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Foreword

We hereby submit our Report on World Bank research on industry and trade.

March 31, 1979

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Semifinal version

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Fifth version

REPORT BY WORLD BANK PANEL ON INDUSTRIALIZATION AND TRADE

by

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February 20, 1979

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CHAPTER I

GENERAL ISSUES

The 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 of research 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 1/6 of the administrative budget of the Bank. About 1/4 of this analytical work seems to be formally classified as "research", of which approximately 1/7, covering the activities of 10-11 man-years of research, is on industry and trade, i.e. the area that is the subject of this report.

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 discussions 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.

I:1 Bank research - 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". Thus, the audience for Bank research should, according to these statements, be both Bank representatives who are responsible for general policy issues and staff members who are engaged in more narrowly defined Bank operations. However, it is obvious that Bank research may be of great relevance also for people outside the Bank. In fact, the Bank has regarded it as a duty both to stimulate research in the less developed countries, and 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.

(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 inforamtion, 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 continues 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 fact, 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 the revealed research priorities of the Bank during recent years simply by looking at the background of the economists who had already been hired by the Bank! But it is equally true that 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 avoid 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 the operational and the policy advisary activities of the Bank. In fact, the Bank offers to researchers the incentive to work in fields close to policy formulation and implementation, which makes the Bank a natural place 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 studies of government policies and regulations. And a third quite related example is 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 of 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 comes 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 dominant 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 therefore be reasonable to limit demands on the Bank in this field to taking greater responsibility for the data which it actually collects and uses 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 do not have.

A strict adherence to the principle of comparative advantage in the production of research would imply that types of research knowledge which are not effectively produced by the Bank - regardless of how important they are for Bank operations - should be imported rather than produced by the Bank. 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 inference of the principle of residual supplier of research is probably that it motivates 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 applications 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 motivates studies also concerning problems in which Bank research has not so far been strongly involved, but which in our judgement reflect severe bottlenecks for economic development in many LDCs. This brings us back to a recommendation for some research that earlier was motivated by the comparative advantage principle, i.e. research on the consequences of (1) government policies and regulations, (2) institutional arrangements and (3) 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 earlier on the least developed LDCs, 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 operations, or because knowledge is needed in these fields for Bank operations (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 have so far 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, in many cases research done within the Bank may be more visible to the operations people in the Bank than research done elsewhere in the same field. Also practitioners in an organization can be made aware of what researchers outside the organization are doing by having a number of researchers within their organization (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 born in mind in allocating researchers non-research time.

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 <u>general</u> quality of the researchers within the organization. The presence of good researchers, like competent people in general, helps to set standards of performance 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 possibilities of the Bank to work efficiently, and perhaps even to survive in a long-run perspective.

In other words, the research of the Bank should not be subordinate only to the (relative) efficiency of the Bank as a producer of the research, or to the immediate demands of the operating 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 also should be interested in the externalities of its research on the research potential of the LDCs. The adherence to this principle is an argument for (a) choosing fields of research of interest to scholars and research institutions in LDCs, and (b) 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 searching for 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 an 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:

(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 <u>one</u> 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, that should be emphasized. A similar attempt has, of course, been made by us.

Also the <u>procedure</u> 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 on research.

Finally, if a high priority is given to the ambition to improve the research capacity in the LDCs, participation of researchers from the LDCs 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 research portfolio of the Bank to assure reasonable efficiency.

I:2 How can import, production, application and dissemination of research be made 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.

a) 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 the Bank. We therefore suggest that the Bank strengthens 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 operation 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 operation positions in the Bank; researchers of the Bank may move over permanently to operation positions; and researchers of the Bank may take operation positions temporarily (for instance one or a few years). 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 already when they are employed, rather than by reading research documents or doing research later on.

If the Bank is anxious that research which is financed by the organization is highly relevant for Bank operations, i.e. if a heavy 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-sidedness 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 (p. 00).

b) 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 therefore 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 understanding 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 for 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 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 know 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.

c) 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 operation officers, 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 policy and industrialization strategy. By being decentralized, such 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 probably 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.

d) 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 operations people and policy advisers, but rather to spread around a certain way of looking at things.

Good interaction between researchers and operations people is not easy to achieve. In fact, our interviews among people in the Bank revealed a certain tension between researchers and operations officers. This is nothing peculiar to the World Bank, however.

