being involved in planning, and Yuji Kubo in carrying out the work.

The reviewer had access to the research proposal, which contains two appendices, one on the Turkey model and the other on the decomposition of sources of growth according to factor inputs and technical change.

The study of sources of growth from the supply side is of some interest. It is a useful way of organizing relevant economic data, which the demand-oriented view of the previous project leaves relatively untouched. However, after the methodological and empirical criticisms of this type of work by Frank Fisher and Griliches and Jorgenson, one perhaps should approach the study of production function decomposition with more modesty than is apparent in Appendix A to the research proposal.

The micro-focused analysis of two sub-sectors seems to be the most promising part of the research. Unfortunately, the proposal is somewhat unclear not only on the choice of the sectors but also on the methodological approach to be adopted.

The general equilibrium models are presented in the proposal as a superior way of looking at industrialization processes. They would overcome the partiality of the "sources of growth" studies, and provide an integrated approach to the problem of industrial expansion. It is our feeling that experience says differently. Accumulated knowledge on the development of large models seems to indicate that their main merit is as organizing devices

for the collection of relevant data for economic policy making. The important empirical results that they produce apparently can as well be obtained much more inexpensively from simple macroeconomic formulations.

In view of this experience, perhaps it is wrong to say that multi-sector models are a step ahead of demand-oriented sources of growth studies. Theory simply is not firm enough to allow a reasonably complete specification of the most relevant socio-economic interactions in the process of industrial growth. It would seem more appropriate to accept with scientific modesty the extent of our ignorance, use simple tools, and proceed bit by bit. The answer apparently lies not in implementing programming models, but in devising simple ways of approaching critical socio-economic relations, from the empirical exploration of which additional knowledge may be generated.

Non-RPO: Two Papers by Bela Balassa. The papers by Bela Balassa contain empirical tests of important predictions of orthodox trade theory. Trade barriers are presumed to reduce international trade flows and to affect the international location of production. This topic is analyzed empirically in Balassa's contribution to the Nobel Symposium.

Balassa's paper to the Tokyo IEA Congress tests the prediction that comparative advantage in trade in manufactures, as revealed by relative export shares, is affected by the capital-labor ratio of trading countries.

In devising his tests, Balassa demonstrates once again his justly praised ingenuity in empirically implementing abstract economic theories. His results are most comforting to orthodox trade theory. The Tokyo paper also serves the purpose of dispelling some of the "export pessimism" that has become fashionable since the oil crisis, when it stresses the importance of supply side factors to the success of export promotion policies for manufactured products.

A minor complaint relates to Balassa's propensity to carry his findings perhaps a bit too far. For example, on p. 24 of the IEA paper, he asserts that, "The empirical estimates show that inter-country differences in the structure of exports are in large part explained by differences in physical and human capital endowments". First, alternative theories were not tested; the empirical analysis refers to the statistical significance of the relevant coefficients for the Hecksher-Ohlin hypothesis, investigated by means of ordinary least squares. Second, the coefficients of determination of the regressions explaining export shares are not published (though we have been told that in the best regressions the coefficient reaches .68). Hence, the author can say that his results are statistically significant, but should be cautious in claiming that they explain a large part of the differences in export structures.

Balassa's "stages approach" to comparative advantage may turn out to be a useful way of bridging the gap between "static" and "dynamic" views on desirable patterns of trade specialization for LDCs. Hence, it would be

interesting to see him building a model of comparative advantage as a continuation of his work on the "stages approach" to international trade patterns.

General Evaluation

Seven projects were analyzed. One, consisting of two non-RPO papers by Balassa, was completed successfully. Two others, on the migration of industries from Japan to South-East Asia, were clear failures. A fourth one, a general equilibrium model of international trade, has not yet produced empirical results. The three others are on sources of industrial growth and structural change: one is designed to organize a data bank on relevant industry and trade magnitudes; a second has only recently started; and a third is about to conclude--but its eight country studies have not yet been made available.

A definite evaluation of the relevance and impact of the projects is not possible at this stage, as most of the final products have not yet been written. However, one may say with confidence that, excepting the two failures, all the material that was reviewed is of the best quality that one can find anywhere in the profession. One may argue with methodologies or implicit visions of the world; competence is indisputable.

There is one important lesson to be learned from the failures. The Bank research group has some difficulties in dealing with outside consultants. Misguided choices of persons apart, the trouble seems to lie in that the conceptualization of the project is done in detail within the Bank. Then, outside practitioners are expected to do high quality research on these topics, without close monitoring by the Bank people. It cannot work. Either the consultants are good enough researchers to share in the conceptualization stage, or they should be handed clearly designed tasks, to be performed under the supervision of the people responsible for the projects.

Expertise in promoting joint research ventures with outside consultants seems to be lacking. Perhaps the Ford Foundation format of first designing broad terms of reference and then submitting these to an open international competition should be tried, as a supplement to the present procedures of hiring outside consultants.

Future Directions

The studies of sources of growth on the demand side are nearing completion. Further accounting exercises of this type do not seem to be necessary. But one still would want to see additional analytical work on a framework of analysis that articulates "the connections between individual policy instruments, changes in industrial structure, and economic performance". The purpose would be to develop a kit of tools for development economics similar to that already available for short-run macroeconomic policy making. It is not clear that the Bank is better equipped than the academia to do this type of research, but the policy need is evident in the LDCs for such an analytical effort.

For reasons that were mentioned here and in Chapter II, we do not think that it would be advisable to expand the project on sources of growth from the supply side beyond its present size. But perhaps general equilibrium models will be found more useful for general policy analysis outside the scope of the panel's review.

In spite of the failures of the projects on the export prospects of labor-intensive manufactured products from LDCs, we consider that this is a promising line of research. Perhaps this is a good candidate for testing the proposed system of open international competitions for research projects, to be sponsored by the World Bank.

I.3 Export Promotion Policies in the Less Developed Countries (LDCs) and Access to Markets in the Developed Countries (DCs)

Jae-ik Kim

The Subject

One of the recent developments in international trade is that a growing number of countries, mostly middle income countries that have been recipients of Bank loans, have become exporters of manufactured goods to developed countries. Typically, these countries have moved away from import substitution policies and promoted exports of manufactures, particularly labor-intensive light industrial products. Such development strategies, however, have brought a reaction from many developed countries, which began to restrict the new exporters' access to their markets.

The twelve studies reviewed signify an important step towards an understanding of this problem, which could very well develop into a political issue gravely undermining the Bank's role as a development institution. What follows is a review of these projects. Most of them contain material that falls within the following three categories: (i) surveys of selected industries, data compilations, analyses of commodity markets or the overall environment of trade; (ii) analyses of problems concerning import restrictions by the DCs on the LDCs' manufactured goods (the reverse problem, that of obstacles to inter-LDC trade, is given little attention); and (iii) a discussion of national policies and the role of institutions in promoting trade in the LDCs.

One immediate problem is that the Bank has a very narrow definition of research, so that the projects solely in the first category above are not always considered to be research. As a result this extremely valuable work has to be conducted under exceptionally difficult conditions with a totally inadequate allocation of manpower. It is only natural that there are more problems in this area than any other.

It cannot be stressed too highly how valuable this sort of study is to policy makers in developing countries. Even countries like Korea, with relatively sophisticated research institutes, can easily learn of their relative position in world markets only through this sort of work. From the point of view of developing countries it is strongly recommended that this category of research be given a higher status in the Bank. 1/

1/ Some of the remarks made may seem harsh to the staff concerned who know that, if more time or manpower had been available, the faults could have been avoided. Where this is the case they should understand that all this criticism is essentially a commentary on the management, which deprives them of adequate resources to do their job with optimal efficiency. If the remarks given below seem critical it is also because of the very high standard of research one has come to expect from the Bank.

All but four of the projects are ongoing and only drafts and proposals were available for review. The three finished works, by Balassa, Hughes and Sharpston, were of notably higher quality, and since this report was originally drafted there have been further improvements in the other projects.

Project Review

Plesch's study of Developing Countries' Exports of Electronics and Electrical Engineering Products, which was compiled over a few months, presents a survey of the remarkable growth of electronics exports by eight newly industrializing countries (NICs) to the United States. It will be useful if it is kept up-to-date. At present production data are only given for 1973 and exports to mid-1977. In view of the recession of 1975 it is too early to judge how far the trend has been interrupted. It does provide a useful starting point for serious research, but it could have been improved as an intermediate product by additions to the policy section and by incorporating some of the observations of Sharpston.

Keesing, Plesch and Triner's unfinished survey entitled Developing Countries' Exports of Textiles and Clothing: Perspective and Policy Choices has an excellent summary, which also deals with the Multi Fiber Agreement (MFA) complexities in a lucid fashion, and advocates the desirability of a shift to LDCs of textile manufactures. In textiles and clothing, as in electronics, the dominance of a few countries is apparent. The authors raise the interesting question of windfall gains resulting from quotas but, unfortunately, because of restrictions on this type of research, can offer only "speculative inferences from unsystematic observations" concerning this aspect. As far as the policy choices section is concerned the recommended policies could have been more clearly differentiated. Policies recommended to the firms in textiles and clothing, which assume no changes in the policies of the importing countries regarding either quota or tariff, should be distinguished from policies that take into account the impact on the importing countries' policies of any expansion of exports. Also, policy choices for government have to be comprehensive, and their successful implementation requires close cooperation with the exporting countries.

Keesing and Plesch's work, Recent Trends in Manufactured and Total Exports from Developing Countries, is essentially a commentary on a set of tables and was compiled in less than a week. It is invaluable as a reference work, but has a rather limited lifetime unless updated every year. It is understood that it has subsequently been revised. Again what emerges is that eight newly industrializing countries (NICs) enjoy 76% of manufactured exports from LDCs. If more research time were available, could not more resources be given to investigating unsuccessful countries in all the above studies?

Morawetz's study (RPO 671-56) on Marketing Manufactured Exports was intended as an analysis of the institutional and non-price aspects of the growth in that trade from Colombia. In fact, in a recent seminar, Morawetz

has shown that this growth was not as remarkable as was first thought. It is not clear how useful this study will be for countries that hope to expand their clothing exports to developed markets, and it may prove that the selection of another country would have been more appropriate.