Researchers within an organization that deals with practical affairs almost inevitably will be viewed by operations officers as being somewhat distant and academic - a kind of research enclave. But it is important that research maintains a certain distance from the pressures bearing on the operations 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 operations staff is that researchers are usually concerned with a much longer time perspective than operations officers. The production period of research is, moreover, often so long that when results do emerge, operations people may have lost interest in the question. And sometimes the empirical data which are 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, operations people are often more interested in drawing on knowledge, in particular in obtaining knowledge that pertains to a particular situation. The researcher often regards the search for the latter type of knowledge as "information gathering" rather than research. Operations people, 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, many operations officers are not aware of, interested in, or able to absorb results of research 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 sophisticated understanding of what operations officers 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 operations people, and if researchers could communicate more effectively with operations officers. 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 the 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.

(1) 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 that not only outstanding academic contributions are summarized. It may also be useful to try to find out what types of research have been successfully used in other operating organizations.

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

(3) That more circulation of people between research and operational activities is brought about. Sabbatical leave for research, within or outside the Bank, for the operations 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.

(4) 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 operations people - 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 most efficiently achieved if researchers and operations people 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 operations people to make more research suggestions. More informal - i.e. less bureaucratic - procedures when drafting and planning new research projects might increase the possibilities of operations officers contributing to the initiation of and participation in research.

(5) Apart from individual research projects, cooperation between researchers and operations 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.

(6) 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.

(7) Moreover, the earlier suggested employment of people with research background in operation 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 the operations officers. 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 operation positions, and the suggestion to recruit (more permanently) people with a research background to operation positions means that some bridges would be built between research and operational activities. 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 operations people - 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.

e) 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. In turn, it must be considered whether the time spent by researchers in their non-production 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.

I:3 Implications for research of alternative strategies for economic development

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 (a) a specification of what types of countries we are talking about and (b) 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, research on nationwide planning models and empirical studies of the process of central planning in various countries would probably be top priorities of research. It would then, of course, be important to remember that there are substantial elements of decentralization of information, decision making and initiatives 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 many of research resources into the acquiring of research knowledge in this field is most likely that very few member countries of the Bank are centrally planned economies, though of course elements of such exist everywhere, 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 sectorial planning or programming of investment decisions, for instance in sectors where there are huge returns to scale, externalities, or (direct) inter-sectorial linkages. Then it may also be possible to consider aspects which are not usually 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 in fact also been so for the Bank in recent years (see Section III:6), 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 process 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 research in the Bank in this field has in fact 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 substituion and technological change (Section III:5) - a research field of great interest both in the case of centralized and decentralized versions of the development process.

Research knowledge of patterns of growth of production and trade (the field covered by Section III:2) is too 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 reliance on different types of policy and planning operations than in centrally planned economies. Obvious examples, beside 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 (the field covered by Section III: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 (the field covered by Section III: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 of course is a dichotomy concerning the mode of organization rather than concerning the allocation of resources. In reality, the development strategies of various countries differ of course also with respect to the allocation of resources. For instance, one important choice is between export-oriented (outward-looking) and import-substitution-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 which 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 of course also heavily dependent on a number of other features of individual countries, such as the initial conditions 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.

I:4 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 on such issues through its operations, or because knowledge is needed in these fields for successful Bank operations (and is not easy to import). 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 adapted 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 operations people in the Bank to each others interests, needs, and competence. We have also suggested a number of arrangements to help achieve this.

In general both researchers and operations people are obviously 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 operations people but without too frequent demands as to the reporting and evaluation of their work.

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REPORT BY WORLD BANK PANEL ON INDUSTRIALIZATION AND TRADE

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CONTENTS

CHAPTER I

GENERAL ISSUES

The 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 of research but rather the type of research produced by the Bank 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 - comprise approximately 1/6 of the administration budget of the Bank. About 1/4 of this analytical work seems to be formally classified as "research", of which approximately 1/7, covering the activities of 10-11 man-years of researchers, is on industry and trade, i.e. the area that is the subject of this report.

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 of the research. We shall thereafter make som suggestions as to how the efficiency of various research activities of the Bank - import, production and application 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. 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 about future research priorities of the Bank in this field. More detailed discussions on these issues are presented in Chapter III, where the previous and present research of the Bank on industry and trade are classified into six main areas.