Baldwin's proposal, The Effects of Increased Imports of Manufactured Goods from Developing Countries in the United States (RPO 671-67), and Waelbroeck and Others', The Effects of Increased Imports of Manufactured Goods from Developing Countries in Europe (RPO 671-66), are investigating the market share of manufactured products from developing countries and the sources of protectionist pressures in industrialized countries. The European group also proposes to study the role of the EEC in trade restrictions in the future. These proposals hold good promise and are to be joined by a study on a similar basis of Japan and other Pacific countries, which was not offered for review.

Although much has been written about the economic impact of manufactured imports to developed countries, few studies so far have made any serious attempt to analyze the political impact of these imports. This sort of analysis should be very valuable, and should provide important insights into the political processes that transform these impacts into protectionist pressures and eventually into protectionist policy actions. Both studies began with identical hypotheses on protectionism. Protectionism and its successful reflection in government policies is believed to be a function of a dozen or so broadly defined factors, some quantifiable and others not. It is not clear from the proposals how weights will be assigned to each of these multiple factors. In the case of the American study, Baldwin originally proposed to measure the market penetration of some 400 items, classified on a 4-digit SITC basis for a time span of eight to ten years. The history of the protectionist trend and its politico-economic causes will be studied, while both projects will provide empirical analyses of the structure of protectionism.

It is not possible to review these projects satisfactorily since they have so far produced no concrete results. A recent paper by Balassa entitled "The Market Penetration Studies: Approach and Methodology" sets out standards for methodology and comparability, which suggests that, after the usual problems where a number of outside consultants are involved, the Bank is pulling these projects into a clear and thoughtful shape. Recent reports suggest that close cooperation with the EEC will in the end produce some very useful results.

Export Incentives in Developing Countries (RPO 671-35), under the supervision of Balassa, evaluates the export promotion efforts of four developing countries, with a comparative framework using a cross-section investigation of major export products, and a time-series analysis of the effects of export-promotion measures. The project is a major effort in data collection and is designed to yield practical information for other countries that contemplate the introduction of a system of incentives for export promotion

or a reform of the incentive system. This project is of the greatest relevance to operational staff and to policy makers. It is precisely the sort of work the Bank should be doing. Its grasp of methodology at the initial stages is much clearer and appropriate than either Waelbroeck or Baldwin at a comparable stage, and its potential pay-off for developing countries may even be higher.

Seen from the same perspective, Keesing and Wortzel's proposal, Key Institutions and the Expansion of Manufactured Exports (RPO 671-68), is expected to make an equally important contribution. Its purpose is clear, its frame of investigation concrete, and its theoretical and methodological foundations sound. As any export promoter knows, marketing has been the most serious bottleneck for export expansion. Particularly for the inexperienced exporter, who usually takes low prices for granted and bases his export projections on them, a marketing bottleneck is a frustrating, and frequently invisible, roadblock. Keesing's research will therefore fill a large gap in existing information and be a valuable educational tool for exporters of consumer goods to the US market.

This project explains the relevance of a number of the surveys previously reviewed. It frankly admits many of the criticisms a reviewer might make of those other projects (p. 2) in that "in predicting or projecting manufactured exports, areas of uncertainty (and plain ignorance) are also evident in World Bank operational work on manufactured exports, and in the work of the team proposing this research".

Sharpston's study of International Sub-contracting is an excellent piece originally printed outside the Bank. Although presumably written in 1974 the quality is such that the analysis is still of great value. He answers questions raised by other writers, or suggests lines of approach that have not been taken up. The check list of tasks suitable for sub-contracting is one of the most valuable short pieces of analysis in this field. Perhaps the mixed background, part academic, part World Bank, provides the perfect balance.

On the other hand, Wall's Export Promotion and Preferences: A Case Study of India (RPO 670-21) used an inadequate methodology to attain the purpose of the project. It failed to identify the system of policy measures that would be desirable and sufficient in India to enable entrepreneurs to respond to new opportunities in foreign markets, and was never published. Nevertheless, in terms of results it was not a total failure. It contributed to a series of Bank reports on India that were influential, at the time, in changing India's policy on exports and export incentives. The critical tone of the report itself prevented it from being published in India, but although opinion within the Bank is divided, it is understood that it has been influential in other ways, not least through training local researchers who have subsequently gone on to assist in policy formulation. This suggests that even where the end product is faulty, the conducting of extensive research with the assistance of local scholars may still serve a useful purpose.

The editor, Helen Hughes, and authors who contributed to Industrialization and Trade Policies for the 1970's (RPO 670-20) made an important input to documenting recent growth in manufactured exports from the LDCs and its impact on industrialized countries. Systematic and comprehensive studies of the effects of increased manufactured exports from low-income countries on employment, social policies, and investment in the developed countries were long overdue. Without effective policy measures on adjustment assistance in developed countries, economic growth in many low-income countries will have to slow down, causing a chain reaction. This work, which contains important statistical information based on a thorough analysis of primary data, has also stimulated other studies.

Balassa's recent paper, World Trade and the International Economy: Trends, Prospects and Policies (May 1978), is a timely study, valuable to the entire development economics community. It compares trade liberalization and economic growth prior to the oil crisis with trade barriers adopted during the post-crisis recession. Recent non-tariff restrictions, government aids to industry, international cartels, and market sharing in major developed countries are documented and their effects evaluated. Concluding that the risks of new protectionism are high, Balassa proposes policies for long-term growth, structural adjustment assistance, and an international code of good conduct. Semi-industrial developing countries were also advised to reduce existing protection, to upgrade and diversify their exports, and to abandon gradually the export of simple, unskilled-labor-intensive manufactures for the benefit of countries at lower levels of development, a course of action that some developing countries in Asia have already set out to follow.

Overall Evaluation

In the past, Bank-financed research has been well respected for the quality of information on specific problems or situations. The country reports and industry surveys have filled large gaps in existing knowledge that other research was not able to fill because of its distance from the scene. A continuation of this "division of labor" is still a good arrangement. In research, the Bank's comparative advantage lies in its proximity to ongoing development problems. It has a unique global network of information, and Bank researchers, whether in-house or commissioned, are well received by most member governments.

One would therefore have expected the Bank to have evolved a satisfactory system of rapid data collection and assimilation. But if we consider the three categories of work--data compilations and analysis; problems concerning import restrictions; and national incentive policies and the role of institutions--it is with regard to papers solely falling within the first category that the Research Department is severely constrained through lack of staff, and the Bank's narrow definition of research. Apparently the lack of staff has led to the virtual abandonment of the practice of providing information and assistance to the operational staff, thus creating an unnecessary and undesirable gap between research and operational staff. Even more important

than this, there is a vital need throughout the developing world for an in-depth analysis of what is happening in trade on a global scale, and if the Bank does not provide the necessary resources to tackle this area, it will be doing a disservice to its member countries.

Turning to category two, the question of restrictions on trade by developed countries, it is not clear whether this lies within the Bank's comparative advantage. However, it has to be recognized that these restrictions could do more to upset the evolution of LDC's economies than any other action. The effect of these restrictions is to tend to undermine the relevance of both the brilliant general theory of comparative advantage stages of development and specific studies on the income elasticity of manufactured exports from developing countries. The fundamental causes of protectionism need to be better understood and developing countries and various lending agencies that must predict the expected return on investment in manufacturing for export need to tackle this problem. The next stage would be to analyze which forms of protectionism have the most serious and detrimental effects on LDCs.

Turning to category three, it is here that the Bank has a clear comparative advantage. The relevance and application of the results of such research to the Bank and policy makers are clear and there is little doubt that only the Bank could pursue this line. It is noticeable that the methodology and general approach are much clearer in the proposal of Balassa (RPO 671-35) and that of Keesing and Wortzel (RPO 671-68) than in the others submitted for review. One hopes that the general surveys in category one will be kept updated in the interest of these projects, which they are clearly intended to service.

Possible Directions of Future Research

Judging by the project descriptions, more research could be commissioned on several new areas that so far seem to have escaped professional scrutiny. Some of these proposals technically fall outside the Bank's definition of research. The Bank should probably reconsider its rigid boundaries and whether they really lead to optimal efficiency. Restrictions on market access for manufactured exports from developing countries, which are expected to intensify, will increase the need to monitor protectionism in industrialized countries and their adjustment policies. The same trend will encourage further research on manufactured trade between developing countries. In connection with this, the Bank might initiate a series of research projects on the feasibility of industrial cooperation between developed countries and NICs with an eye on positive adjustment through intra-industry specialization. At the same time, as suggested above, an analysis of which forms of protectionism have the most serious and detrimental effects on LDCs would be most useful.

With regard to export-promotion policies, considering the importance of socio-cultural factors that affect a country's entrepreneurship and that vary a great deal from one country to another, it is essential for each country to identify in advance major inhibiting factors that might otherwise render the transplanting of promotion policies ineffective, and then devise methods that will circumvent them. Furthermore, LDCs, whether NICs or not, may have to adopt a new perspective on access to markets. The Bank can assist LDCs by launching projects on trade restrictions by NICs against each other. The trade potential of developing countries, in general, needs to be assessed, not on the basis of past performance records, but on their future possibilities, before the trade barriers start to arise between developing countries.

I.4 Small Enterprises, Credit Markets, and Public Enterprises

Gerardo Bueno

The Subject

The following discussion treats each of the above subjects in a separate manner, although it could be argued that they are, in some sense, interrelated. It rests on the analysis of five RPO projects and two non-RPO projects, which are mentioned throughout Appendix I, as well as in other relevant documents.

The themes are important for the Bank from an operational angle, while the research will be valuable for both the Bank and other potential users provided it is of good quality and that it provides the relevant insights. This appears, in general, to be the case. Small- and medium-scale enterprises, public enterprises and credit markets are subjects that appear in the fora of discussion, particularly within the LDCs, where it seems important to have a better understanding of the problems involved for policy formulation. Progress in research in these areas by the Bank will allow it to increase its capacity to give meaningful and relevant policy recommendations.

Still, in evaluating the research done in these areas, at least two points should be kept in mind. First, that they are relatively "new" areas of Bank research. Therefore the Bank, as yet, has neither the experience nor the influence on policy recommendations that it has gained in other research. Second, many of the projects have not been finished and, indeed, in some cases, they are still at the conceptual stage.

They are, however, very promising areas for relevant research on which, we think, it will be necessary for the Bank to consider an intensification of its efforts.

Small Enterprises

Research on small enterprises has been to a large extent the result of a growing interest in the Bank in these questions at the policy formulation and the operative levels. Not only does one find a sectoral policy paper (Employment and Development of Small Enterprises); the subject is also frequently referred to in the latest Bank annual reports and in the President's addresses to the Board of Governors. 1/ Accordingly, and although the subject

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1/ The Bank's position in relation to these questions is summarized in its latest annual report: "The Bank has found that the benefits of growth cannot be assumed to 'trickle' down automatically; to ensure that development benefits the poorest it must be deliberately directed to the poorest--(thus)--financing for industrial development (should) shift towards small scale enterprises."

does not fall squarely within the Bank's traditional lines of research, the decision was taken to support it. Thus, it appears to be one instance where the need for operative and policy formulations determined to a large extent the opening of new lines of research.

In other places, too, the subject is very much in vogue on account of the relationships that would appear to exist between small-scale enterprises' (SSEs) development and the growth of employment. SSEs have become fashionable not only in international and regional organizations but also on the national level in both developed and developing countries. However, it can validly be affirmed that up to now this interest has manifested itself mainly through the formulation of programs to support SSEs and in the adoption of several specific measures to that end; but little has so far been done in terms of policy formulation in the context of a global strategy. In short, the emphasis has been placed more on action than on reflection. Research, on the other hand, has tended to be descriptive and of a general character as well as repetitive.

The approach taken by the Bank is more original and appears to be more promising. To a large extent it is in accordance with the view that regards development as a dynamic process involving innovation, imitation, learning and competition in a Schumpeterian sense rather than as a movement towards competitive equilibrium. The possibility of falling into some pitfalls in following this approach is by no means negligible; but nevertheless it is the right approach, and what may be suggested here is that the Bank should be more explicit about it.

Research by the Bank in this area started more intensively with the RPO project 670-77 (Financing of Small-Scale Industries); in addition, there were two other non-RPO projects on Brazil and one on Malaysia. The two non-RPO projects on Brazil are econometric research of good quality, the main conclusions of which were that the SSEs sector had lost in relative importance and that new SSEs had tended to concentrate on the growth poles of Southeast Brazil. Therefore, they recommended (something that the Brazilians were already doing) the formulation of support programs for SSEs giving special attention to geographical distribution. The conclusions, as can be seen, were not particularly different from those reached in similar studies.

The follow-up was RPO project 671-59 (Small-Scale Enterprise Development). The formulation of the project owes much to Ian Little, Dennis Anderson and Larry Westphal, as well as, at a later stage, to Mariluz Cortes and Dipak Mazumdar, and to their dissatisfaction with the type of research that was being done, particularly with the absence of ways and means that would allow the evaluation of different policy alternatives. The project has two main objectives: on one hand, a review of the existing information and literature on small enterprises in developing countries and, through surveys, an attempt to define ways in which the information could be improved and certain policy recommendations be formulated. On the other, the second objective, which should be the most interesting in the medium term, is to

develop a basis for assessing the impact on income and employment of various policy options and an attempt to estimate the costs and benefits of various policies for small enterprises.

The first phase of the project includes a review of the literature as well as case studies and enterprise surveys. The studies will cover India, Colombia, the Philippines, Japan, Korea, Taiwan and Nigeria, and the enterprise surveys India and Colombia. The review stage of the project is not confined to a useful revision of the literature and programs of support for small and medium-sized enterprises; in fact, the main aim will be to examine the role they play within the overall development strategies of the countries. This, it is hoped, will permit the development of a series of policy recommendations.

The Bank's approach departs in a useful and constructive manner from the more general, descriptive approach followed in most countries and by international organizations. This being so, it would have been beneficial to extend the work to include more countries, especially given the fact that in many countries there is an increasing interest in the subject. However, this has not been possible because of staff and budget restraints. The work carried out so far in spite of these limitations has been, indeed, quite substantial.

RPO 671-59 was started in November 1977 and the first phase is expected to be completed by September 1979. Three papers have already been completed, two of which touch mainly upon methodological questions, and the third one presents in draft form the preliminary results of the small- and medium-scale enterprises survey of Colombia.

On the methodological side, the first two papers cover, respectively, the problems that SSEs face in the credit and financial markets and in regard to technological development. The first one has made use, though to a limited extent, of research carried out elsewhere in the Bank on credit market imperfections, and the second, of some of the work done in the area of capital-labor substitution. Both of them present suggested approaches, more particularly in relation to the distortions in LDC economies, which SSEs must face.

A lot of work remains to be done, and it is difficult at this stage to provide an evaluation of the whole project. Nonetheless, some insights into the possible outcome are provided by the draft paper on SSEs in Colombia for three sectors (producers of pumps and compressors, producers of agricultural implements and producers of cookers and ovens), although the authors emphasize again and again the partial and preliminary character of the results therein presented. But, however preliminary they may be, they show that it is not only a valid subject of research for the Bank, but also a worthwhile one on which to intensify effort. The survey presents a good discussion of the main characteristics of the SSEs, the origin of the firms and the entrepreneurs, their technological adaptations, their problems vis-a-vis the labor and financial markets, etc. But this said, it also shows very clearly that

there are at least three areas where it appears that further thought is required. The first one is factor productivity, especially in regard to the different combinations of capital and labor; the second is the overall question of technology selection, utilization and adaptation, as well as the learning process; and finally, the overall effect of economic policies on the development of SSEs. The first two are frequently mentioned in the Colombian paper and indeed, it is expected that they will be dealt with in the definitive version of the study. The basis for the first point will be a technology model for which data about factor productivity were collected at the shopfloor level. The third point is an important one, and it is to be expected that at a later stage its significance will be more explicitly presented once research in this third area has progressed. It must also be taken into consideration, just as is mentioned in other Bank papers, that many of these distortions that affect SSEs have their origin in the economic policies followed by many of these countries. (And it is clear that there is a role for the Bank to play in ascribing the responsibility for many of these distortions to where it ~~is~~ rightly belongs.)

Lastly, in this section we will mention some of the problems that have surfaced during the actual carrying out of the research. Here, however, only general impressions will be put forward, since it is difficult to be more precise given the lack of more detailed information. Amongst these impressions, perhaps the most important is the absence of some basic methodology, such as was provided, for example, in the case of the incentive studies and others. Although it can be argued that the development of just such a methodology is the result of an iterative process, it is important that this is done in order to attain the fullest possible comparability of the results of the different studies. Another and perhaps more relevant observation at this stage, is that the number of participating researchers seems to be relatively small, especially in comparison with the aims of the project.

Credit Markets

In this area we find two RPOs, one of them (671-65) on Small Enterprise Financing, and the other (671-69) on Capital Market Imperfections and Economic Development, both of them under the responsibility of the Public and Private Finance Division. The discussion that follows also took into consideration a paper prepared by this Division (Domestic Finance Studies No. 43) presenting its research program. It must be pointed out that some of the papers provided do not attain the quality levels that prevail in other areas of Bank research.

RPO 671-65 has not, as yet, any relevant paper. We have, therefore, only a research proposal to study the role of informal credit markets. The project is justified on the basis of the importance of those informal credit mechanisms for financing small enterprises and trade in various Asian and African countries. It is suggested that the studies could serve to ascertain whether the real cost of lending in this market is significantly lower than in the formal financial market and if it is possible to establish some sort of link between the two.

A pilot study has been initiated that focuses on "a significant element of the informal credit markets in India, namely the Shroffis". (The Shroffis are indigenous style bankers who extend and create credit through the issue of hundis as well as other credit instruments). With due regard to the focus of the study, certain questions arise in connection with this project for which adequate answers are so far missing, perhaps because of lack of time to delve more thoroughly into these matters. Nevertheless, it is useful to state at least a few of the questions. One of them has to do with the fact that we are left in the dark as to the significance of these mechanisms with respect to financing and allocation of resources; the other would concern the significance of these questions for the normative aspects of the Bank's operations. Without meaning to cast doubts on the validity of this research proposal - which in fact did not receive overwhelming support in the Bank - the point that we would like to make here is that special care should be exercised in formulating these "new" fields of research in the Bank; otherwise they may be rapidly abandoned in favor of more "proved" lines.

RPO 671-69 on capital market imperfections and economy developments owes much to the formulations of V.V. Bhatt and to previous work done in the Division itself. The project was initiated in January 1978 and it is expected that the final report will be finished in 1979. Various papers have already been produced and published (see Appendix II).

In the development of the project, three types of interests are clearly discernible. The first one is to examine the impact of transaction costs on lending as well as on borrowing in different markets. This is a subject in which there are obvious gaps in the literature and particularly in relation to the financial systems of LDCs. The second relates to the possible effects of banking regulations in LDCs on the banking structure, and to the long-run financial viability of the banks. The third one concerns the need to have a model that would express the possible interaction between the development of capital markets and general economic development. These will be briefly examined in reverse order.

The third subject of research is covered by two papers, one of which presents a general model and the other attempts an empirical verification of some of the issues raised in the first one; but the latter, in fact, is more than just an extension of the former. The aim of both papers is to better understand the role that financial intermediation plays in the development process as well as to identify the factors that limit its effectiveness and the benefits that may accrue from improving it. This was justified by the valid observation that economic analysis in the past has generated "neither a body of reasonably rigorous theory nor a wealth of credible economic results".

The central argument of the model, which does not as yet seem adequately proven, is that for a given production function and state of non-financial technology there is a role to be played by financial intermediaries channeling resources from low to high return sectors, thereby raising the aggregate marginal productivity of capital. The limits on this

role have been defined at one end by the overall rate of return that would prevail in the absence of financial institutions and, at the other, by the rate that would prevail when capital markets are perfect.

Together with the presentation of the model, the papers include a relatively thorough review of the existing literature and, as mentioned above, an empirical verification of a limited version of the model. However, it is obvious that further work will have to be done. One obvious need concerns the nature of the data and also further examination of the results provided by the model. A possible approach could be to choose countries not only on the basis of geographical considerations but also on account of their level of development and the structure of their economies.