I:1 Bank research - 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 of Bank research is supposed to be. A hint on these issues is provided by a formulation in our terms of reference according to which the research objectives of the Bank include the task "to support all aspects of Bank operations ..." and "to broaden our understanding of the development process". Thus, the audience of Bank research should, according to these statements, be both Bank representatives who are responsible for general policy issues and staff members who are engaged in more narrowly defined Bank operations. However, it is obvious that Bank research may be of great relevance also for people outside the Bank. In fact, the Bank has regarded it as a duty both to stimulate research in the less developed countries, and 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 is 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 needs 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, neglecting for the moment the issue of "for whom" Bank research is supposed to be performed. 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?

It would seem that this appraoch 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.

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.

(b) Bank research should exploit the skills and information that are acquired within the Bank in its operational activities as a 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 about facts and problems in a large number of countries to make comparative studies of national economies.

(e) The Bank should try to produce 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 if this should be called "research".)

Aspect (a) would imply that the Bank continues 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 fact, 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 an emphasis on effective protection and domestic resource costs (the field of Bela Balassa), growth patterns and sources of growth (the field of Hollis Chenery, Don Keesing and others), 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 the "revealed" research priorities of the Bank during recent years simply by looking at the "background" of the economists who had already been hired by the Bank!

By contrast, the Bank has not been very successful in contracting outside scholars and in making researhc in circumstances where there has not been strong leadership exerted from researchers within the Bank. It would seem that projects dominated by "outsiders" have usually not succeeded as well as projects 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 recommend the Bank to void running into a straightjacket in subjecting the appointment of researchers to nationality criteria. The experience of often 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 to the operating and the policy advice activities of the Bank. In fact, the Bank offers to researchers the incentive to work in fields close to policy formulation and implementation, which makes the Bank a natural place 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 that is useful for research activities is acquired by Bank operations is probably studies of government policies and regulations. And a third quite related example is studies of institutions and incentive systems 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 of studies of this kind has also been made in sector reviews and studies of small-scale enterprises, state enterprises and financial intermidiaries, for instance within the Industrial and Finance Division (IFD). Ongoing analytical work within the Bank on trends and problems on international markets and on 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 about 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, would suggest a shift in Bank research activities to more collection, processing and publishing of data that comes 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 dominant "data bank" in the development field - would require truly enormous resources. We know how great such a task is already on a national scale. To do the same thing for about 100 countries would therefore require a formidable effort. It may therefore be reasonable to limit demands on the Bank in this field to take a greater responsibility for the data which it actually collects and uses in its own research and surveys, and to make these data available for outsiders to a large extent. It may be noticed that this is not an issue only 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 collection, 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 activities, to be efficient, often require a rather strong and permanent research organization, which the social science departments of universities often do not have.

A strict adherence to the principle of "comparative advantage" in the production of research would imply that types of research knowledge which are not effectively produced by the Bank - regardless of how important they are for Bank operations - should be imported rather than produced by the Bank. 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 of (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 for certain types of research knowledge that the Bank needs (demands). Thus, in order to satisfy its own needs for research knowledge, the Bank certainly has to make research that is particularly useful for operating Bank needs, and that is not done elsewhere. In other words, the Bank has to perform also 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 activities of the Bank as a 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 comes out to the effect that the techniques may have been somewhat "overelaborated" for the purpose of Bank operations.

Another inference of the principle of "residual supplier of research" is probably that it motivates research concerning large investment projects (in particular where the Bank is involved as a lender), including analyses of the externalities (such as learning by doing) of the projects, returns to scale and the 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 (IPD). It would appear, however, that the operational staff of the Bank, particularly in the regional offices, has usually not found the research done in this field very helpful.

However, we would argue that an adherence to the principle of "the residual supplier of research" motivates studies also concerning problems about which Bank research has not so far been strongly involved, but which in our judgement reflect severe bottlenecks for economic development in many LDCs. This brings us back to a recommendation of some research that earlier was motivated by "the comparative advantage principle", i.e. research on (1) comparative government policies and regulations, (2) institutions, and (3) incentive regimes not only in product markets (where the Bank has done considerable research efforts) but also in factor markets. The argument for more research efforts on the roles of institutions is probably particularly strong if the Bank would choose to concentrate research more than earlier on the least developed LDCs, 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 <u>both</u> seem to imply a strong emphasis on studies of the consequences of alternative institutional arrangements, 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 on such issues by way of its operations, or because knowledge is needed in these fields for Bank operations (and is not easily imported). A rather special reason why the Bank may be a suitable place for institutional research, relative to university institutions, is that institutional research so far has not gained a very high "status" in the academic world. Perhaps the World Bank could to some extent break out of the academic status ranking system?