Another paper examines the relationship between earnings and portfolio of the commercial banks of three Asian countries: The Philippines, Thailand and Singapore. The technique used was that of statistical cost accounting and followed, with some variations, the methodology previously used by Hester and Zoellner. As in other cases the research was also justified on the grounds that analyses of this type are scant in developing countries.

The investigation is well conducted and the conclusions are interesting in that both the rate of return on loans and administrative costs seem to be positively correlated with the degree of regulation in banking operations. The lowest figures relate to Singapore, which, of the countries examined, is the one that presents the most competitive financial market and is the least regulated. This is an analysis that undoubtedly can be further refined and extended to a larger number of countries, although we should mention that data problems in several cases will prove to be quite significant.

The paper on the relations between interest rate and transaction costs is an interesting one and developed with technical competence. It raises a valid point in calling attention to the importance of transaction costs for the borrower and the lender and thus, in a certain sense, to the need to innovate in the process of financial intermediation. However, the significance of this factor alone is not clear yet. The study previously referred to on banking in The Philippines, Thailand and Singapore estimated the administrative costs of lending on the average at 0.1-0.5 percent, 1.4-2.0 percent and 0.77 percent, respectively. It seems, therefore, that there are other more important factors to be taken into consideration than only the size of the borrower.

Public Enterprises

RPO project 671-11 (Managerial Structures and Practices: Public Manufacturing Enterprises) is up to now the only research project by the Bank in this area. It was started in March 1978 and it is expected that the final report will be submitted in January 1980. In a sense, it is being considered as an extension of some previous work and in particular of reconnaissance missions that were previously made to Egypt, India and Yugoslavia. As yet, the only report available is a case study of a public enterprise: India's Swaraj Tractor.

The research proposes to examine a set of hypotheses of which it is said, "are best described by the general proposition that the performance of public manufacturing enterprises is dependent upon their organizational and managerial structures and practices as well as the policy environment within which they operate". It is justified on the basis that a large part of the lending operations of the Bank concerns public enterprises and that they are a growing sector in LDCs industries. In addition, they are a type of enterprise about which not much is known and where in general, little research has been done.

As mentioned before, the only report available up to now is on the Swaraj tractor plant in India. The report is interesting, particularly where it shows the significance of indigenous technical developments and adaptations in the shaping of and the improvements in the competitive position of this plant. Nevertheless, it does not allow one either to evaluate the overall approach taken by the Bank on this question or to arrive at definite conclusions.

In view of this it might appear convenient for the management of the Bank to give more reflection to the research work done in this area. The area itself is of prime importance for the Bank from the points of view of (i) allocation of resources, since a large part of its loans goes to public enterprises and (ii) its advisory role to LDCs. In the absence of a coherent body of thought on these questions there is a great risk for dispersion of the research efforts. This would be unfortunate since it appears to us that on account of its experience and the insights that could be provided by research executed in other areas, it is difficult to think, worldwide, of any institution other than the Bank that could do it.

General Evaluation

It is quite clear that the subjects pertaining to small enterprises, credit markets, and public enterprise, have great relevance for Bank operations and for LDCs. However, it is also clear that here we have a case where relevance is not matched by the research effort actually carried out.

There are several factors that could account for this situation. One of them, perhaps, is the fact that in the past the Bank thought that the best way to approach these problems was on an ad hoc basis through the operational units and, thus, that there was no need to support their actions with a research basis. This is what would account for the additional fact that all these areas of research are relatively "new" within the Bank. Indeed, one could go even further and argue that there was some sort of resistance towards including these and other types of institutional research within the body of research carried out in the Bank.

Even now, one can discern the prevalence of two types of attitudes. On one hand, there is the attitude of considering research on institutional questions as "interesting" but devoid of academic rigorousness and, on the other, the attitude towards institutional matters of some researchers who tend to consider other research by the Bank as frequently too academic and of

little relevance either for the Bank or, worse, for the real world. This is by no means a unique situation in research institutions, but there is no doubt that, as has been proven by the Bank itself, there is a lot to be gained through a better coordination of efforts.

All of this, obviously, is not something that can be attained overnight. But it is probable that a large part of the problem could be solved if the Bank could attract high caliber researchers with solid reputations. It has done successfully in the case of small- and medium-scale enterprises and there is no reason why in credit markets and public enterprises it should not also have the kind of intellectual leadership that prevails in other areas of research. Such leadership would provide the Bank with a better analytical framework to enhance the coherence and sense of direction of research as well as to establish a better working relationship with researchers in other parts of the Bank and elsewhere. In the absence of this, we think that there may indeed be a great risk of either a dispersion of efforts, or of conducting research that could be of little relevance for the Bank and for policy formulation.

One other aspect that we would also like to touch upon is the question of how normative this type of research should be. In our opinion it ought to be high. Through its research the Bank should be able to discern which types of policies and programs for development are successful and under what conditions, and likewise which are unsuccessful and why. This is a frequently mentioned objective in the research proposals, but does not come across clearly enough in many of the papers under review. Many of the RPO projects are still at a preliminary stage and therefore it may be difficult, at this point, to extract this type of conclusion. Nevertheless, this is an area where the Bank would be utilizing, to the fullest, one of its comparative advantages in research vis-a-vis other institutions.

To conclude this section, we will touch briefly upon the question of basic research and on the degree to which research by the Bank has duplicated that of other institutions. With regard to the first part of the question, the answer is that there has been some basic research, especially in the area of small enterprises and on the role of financial development in the development process. But our recommendation here would be in favor of more, rather than less, basic research. There are a number of aspects on which the existing knowledge is imprecise, especially when dynamic factors are taken into consideration. As to the second part of the question, it simply does not appear that the Bank has duplicated work by other institutions. In practically all RPOs a point has been made to undertake overall reviews of the literature on the different subjects. This has assured, to a certain extent, that research duplication is avoided. To be more precise, in many cases the research done by the Bank on these topics has been original and pathbreaking. This is the case, for example, in research on SSEs, and there are obvious possibilities of doing likewise in connection with public enterprises.

As to the impact of research on LDCs, the questions are somewhat more difficult to answer. We would like to start by pointing out that in all RPOs under review there is participation by researchers from LDCs, though in some cases more than in others. As to the influence that this may have upon LDCs' policy formulation and relations with the Bank, it is difficult at this time to define what their precise character will be in the future. But the Bank has alredy quite substantial experience in this regard and we think that in this case it will not differ from former experience; i.e., that if there is substantial interest in the problem under consideration, many of the recommendations filter through to the policy decision-making bodies. There is in many countries a real need to know more and to receive better advice on most of the subjects covered by this chapter. And this holds true for the Bank itself. We were impressed in our conversations with Bank staff and in reading Bank memoranda by the difficulties that many Bank missions have in answering questions that are frequently posed to them by government officials, such as what would be a sensible policy vis-a-vis public enterprises in SSE-development programs, and for which, up to now, only very vague or general answers could be provided.

We come now to the question of allocation of research resources. Here we can make only two suggestions. First, within the area itself, our impression is that it would have been preferable to concentrate more resources on SSEs and public enterprises than on SSEs and credit markets, as has been the case. To us the former relationship appears to be more significant from the point of view of the development process, policy formulation and Bank operations. In studies of SSEs one cannot afford to ignore the fact that small enterprises often face discrimination in the capital markets. Second, and perhaps more important, there is the problematic issue of the financial resources that have been allocated for research in these areas. There is a need to increase them and, in particular, to obtain the concourse and collaboration of highly qualified people in all cases, but more particularly in that of credit markets and public enterprises.

Recommendations Regarding Further Research

The work done by the Bank on capital utilization, capital labor substitution, SSEs and on technical change itself has provided evidence of the need to undertake more thorough studies of the role that technical change plays in economic development. This, by the way, coincides very much with a growing preoccupation in many LDCs with the relation between technological development and overall economic development. This preoccupation has to do with a number of factors. In the past, for example, it had much to do with the question of negotiating international transfers of technology, but nowadays it is clear that this is only one of the aspects involved and that perhaps more important are the aspects concerning the selection, adaptation and absorption of technology.

It appears that the area of public enterprise has not been sufficiently studied in the Bank or, for that matter, anywhere else. The recommendation was made above for the Bank to attempt what would amount to a fresh

approach to research in this area. There are many questions here for which answers are, at best, only fragmentary. Amongst them, we should like to point out the relationships between public enterprises and the public sector, or with the private sector of the economy, or their role in technological development, etc. It is, to a certain extent, an area not fully explored, though its importance for economic policy formulations and for the Bank's operations is well recognized.

I:5 Capital Utilization, Capital Labor Substitution, and Technological Change

Richard Nelson

The Subject

The body of bank research reviewed here is concerned with the connected topics of the range of choice of technique, factors influencing the technique chosen, efficiency in the use of a technique, adaptation of techniques, and technical progress in industry.

The policy thrust of the research has been provided by the belief that in less developed countries labor is cheap and capital expensive relative to developed ones, that this ought to be reflected in the use of more labor intensive techniques, but that while this was occurring to some degree it was desirable that the techniques employed be yet more frugal on the use of capital. Throughout, the research has been concerned with market and other forces that explain the prevailing situation, and with policies that could improve the environment so that choice of technique would be made more appropriately, techniques used more efficiently, and adaptation and learning proceed more effectively. The analysis has been concerned both with contexts in which decision making with respect to technique is made largely by private profit-oriented entrepreneurs, and with situations in which the government is the decision maker or has a great deal of direct control over private decisions. The specific topics studied have evolved over the years as work done under one project generated insights and puzzles that influenced work under a subsequent project. The overall impression is that of a cumulative research program that not only makes a significant contribution to thinking in the Bank about industrialization problems, but also influences thinking within the development economics community.

Project Review

The specific RPO projects reviewed here are 670-23, 670-25, 670-54, 670-95, and 671-51. Three of these projects now are completed, and have received earlier reviews. The Bank research on industrial capacity utilization (670-25 and 670-95), both projects now completed, was concerned with assessing empirically casual observations that despite the scarcity of capital in less developed countries, the existing capital was not being used very intensively. If the proposition held up under such scrutiny, the project aimed to contribute to understanding the phenomena and to finding ways to improve the situation. The basic ideas originated outside of the Bank, and scholars had been working on the problem before the Bank got seriously into the study of capacity utilization. But the Bank work was of considerable scope, and contributed to the growing appreciation that indeed in many cases capital was not being worked very intensively in LDC industry. Bank research identified a number of factors, associated with imperfect input markets and various legal and institutional restrictions, which seemed to lie behind the non-economical use of plant and equipment. On the whole, however, not much was added to existing knowledge in this field. Project 670-95, which was completed largely by or

under the close supervision of bank employees, was more successful than 670-25, which was farmed out to a consultant and which suffers from some methodological weaknesses. Neither of these two projects seemed to have influenced Bank operations.

The project on employment and capital labor substitution (670-54), now also completed, was related to the work on capacity utilization in that this project also was concerned with examining ways in which capital could be used more frugally. The research under the project did not meet the stated objectives and treated important questions improperly. However, the questions considered under the project still are of prime importance to the Bank, and their exploration continues under another project, 670-23.

The two major RPO projects currently under way at the Bank are the above mentioned 670-23 "Scope for Capital-Labor Substitution in the Mechanical Engineering Industry", and 671-51, Appropriate Industrial Technology. The former project has been under way for much longer than the latter, more resources have been invested in it, and the output is much more impressive.

The research task taken by the current project on capital labor substitution is to explore in microeconomic detail the nature of the production processes involved in particular areas of manufacturing, and to examine the range of capital-labor substitution available in each. The project has had to face methodological, empirical, and theoretical questions of considerable difficulty. The struggle has been valiant, and the project has provided an analysis of production processes and the range of choice that is more detailed and more sophisticated than other work probing similar questions. Other studies under the project have explored the choices of technique actually made by firms, and the market and other institutional factors that have influenced those choices (which in a number of cases have been much more capital intensive than would make sense from an economist's point of view). The project appears to be having considerable impact within the Bank, and while academics tend to lag in the development of their appreciation of work of this sort, the importance of the project is beginning to be recognized by the academic development economics community.

The project on Appropriate Industrial Technology is behind schedule. The project has two thrusts: measuring the gains to less developed countries from adopting more appropriate technologies, and examining the capital goods sector in less developed countries as a possible major determinant of the availability of appropriate technologies. A few illustrative numbers have been put together on the first issue, and some literature review, casual empiricism, and thought have been directed at the second. The work, particularly on the domestic capital goods industries, shows promise, but it is too early to tell how successful the endeavor will be.

Projects 671-51 and 670-23 complement and support each other. The relevance to good policy making of better understanding of choice, or (more sharply) understanding why more capital-using techniques are chosen when less capital-using ones are available, is obvious. The more recent reports on both projects reveal the authors' growing concern about innovation, as contrasted

with choice among "obvious and available" alternatives, and a complementary interest in institutional structure. These intellectual developments will be discussed later.

General Evaluation

The work reviewed above is of variable quality and merits a high rating only in the case of RPO 670-23. The worst project was RPO 670-54, whereas the others occupy a middle category. The better pieces of work have no doubt contributed, and are contributing, to knowledge. In this regard, the Bank has exploited its comparative advantage, has avoided duplicating work that was being done elsewhere, and has proceeded in good awareness of that work. This research certainly has influenced the thinking within at least some parts of the Bank regarding the importance of the capital-labor substitution problem and the scope of possible substitution. In some instances, researchers have been involved in operational support missions to countries such as Argentina and Korea, evaluating technology issues. At the present time efforts are being made to implement this appreciation in project decision making. The work seems to have influenced thinking regarding both the importance of having the right prices, and the importance of having institutions that channel funds and information effectively. Particularly the empirical work has involved researchers in less developed countries. By and large, the work has been led by people full-time at the Bank. Where outsiders or part-time consultants led, the results were much less satisfactory.

Recommendations Regarding Further Research

We think that Bank research exploring and documenting the range of possible capital-labor substitutions in particular technologies is now running into diminishing returns. The general point that there is a wide range of choice is now well substantiated. The techniques involved in identifying the relevant substitution possibilities in particular technologies may now be ready for practical application. We note that practical application almost certainly will involve less detailed breaking down of processes than was relevant when the endeavor was viewed as a research study. We think this research endeavor should be phased down and effort placed in helping the operative divisions and departments to be able to do the relevant analyses themselves.

The project on appropriate technology is being conducted at a modest level, and in an exploratory manner. We think that particularly the part of the project exploring the design capabilities of domestic capital goods producers has considerable promise. Research within the Bank, and elsewhere, is revealing that there does indeed exist this design capability within the capital goods industry of at least some countries. This project is a serious candidate for more resources.

The Bank's research under the current two projects increasingly is highlighting that effective capital-labor substitution, the adoption of appropriate technology more generally, and over the long run the development of an efficient industrial sector, are not adequately described merely in terms of

"picking things off the shelf". The research has documented the wide range of catalog items available. But it also has documented that it is no trivial matter for a firm to be aware of more than a small portion of the range of choice. Increasingly the research is showing that at the least adaptation and in many cases innovation is involved in effective choice of technique. Recognition of the importance of adaptation and innovation calls attention to the entrepreneurial dimension in business leadership, and to the institutional structures that encourage, support, constrain, and deter entrepreneurship.

We detect an ambivalence on the part of the Bank with regard to its research commitments where their own research findings indicate they ought to be put. The tradition of economic research at the Bank has stressed neat, quantitative, formally specified models; there has been a reluctance to delve into areas of economic research where precise models and econometric technique cannot serve as the primary tools. And in fact the batting average in projects where there was little analytic structure has not been high. Issues of innovation, and institutional structure tend to be viewed as "unresearchable" or at least not amenable to rigorous research. But we propose that the logic of the Bank's own past research endeavors has led it inexorably to a requirement to engage in this kind of research. It already is engaging in such work, with studies on the capital goods sector in selected LDCs, on small and medium scale enterprises in Colombia, and on organizational and institutional factors in Colombia's garment industry exports. But there is still apparent methodological uncertainty. However, the methodological situation is not as bad as some people in the Bank may think. Over the last decade both the interest and the rigor of research on industrial organization has increased greatly. A considerable body of good and rigorous research on the economics of technical change has evolved. However, for the most part, research in these fields has been focused on issues and phenomena in advanced countries, not developing ones. We think the Bank should accept the obvious challenge.

It may be useful to give several examples of the kind of work on industrial organization and technical change that might serve as models for research at the Bank. Over the past half dozen years a considerable amount of research has been done, principally by American economists, aimed at exploring the costs of government regulation. A good reasonable overview of much of that work is provided in A. Phillips (ed) Promoting Competition in Regulated Markets. While we are nervous that much of the research by American economists on regulation has stressed the costs and downplayed the benefits, the identification and attempted quantification of the costs has been illuminating, and such work has had a significant effect on policy.

The work of Edwin Mansfield provides good examples of quantitative studies on R & D, technical advance, and productivity growth. A good collection of Mansfield's earlier work is contained in his Industrial Research and Technological Innovation. A recent study of his (and his students) in which he attempted to measure both private and social rates of return on industrial innovation was published as "Social and Private Rates of Return on Industrial Innovation", QJE, 1977. There also has been a very good work on the economics of technical change in industry undertaken by Christopher Freeman at the

University of Sussex, often in collaboration with other members of Science Policy Research Unit there. A portion of that work is presented, in capsule form, in Freeman's book The Economics of Industrial Innovation.

There has been some work on technical change in less developed countries, but not much. There are several interesting studies focused on agriculture. Hayami and Ruttan's book, Agricultural Innovation, is a useful reference. At present, Jorge Katz is directing a study on innovation in Latin American industry under the joint auspices of the IDB and ECLA. These studies represent a start. We think the time is ripe for the Bank to get into the field in a big way.

I:6 Programming in the Manufacturing Sector

Kirit S. Parikh

1. The Nature and Importance of the Research

A number of research reports and monographs are expected from the research program carried out under the heading "Programming in the Manufacturing Sector" (RPO 670-24). The list of reports/draft reports which were studied for this review is given in Appendix II. These constitute only a part of the voluminous output of the research program. The program has dealt with the problems of investment planning in industries characterized by increasing returns to scale and in industries where interdependence in the production of different products is important. Interdependence may be important when different products share capital equipment or when they use the same intermediate inputs, the manufacture of which may exhibit economies of scale. The Bank's research in this area has focused on development of improved methods for selecting investment projects from among the many alternatives in size, timing, location, technology and output mix. In addition, it has investigated the extent to which such interdependence affects project selection and planning for the development of a sector and offers scope for cooperation among the countries of a region.

The importance of externalities resulting from economies of scale and interdependence of various kinds has been widely recognized in the literature on development economics. In fact, the "big push" theory of development is based on the recognition of such interdependence and indivisibilities. Where economies of scale are predominant, a developing country's market may be inadequate for the economic scale of the plant. The make/buy choice would always seem to go against domestic production when products are considered in isolation as they would be in a cost- benefit analysis within a project-appraisal framework. The process of development could then hardly begin. On the other hand when a large number of interdependent projects are examined together, the market size for the products of some of the projects could increase sufficiently to justify domestic production. Thus the research program under review deals with issues of great importance for development policy and planning and particularly for sector planning.

It is also a part of the mythology of development planning, that there exists a shelf full of project reports and that the planners' task is to select a subset of these projects. In fact, it is hardly the case that such a shelf full of project reports is available. Detailed project reports are expensive to prepare, particularly in terms of the skilled manpower which is almost always scarce. Moreover, when the project-executing authority (or Ministry) is different from the project-sanctioning body (such as a Finance Ministry of Planning Commission), a lot of vested interest gets created in a project by the time a detailed project report is prepared. With the pressures

from such vested interests it becomes difficult to have an objective evaluation of projects. Such difficulties can be minimized if projects are identified at an early stage. Thus the development of a methodology that helps in pre-selection of projects for further detailed investigation is of great practical significance.

Moreover, before a detailed project report is prepared the techniques have to be selected. To the extent that the choice of techniques itself is affected by interdependences, the choice should be made in a wider context. This would be the case when different products share the same capital equipment. Besides its possible impact on the choice of technique, such capacity sharing would also affect the economic scale of production. Though the qualitative effects of economies of scale, indivisibilities and interdependences have been theoretically well recognized, systematic, empirical and quantitative evaluations have been rare. The set of studies carried out under the RPO 670-24 has explored these issues with commendable thoroughness.