We shall later on (Chapters II and III) discuss in some detail both why more research of these types are important and how they 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, in many cases research done within the Bank may be more "visible" to the operations people in the Bank than research done elsewhere in the same field. Moreover, the only efficient way of making practitioners in an organization aware of what researchers outside the organization are doing is probably to have a number of researchers within their organization (a point to be developed in the next section).

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. The presence of good researchers, like competent people in general, helps to set standards of performance 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 possibilities of the Bank to work efficiently, and perhaps even to "survive" in a long-run perspective.

In other words, the research of the Bank should not be subordinate only to the (relative) efficiency of the Bank as a producer of the research, or to the immediate demands of the operating units of the Bank, but also to long-run considerations of the general competence of the Bank in various respects. In particular, it is importnat to stimulate the capacity 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, in our view, be interested in the "externalities" of its resarch on the research potential of the LDCs. The adherence to this principle is an argument for choosing fields of research that are of interest for scholars and research institutions in the LDCs, perhaps in particular from some of the least developed ones. However, there is then some risk, of course, both that the Bank will not always get the most competent researcher for a specific project, and that the Bank will be criticized for "distorting" research in the LDCs ("research imperialism"). It is difficult to say anything in general on this issue, except that LDC researchers should be included to a considerable extent, though the quality of the research, in our judgement, is an overriding task which should not be jeopardized.

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

(a) a comparative advantage approach;

(b) an ambition to function as a residuel 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 derive 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 <u>one</u> 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, that should be emphasized. A similar attempt has, of course, been made by us.

Also the <u>procedure</u> 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 organizations 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" for the operations in the Bank. A more dispersed research portfolio would then probably follow than by adhering strictly to the principle of comparative advantages 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 the LDCs 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 research portfolio of the Bank to assure reasonable efficiency. (The studies of incentive regimes illustrate that this can successfully be done.)

1:2 How to make import, production 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 and use 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 the Bank. We therefore suggest that the Bank strengthens its capacity 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 operating 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 operating staff. This can be achieved in several different ways: people outside the Bank with a research background may be hired to operation positions in the Bank; researchers of the Bank may move over permanently to operating positions; and researchers of the Bank may take operating positions temporarily (for instance one or a few years).

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 operating officer; knowledge is often most effectively imported "in the heads" of people already when they are employed, rather than by reading research documents or doing research later on.

Research knowledge may of course also be imported by way of consultants. If the Bank is anxious that research which is financed by the organization is highly relevant for Bank operations, i.e. if a heavy emphasis is put on the principle of "the residual supply of research", 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 that are able to communicate with bank staff members. However, to avoid "inbreeding" and "one-sidedness" of Bank research, we advise that this network of consultants include scholars with different "philosophical outlook", skills and methodological preferences.

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

Production of research

The most important aspect on 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 therefore crucial that the Bank hires outstanding specialists in fields relevant for such research. What this means in concrete terms has to be considered carefully by the Bank. Our understanding 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 for 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, monetary and fiscal analysis, and labor economics. If the Bank, as we believe, should analyze "success stories" and "failure stories" of various countries, there is perhaps a case also for hiring some scholars with high competence on the analysis of economic systems and modern economic history. When research on these difficult, and partly controversial, issues is launched, it is important to use scholars with some dispersion of "philosophical" outlook on the issue of economic development.

It is possible that research in these "new" fields sometimes would gain on applying rather inter-disciplinary knowledge and methodology. Examples of such fields are research about entrepreneuship, innovation and technological development. For an important question in these fields is under what general conditions (policies, institutions, systems of incentives and perhaps also cultural characteristics) entrepreneurship, technological advance and innovations are likely to flourish. Studies of the determinants of "work ethic" is another topic that may require rather broad inter-disciplinary knowledge and methods of analysis.

It is well known, however, that inter-disciplinary studies are extremely difficult to pursue successfully. It is in fact difficult to give examples of good interdisciplinary work by groups of scholars from different disciplines. Perhaps the most efficient way to integrate knowledge from different fields "simply" is to let the integration take place "in the head" of the individual scholar. This would mean that some economists with some interests and competence also outside the field of technical economics should perhaps be hired, for instance economists with some knowledge in economic history, political science, sociology or technology. An alternative is, of course, to bring in such competence into a project by way of consultants. Joint ventures of scholars from several different fields is theoretically an attractive way to make inter-disciplinary work, though in practice very difficult to implement in a fruitful way.