The research has been carried out within the context of specific investment planning problems in two sets of empirical studies, one set dealing with what are termed "process industries" and the other with "non-process industries". Those industries which are characterized by a manufacturing process stream which is more or less continuous, which have a limited number of processes and where the cost of carrying midstream intermediate products is large are termed process industries. Examples of such industries are gas transmission, fertilizers, cement, etc. These industries also have a limited number of products which are more or less uniform.

The "mechanical-engineering sector" on the other hand has a variety of products and processes and the same processing equipment can be used for manufacturing many different products. Such industries are termed "non-process industries".

Both these sets of studies have used mixed integer programming models in a fixed charge formulation to account for economies of scale. The major problem that has been faced in previous studies, apart from the considerable time and efforts that usually go into data collection and organization, has been the problem of obtaining solutions of the mixed integer programming models with a large number of integer variables. Solutions to such problems require a large amount of computer time. The research program under review has developed a number of procedures to eliminate, through simple but sophisticated analysis, a number of integer variables which represented uneconomical choices, to reduce the size of the programming model. This makes obtaining solutions to such problems practicable.

Important Results

Apart from the specific sector development plans that emerge from these sector studies, they have also provided some insights into the nature of technology and its consequences.

For the process industries that have been studied:

a) Significant economies of scale are present in production activities and there is a good deal of potential interdependence within the system as a whole.

b) The use of programming models help in evaluating the consequences of alternative policies. The cost or benefits of particular policies may be significant.

c) Programming models provide a tool to estimate the benefits of regional cooperation to individual countries and help in designing schemes for sharing the benefits.

For mechanical engineering, the only non-process industry studied:

a) The relative cost of complete neglect of interdependence in choosing between production and imports is sometimes not significant at the sector level. For the part of the mechanical-engineering sector of Korea that was studied (120 carefully selected products), this would have led to an increase in sector-wide total supply cost of no more than 3 percent of the total value added for the products involved in the study.

b) Of the 120 products involved in the study, only a few were "critical" in the sense that for the others the make/buy choices were clear and unambiguous for the relevant ranges of parameters. Simple programming exercises are adequate to identify the critical products. As a percentage of value added in these critical products, the savings in cost that result when interdependence is accounted for are much larger and are found to be more than 25 percent in one variant. For individual products the savings over cost of imports in some cases exceed 40 percent.

c) Though the loss at the sector level is small for particular products, the conventional cost-benefit analysis, which neglects interdependence, may lead to wrong make/buy choices when the products are part of a sector that exhibits interdependence.

d) The absolute cost of neglect of interdependence is "by no means trivial" and is "far more" than the cost of conducting studies that take interdependence into account.

From a methodological point of view the most significant contribution of the research is the demonstration of the use of large mixed-integer programming models. In particular, the following results have been demonstrated:

a) Even without obtaining a globally optimum solution, use of programming models can provide a lot of insight into the nature and costs of the various alternatives.

b) Problems with a fairly large number of integer variables can be solved with reasonable costs.

c) With a systemtic exploration of break-even analysis a number of useful decision rules can be employed to eliminate a significant number of integer variables.

Review of Studies

Process Industries. The studies carried out for the planning of the fertilizer sector in Egypt and in East Africa have explored the choices of technology, size, location, transport, product and trade. The East African study has in addition explored in quantitative terms the gains from cooperation in fertilizer sector development for the three countries of the region (Uganda, Kenya and Tanzania).

The formulation of the models for the fertilizer sector is straightforward in mixed-integer programming terms, though the inclusion of substitution among products marks useful innovation. However, because of the shortcuts, the models have been considerably more detailed, and consequently, operationally more meaningful.

An obvious and recognized limitation of the models as developed is their neglect of uncertainty. Accounting explicitly for uncertainty would make the computation problem even more formidable. Therefore, the researchers stress the use of "pre-analysis" and sensitivity analysis to account for uncertainty.

A number of other studies have also been carried out for different sectors, countries and regions. These include among others, fertilizers for ASEAN countries, the Andean pact countries, and for India; the forestry sector for Turkey; paper and pulp for countries of the ASEAN Region and FAO's World Program; clinker production for Brazil, and energy for Nigeria. Other studies are under way.

An attempt to use this approach to develop a model to quantify the benefits of regional integration based on a simultaneous analysis of a number of industrial sectors, the pros and cons of which were succinctly presented in a small paper, was abandoned as being too ambitious.

The project has obviously been successfully carried out and to judge from the number and variety of applications that have been made and are planned it has been found useful too. Some time ago, the project reached a stage where research ended and applications began. But further effort is required to successfully disseminate and transfer the methodology for use in practical applications. A most important element in facilitating such transfer would be the development of computer software, which makes it convenient to specify the problem preferably in the language of the users as opposed to the language of the computer specialists. In addition, it would also be essential

to generalize and automatize the break-even analysis, as otherwise applications would need not only trained but also clever people, and would be severely limited in scope. The recently initiated GAMS project (RPO 671-58) should go far towards providing the necessary software.

Non-process Industries - Study of the Korean Mechanical-Engineering Sector. The study of the Korean Mechanical-Engineering Sector has explored the gains from planning simultaneously the supply of a large number of products. As already said, 120 carefully selected items were analyzed. The gains in economic efficiency of domestic production are derived from the selection of technique and scale of production, taking into account the possibilities of sharing capital equipment for a variety of products, as well as the possibilities of domestically producing on a large scale intermediate goods used in a number of products. The import or domestic production decisions are taken after considering the effects of such sector-wide interdependence.

The mechanical-engineering sector is not easy to model in the conventional way. Problems of appropriate description and specification of products and processes have to be faced, and such problems are not insignificant. In applying a micro-analytic approach to a sector-wide study, there is a danger of getting lost in details and not seeing the wood for the trees. This is avoided in the study, by describing certain standardized products whose production processes are described at the shop level rather than at the machine level. Even then, the breadth and depth of the technical-engineering detail that is incorporated in the study is to be found in hardly any other programming study.

The model used here is a mixed-integer programming one, and break-even analysis is developed and used to reduce computational difficulties.

In addition, the allocative consequences of the results are explored in depth. An evaluation of alternative investment criteria is also made in the context of the results obtained from the model. The conclusion is reached that simple cost-benefit criteria, or the measures of comparative advantage when substituted for the more thorough analysis of the type undertaken in the study, lead to wrong make/buy decisions for some of the products with consequent loss to the economy. It is shown that the estimated loss entailed in using the best cost-benefit criteria is found to be large enough to pay for the cost of more comprehensive analysis within one to two years. However, it may be noted that in order to fully realize the benefits from a project which becomes economically attractive only when interdependences are accounted for, all the interdependent projects have to be executed together. A delay in one of them would adversely affect not only its own economics but also that of the others.

Moreover, if an integrated sectoral planning procedure leads to, which it need not, additional delays in clearing projects, the benefits of such planning to some extent become negated. Yet this is not to argue for the exclusive use of simple social cost-benefit analysis. It is merely to underline the context in which sectoral programming can be fruitfully applied.

Since such programming earns a handsome rate of return, there is no reason not to pursue it.

The project also complements a number of other research studies where attention to micro-level details may be crucial. The research programs on The Scope for Capital-Labor Substitution in the Mechanical-Engineering Sector (RPO 670-23) and on Appropriate Industrial Technology (RPO 671-51) are such projects (see A.5).

Evaluation

Quality of Research. The research work is certainly of a high caliber and at the frontier of work in this area. Moreover, such research is hardly done outside the bank. The volume of research output is also high.

Usefulness for LDCs. Large programming models create an impression that the vision behind the process of development that motivates such studies is one in which an elite all-knowing planning authority attains economic growth through effectively allocating resources to various sectors. Yet one need not share this vision before considering such models to be useful. Process industries such as fertilizers, cement, etc., characterized by economies of scale and a relatively small number of plants, are the industries whose development is usually guided and promoted by most governments of developing countries. Starting a few large industrial projects is one of the easiest things that governments do to promote development. The planning models developed by Bank research have the potential to improve the rationality of government decisions in developing these sectors. Some of this potential is already realized in the numerous applications made for different sectors and different countries.

However, the full potential usefulness can be realized only if adequate "extension work" follows this Bank research. We shall return to this later.

Usefulness for Bank. Clearly, the studies related to specific areas must have been carried out in collaboration with the operational staff of the Regional Department concerned. The results should have been useful in guiding the Bank's lending operations, provided they were available in time. But clearly a lot of potential is there for such work to be useful in Bank activities.

Improvement in project identification methodology in the LDCs could be of considerable significance for the Bank's operations. If more attractive projects are identified before the detailed project reports are prepared, these reports would be made for comparatively better projects. This would imply that better projects are put up to the Bank for financing.

Part of the work on the fertilizer sector plan for East Africa might have been made irrelevant by the subsequent political developments

in East Africa and the breakup of the economic union. Even then, the non-cooperation solutions could still have been useful to the policy makers in the three countries.

The Bank may have a unique comparative advantage in carrying out studies, such as the fertilizer study for East Africa, that identify areas for regional cooperation and facilitate the process of realizing such cooperation. As an authority that lends money to the various countries of a region, it may have access to data and policy makers in these countries. Moreover, as a third party its analysis may be less suspect. On the other hand, the Bank should also be interested in promoting such cooperation that reduces the need for credit in the region.

Development of Research Capacity in LDCs: Creating research capability is a time-consuming task and learning by doing is an essential element in the development of research skills. Significant participation of researchers from the LDCs is an absolute requisite for the successful transfer of the research to application in the LDCs. However, time constraints hamper research programs; communication problems across large distances and lack of access to computers and xerox machines in many LDCs make such participation difficult. The country specific-sector studies undertaken to date have involved participation of local persons. But an effective program has yet to be designed to facilitate participation of researchers from the LDCs, to ensure that there would be established within the country the capability to either update and/or improve the particular sector study or to carry out a similar study for another sector.

Suggestions for Research in Future

a) Extension: The research results and the methodology developed are sufficiently important that they be brought to practitioners, planners and policy makers in the LDCs. This would call for a considerable amount of "extension" work. The manuals currently under preparation are vital to facilitate extension, but their availability constitutes only one element in the process.

Similarly, short training courses would also be inadequate by themselves. What would be required is a case study for a sector, which is carried out with active participation of a local team or, better still, a local institution. It may even be desirable that the studies be carried out by a local team. Such studies may be coordinated by Bank staff who are familiar with such research work, and should certainly be financed by the Bank. Even when the success rate of such research is not high, it may be considered a necessary investment in building up research capability in LDCs. Such support should also include provision of computer hardware in the case of many LDCs. A sector study based on mixed-integer models needs convenient and substantial access to fairly large computers if the study is to be completed within a reasonable time.

Development of user-oriented software that permits convenient specification of the problem by users, who may not be computer specialists, could be of invaluable help in promoting such studies in the LDCs.

The emphasis on extension is motivated by a vital consideration. An important benefit of any modelling accrues largely to the modeller in the form of sharper insights and improved understanding of the processes being modelled and their interdependence. The major benefit to be obtained from using a programming approach to the planning of an industry is to be derived from training that would be imparted, in following such an approach, to a set of engineers and economists to think more systematically about the scope of technical options in developing the sector. The engineers would be less certain of their thumb rules and the economists would be more aware of the technological constraints. The information and insights generated could substantially improve the quality of decision making in the sector. Unless these insights accrue to the people who will continue to take decisions in the LDCs, the benefit would not be maintained.

The foregoing is not to deny the usefulness of attempts at extension through previous case studies, but to recommend that more resources be devoted to extension, and that the case studies have as their primary objective effective extension.

The decision, which is reported to have been taken to set up a special group to promote case studies, is to be strongly endorsed. To begin with, two to three full-time professions, capable of guiding projects in a number of different areas and countries, should be provided for in this group.

b) Consequences of Interdependence for Planning and Promoting Industrial Development: If economies of scale and interdependence are important, then the development of that industry would benefit from some form of central planning or coordination. On the other hand, effective implementation and central planning of a sector such as the mechanical-engineering industry, characterized by a large number of products and processes, is difficult, to say the least.

Thus the findings of the Korean Mechanical-Engineering Industry study, that interdependence appears not to be of much quantitative consequence, are very comforting as one can rely on implementable, decentralized procedures for developing the industry. Yet, the authors of the Korean study emphasize that the finding needs to be confirmed with further research. Moreover, as they point out, there is reason to suspect that the aggregation approach used may bias their findings. Would a different set of international prices, such as may prevail at other times, lead to a different result? Would different domestic prices, such as may be found in another country, give a different result? Would a different product-mix produce contrary results? Is it possible that under such different circumstances the benefits of accounting for interdependence would be highly significant, say 50 percent or more of the value added in the sector?

The quantitative dimension of the effects of interdependence is sufficiently important for policy purposes that further explorations to test the generalizability of the Korean Mechanical-Engineering study are called for. This has been the only study that has investigated these issues empirically, and after having made the investment in developing conceptual frameworks and methodologies for such studies, it would be desirable for the Bank to pursue this research to its logical conclusion. It would, according to the reviewer of this section, be a grave loss if the Bank were to stop after studying just one sector in one country.

c) Programming models to evaluate Appropriate Technology for Rural Industrial Development: The models dealt with in this research can be used to throw some light on issues of considerable interest in LDCs. To what extent should one develop a decentralized industrial structure? What are the costs of a rural-based industrial development? What is the implication for employment and income generation of development based on small-scale industries? Are the benefits of dispersal sufficient to compensate for the extra cost of setting up smaller industrial units? These issues need to be explored in a systematic, technical and dispassionate way. The case study of pulp and paper in Malaysia examines some of these issues, while the follow-up work under RPO 670-23 has a great deal of relevance to understanding questions concerned with the organization of production.

Nonetheless, with the foregoing exceptions, the studies in industrial programming under RPO 670-24 have tended to view the problems of development in a purely technical way. The objective has been to find least-cost solutions. Institutional issues in the organization of sectors, in the difficulties of implementation, or in the realm of selection of policy instruments should be brought to the fore in subsequent research.

In evaluating the appropriateness or otherwise of technology, not only relative factor scarcities should be taken care of but also the limitations of public policy in using certain instruments. Thus if income redistribution policies are politically hard to pursue, one might put emphasis on income generation in selecting "appropriate" techniques.

d) Institutional Issues In Implementation and Capacity Utilization: Though the bank research in this area of industrial programming has been useful and has indicated benefit in excess of costs, one may still ask if there are other issues that need to be researched for promoting industrial development.

The problem of implementation of projects and the efficiency with which even large industrial projects are operated in many LDCs are of great importance. Mixed-integer programming models can be used to examine the consequences of different levels of operational efficiency in existing plants, as was done in the Egyptian fertilizer case study. Nonetheless, the issues here go much deeper. To what extent are delays in installation of capacity and inefficient use of installed capacity in large industrial projects due

to improper organization, due to inappropriate objectives of the management, or due to inexperience and inadequate skills? Are delays and inefficiencies inherent in the organization of the public sector, which plays a large part in the development of industrial sectors in some countries? Does the private sector really perform better? These issues need to be examined. The potential gains from being able to design effective organizational and institutional frameworks are enormous. Moreover, the Bank would have a comparative advantage in carrying out research in this area as it would be able to pull together experience from a number of countries with a wide range of characteristics.

Appendix II

RESEARCH ADVISORY PANEL ON INDUSTRIAL DEVELOPMENT AND TRADE

List of Projects and Papers Reviewed [1/]

Project Title	Project Code	Reports
Field I. Incentive Policies and Economic Integration		
Development Strategies in Semi-Industrial Countries	670-01	Balassa, Bela, "Exports and Economic Growth: Further Evidence." Journal of Development Economics, 5, June 1978. Also World Bank Reprint Series, no. 58. _____, "Export Incentives and Export Performance in Developing Countries: A Comparative Analysis." Weltwirtschaftliches Archiv, March 1978. Also World Bank Staff Working Paper, no. 248. Washington, D.C.: World Bank, April 1974. _____, ed. Development Strategies in Semi-Industrial Countries. (mimeo, 1978). _____, "Reforming the System of Incentives in Developing Countries." World Development 3 (June 1975): pp. 365-82. Spanish translation in Cuadernos de Economia, December 1974: pp. 33-46. Also World Bank Reprint Series No. 22.
Economies of Scale and Tariff Levels	670-22 *	Hansen, J.R., "Economies of Scale and Tariff Levels in the Iranian Motor Vehicle Industry: A Case Study." (mimeo, November 1973) Pursell, G. Economies of Scale and Tariff Levels (incomplete) (mimeo, 1972).
Industrial Policies and Economic Integration in West Africa	670-87	Balassa, Bela, "Comparative Advantage and the Prospects for Economic Integration in West Africa." Paper prepared for the Colloque sur l'integration en Afrique de l'Ouest held in Dakar, Senegal in March-April, 1978. Monson, Terry D., and Pursell, Garry, "An Evaluation of Expatriate Labor Replacement in the Ivory Coast." Journal of Development Economics, Vol. 5, No. 4, December 1978. (Discussion Paper, no. 49. Center for Research on Economic Development, University of Michigan, April 1976). (French translation in L'Actualite Economique, June 1977.) Pursell, Garry, "Cross-Section Cost-Benefit Analysis of Manufacturing with Foreign Capital: A Case Study from West Africa." Paper presented to the Southern Economic Association meetings, Washington, D.C., November 1978. Shepherd, Geoffrey and Stryker, D., "The System of Incentives and Comparative Advantage in Malian Agriculture and Industry", (mimeo, June 1975).
Promotion of Nontraditional Exports	670-10 *	ECLA. Politicas de Promocion de Exportaciones. (E/CEPAL/1046, Santiago, Chile, October 1977).
rnational Trade Policy for the Development Bangladesh	671-75	(No papers)
Other Papers		Balassa, Bela, Policy Reform in Developing Countries. Oxford, Pergamon Press, 1977. _____, "Tariffs and Trade Policy in the Andean Common Market." Journal of Common Market Studies, December 1973, pp. 176-195. Spanish translation in Revista de la Integracion, May 1974, pp. 7-34. _____, "Types of Economic Integration." Economic Integration, Worldwide, Regional, Sectoral. (Fritz Machlup, ed.) (Proceedings of the 4th Congress of the International Economic Association held in Budapest, Hungary in August, 1974), London,Macmillan, 1976, pp. 17-31. Guisinger, S., "Industrial Protection in IFC Projects." (mimeo, December 30, 1975).
Field II. Comparative Advantage, Trade Patterns, and Economic Growth		
International Model	670-07	Ginsburgh, Victor, "A General Equilibrium Model of World Trade, Part I: Full Format Computation of Economic Equilibria." Cowles Foundation Discussion Paper No. 412. New Haven, Conn.: Yale University, 1975. _____, "A General Equilibrium Model of World Trade, Part II: The Empirical Specification." Cowles Foundation Discussion Paper No. 413. New Haven, Conn.: Yale University, 1975.
Expansion of Manufacturing for Exports in Developing Countries	670-19 *	Chang, Y.S. "An Analysis of the Offshore Activities of the Japanese Electronics Industry." (mimeo, March 1973) Ozawa, Terutomo, "Labor Resource Oriented Migration of Japanese Industries to Taiwan, Singapore and South Korea." World Bank Staff Working Paper, no. 134, Washington, D.C.: World Bank, August 1972.
Economic Development of East and Southeast Asia	670-79 *	Okita, Saburo, et al., "The Interrelated Economic Development of East and Southeast Asia in the Coming Decade." (mimeo, August 1974)
Patterns of Industrial Development	671-05	Prakash, Vinod, "An Overview Paper on RPO 671-05 'Patterns of Industrial Development'." (mimeo, May 1977) _____, "Measuring Industrial Exports: A Comparative Statistical Study of Variations Arising from Differences of Definition." World Bank Staff Working Paper, no. 225. Washington, D.C.: World Bank, February 1976. _____, "Statistical Indicators of Industrial Development: A Critique of the Basic Data." World Bank Staff Working Paper No. 189. Washington, D.C.: World Bank, September 1974.

Project Title	Project Code	Reports
A Comparative Study of the Sources of Industrial Growth and Structural Change	671-32	Balassa, Bela, "Accounting for Economic Growth: The Case of Norway." World Bank Development Research Center Discussion Paper No. 17 and Oxford Economic Papers (forthcoming). Chenery, Hollis B., "Transitional Growth and World Industrialization." Presented at the Nobel Symposium on the International Allocation of Economic Activity, Stockholm, June 1976. Chenery, Hollis B. and Moises Syrquin, "A Comparative Analysis of Industrial Growth." Presented at the Fifth World Congress of the International Economic Association on Economic Growth and Resources, Tokyo, August/September 1977. Syrquin, Moises, "Sources of Industrial Growth and Change: An Alternative Measure." Presented at the European Meeting of the Econometric Society, Helsinki, August 1976. Westphal, Larry E., "Progress Report on a Comparative Study of the Sources of Industrial Growth and Structural Change." August 4, 1977.
Sources of Growth and Productivity Change	671-79	(No papers)
Other Papers		Balassa, Bela, "Effects of Commercial Policy on International Trade, the Location of Production, and Factor Movements,in"The International Allocation of Economic Activity" pp. 230-58, 270-74 (Bertil Ohlin, Per-Ove Hesselborn, and Per Magnus Wijkman ed.) Proceedings of a Nobel Symposium held at Stockholm in June 1976. Macmillan, London, 1977. _____, "A 'Stages' Approach to Comparative Advantage." World Bank Staff Working Paper No. 256. Washington, D.C.: World Bank.
Field III. Export Promotion Policies in Developing Countries and Access to Markets in Developed Countries		
Industrialization and Trade Policies for the 1970's	670-20 *	Hughes, Helen, (ed.), Prospects for Partnership: Industrialization and Trade Policies in the 1970's. Baltimore: Johns Hopkins University Press, 1973.
Export Promotion and Preferences: India	670-21 *	Wall, D., "Draft Report on Industrial Export Policy." (mimeo,August 1974) _____, "Export Development and Promotion Policies in India." (mimeo,March 1975) _____, "Export Processing Zones." Journal of World Trade Law, August/September 1976, pp. 478-489. _____, "The Impact of the Generalized System of Preferences on India's Exports." (mimeo, February 1973)
Export Incentives in Developing Countries	671-35	(No papers)
Marketing Manufactured Exports	671-56	(No papers)
Effects of Increased Imports of Manufactured Goods from Developing Countries to the United States	671-67	(No papers)
Effects of Increased Imports of Manufactured Goods from Developing Countries to Western Europe	671-66	(No papers)
Key Institutions and Expansion of Manufactured Exports	671-68	(No papers)
Other Papers		Balassa, Bela, "World Trade and the International Economy: Trends, Prospects and Policy." World Bank Staff Working Paper No. 282. Washington, D.C.: World Bank. Keesing, D. and Plesch, P., "Recent Trends in Manufactured and Total Exports from Developing Countries." (mimeo, June 1977). Keesing, D., Plesch, P., and Triner, G., "Developing Countries' Exports of Textiles and Clothing: Perspective and Policy Choices." (mimeo, May 1978) Plesch, P., "Developing Countries' Exports of Electronics and Electrical Engineering Products." (mimeo, February 1978)
Field IV. Small Enterprises; Credit Markets, Public Enterprises		
Financing of Small Scale Industry	670-77 *	Kochav, D., et al., "Financing the Development of Small-Scale Industries." World Bank Staff Working Paper No. 191. Washington, D.C.: World Bank, November 1974.
Small-Scale Enterprise Development	671-59	Anderson, Dennis, "Estimating the Economic Benefits of Small Scale Enterprise Credit Projects." (mimeo May 12, 1978)
Small Enterprise Financing: Role of Informal Credit Markets	671-65	(No papers)
Capital Market Imperfections and Economic Development	671-69	Bhatt, V.V., "Interest Rate, Transaction Costs and Financial Innovations." World Bank: Public and Private Finance Division, Domestic Finance Studies No. 47, January 1978. Roe, Alan R., "Some Theory of the Financial Intermediation in Less Developed Countries." World Bank: Public and Private Finance Division, Domestic Finance Studies No. 50, May 1978. Saito, Katrine A., and Villanueva,Dan P., "Portfolio Determinants of Commercial Bank Earnings in Selected Asian Countries." World Bank Public and Private Finance Division, Domestic Finance Studies No. 49, March 1978.

Project Title	Project Code	Reports
Public Manufacturing Enterprises	671-71	(No papers)
Other Papers		O'Mara, G., "An Econometric Analysis of the Role of Small-Scale Industry in Brazilian Industrialization, 1949-1970." (mimeo, June 1976) _____, "The Role of Small-Scale Industry in Recent Brazilian Industrialization." (mimeo, April 1978)
Field V. Capital Utilization, Capital-Labour Substitution, and Technological Change		
Scope for Capital-Labor Substitution in the Mechanical Engineering Industry	670-23	Lamyai, Thanong; Rhee, Yung W.; and Westphal, Larry E., "Factor Substitution, Returns to Scale and the Organization of Production in the Mechanical Engineering Industry." (mimeo, July 1978) Nam, Joon W.; Rhee, Yung W.; and Westphal, Larry E., "Data Development for a Study of the Scope for Capital-Labor Substitution in the Mechanical Engineering Industries." (mimeo, February 1973) Rhee, Yung W., and Westphal, Larry E., "Institutional and Economic Criteria for the Choice of Technology in Developing Countries." (mimeo, June 1978) Lamyai, Thanong; Rhee, Yung, W.; and Westphal, Larry E., "Factor Substitution, Returns to Scale and the Organization of Production in the Mechanical Engineering Industry." (mimeo, July 1978)
Industrial Capacity Utilization in Selected Latin American Countries	670-25	Schydlowsky, Daniel M., "Capital Utilization, Growth, Employment and Balance of Payments and Price Stabilization." Discussion Paper Series No. 22 Boston, Mass.: Boston University, December 1976.
Employment and Capital Labour Substitution	670-54 *	Mundlak, Yair, "Estimation of Production Functions; A Survey." (mimeo, June 1973) Thorbecke, Erik, and Sengupta, Jati, "A Consistency Framework for Employment, Output and Income Distribution Projections Applied to Colombia." (mimeo, June 1973)
Industrial Capacity Utilization	670-95 *	Batista, Romeo, "On Excess Capacity in Philippine Manufacturing." Philippine Review of Business and Economics, December 1971. Hughes, Helen et. al., Capital Utilization in Manufacturing in Developing Countries, World Bank Staff Working Paper No. 242, September 1976. Kim, Young C. and Kwon, Jene K., "The Utilization of Capital and the Growth of Output in a Developing Economy," Journal of Development Economics, April 1977. Lim, David, "On the Measurement of Capital Utilization in LDC's". Oxford Economic Papers, March 1976, pp. 149-159. _____, "Capital Utilization of Local and Foreign Establishments." Review of Economics and Statistics, May 1976, pp. 209-217.
Field VI. Programming in the Manufacturing Sector		
Programming in the Manufacturing Sector	670-24	Balassa, Bela, and Stoutjesdijk, Ardy, "Economic Integration among Developing Countries." Journal of Common Market Studies, 186 (September 1975). Also World Bank Reprint Series No. 30. Choksi, Armeane M; Meeraus, Alexander; and Stoutjesdijk, A., The Planning of Investment Programs in the Fertilizer Industry, Volume 2 in The Planning of Investment Programs. (forthcoming, Johns Hopkins University Press.) _____, "A Planning Model of the Fertilizer Sector in Egypt." World Bank Staff Working Paper no. 269. July 1977. Kendrick, David and Stoutjesdijk, Ardy, The Planning of Industrial Investment Programs: A Methodology. Volume 1 in The Planning of Investment Programs. (edited by Alexander Meeraus and Ardy Stoutjesdijk). Johns Hopkins University Press, Baltimore and London, 1978. Stoutjesdijk, Ardy, and Westphal, Larry, (eds.), Industrial Investment Analysis under Increasing Returns. (mimeo, September 1978) Westphal, Larry E., "Methodology of Investment Planning in the Non-Process Industries." (mimeo, February 1976) Westphal, Larry E., and Rhee, Y.W., "The Allocative Consequences of Economies of Scale." Development Research Center, Discussion Paper No. 18, World Bank, January 1976. _____, "A Process Analysis Model of the Mechanical Engineering Sector." (mimeo, January 1976)
Other Papers		World Bank, "Fertilizer Requirements of Developing Countries." World Bank Report No. 446, May 1974.

Project Title	Project Code	Reports
Industrial Capacity Utilization	670-95*	Morawetz, David, "Israel" in Capital Utilization in Developing Countries (forthcoming).
		Thoumi, Francisco E., "The Utilization of Fixed Industrial Capital in Colombia: Some Empirical Findings," in Essays on Colombian Industrialization. Edited by R. Albert Bery, Princeton, N.J.; Princeton University Press, 1974.
		Winston, Gordon, "The Theory of Capital Utilization and Idleness," World Bank Staff Working Paper No. 176. April 1974, Journal of Economic Literature, 12 (December 1974).
Appropriate Industrial Technology	671-51	Pack, Howard, "The Macroeconomic Effects of Appropriate Choice of Technique." (mimeo December, 1977)
		_____, "The Capital Goods Sector in LDC's: A Survey." (mimeo, May 1978)
Other Papers		Amselem, Michel, "Technological Choice in Developing Countries: The Impact of Differences in Factor Costs." Unpublished PhD Thesis, Harvard University, 1978.
		Choi, Harry and Westphal, Larry E., "Outline of the Mexican Plant Equipment Planning Study," November 1976.
		Cremer, J., "Planning Interdependent Production Processes with Increasing Returns to Scale: Some Theory and an Algorithm." Ecole Polytechnique, Laboratoire d'Econometrie, (mimeo, December 1976).
		Westphal, Larry E., "UNDP Proposal - Mexico Plant Equipment Complex Design Study," March 14, 1978.

1/ Original research proposals and other documentation on the research projects were reviewed by the panel in all cases.

* Completed research projects.