There is also a strong case for the establishment of some minor research units within the operations units, such as within the regional offices. These units could in fact function as vehicles for applications of research methods that have been developed within the Bank.

Dissemination of research knowledge

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

The importance of the issue of dissemination and assimilation is underlined by the fact that one of the most striking findings of our interviews among people in the Bank is the enormous "gap" - one is tempted to say tension - between researchers and operating officers. This is nothing peculiar for the World Bank, however. Researchers within an organization that deals with "practical affairs" will probably always be somewhat of an "academic enclave" of that organization. Research is a full-time, highly specialized job, which has to be protected to a large extent from demands of practical and administrative duties. The enclave character of a research unit helps to give such a protection, and thus helps to create the "community of scholars" in which high-quality research can be generated. In fact, 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 operating staff is that researchers are usually concerned with a much longer time perspective than operating officers. The production period of research is, moreover, often so long that when results do emerge, operating people may have lost interest in the question. And sometimes the empirical data which are used in research projects may no longer reflect existing conditions. (To some extent the studies of effective protection and domestic resources costs, have suffered from this dilemma.)

Moreover, whereas researchers are usually interested in the accumulation of generalized knowledge, operating people are more interested in drawing on knowledge, in particular on rather specific "knowledge about time and place". The researcher often regards the search for the latter type of knowledge as "information gathering" rather than research. Operating people, 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 operating and policy issues.

Besides, many operating officers are not aware of, interested in, or able to absorb results of research 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.

Thus, for good research to be made, researchers should not, in our views, only do the things that are immediately relevant to operating officers, or which these think is immediately relevant. On the other hand, it is also obvious that it would be fruitful if researchers were better informed about the usefulness of research knowledge among operating people, and if researchers could communicate more effectively with operating officers. Unfortunately, there is no easy way out of the conflicting ideas about "enclave research" (free from disturbances) on the one hand and strong interaction between researchers and operating people on the other hand. We have to be satisfied with "uneasy" compromises between these conflicting objectives. Keeping this in mind, several reforms suggest themselves.

(1) 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 that not only outstanding "academic" contributions are summarized. It may also be useful to try to find out what types of research that have been successfully used in other "operating" organizations.

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

(3) That more circulation of people between research and operating 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.

(4) 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 the most efficient way is of achieving this. One possibility would be to form joint ventures between researchers and operating people - certainly in operating activities, but sometimes perhaps also in the design and to some extent in the execution of research. It is likely, however, that dissemination of methodological knowledge is most efficiently achieved if researchers and operating people jointly apply suggested methodologies to concrete issues in the operating departments - in studies of projects, sectors, markets or countries. Perhaps it would also be possible to induce operating people to make more research suggestions. More informal - i.e. less bureaucratic - procedures when drafting and planning new research projects might increase

the possibilities of operating officers to contribute to the initiating and participating of research.

(5) The suggested research units within the operation units (see p. 17) could probably also help the dissemination and assimilation of research results among the staff members of the operating units.

(6) Moreover, the earlier suggested employment of people with research background in operating 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 the operating officers. 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 operating positions, and the suggestion to recruit (more permanently) people with research background to operating positions means that some "bridges" would be built between research and operation activities. Thus, we suggest in fact that the Bank more systematically tries to build up a staff of "bridge people" in the operating departments.

It is important to realize that the limits of using more research knowledge within the Bank are probably determined more by the "absorptive capacity" of research among the operating people - 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.

I:3 Implications for research of alternative strategies for economic development

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 both a specification of what types of countries we are talking about and (b) some kind of "vision" about 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, research on nationwide planning models, and empirical studies of the process of central planning in various countries, would probably be a top priority of research. It would then, of course, be important to remember that there are substantial elements of decentralization of information, decisionmaking and initiatives 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 much research resources to the acquiring of research knowledge in this field is most likely that very few member countries of the Bank are centrally planned economies, though of course elements of it exist everywhere, because of the great role of government decisionmaking in economic matters in all countries of today. This means, of course, that the process of government decisionmaking 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 model or forecasting models.)

A more modest version of planning would be sectorial planning or programming of investment decisions, for instance in sectors where there are huge returns to scale, externalities, or (direct) intersectorial linkages. Then it may also be possible to consider aspects which are not usually well cought, 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 in fact also been so for the Bank in recent years (see section III:6), 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 in particular in less developed countries, a number of infrastructure and process industries, for which investment programming models may be particularly useful, are in fact under rather detailed central government control.[1]

1) It is sometimes argued that investment studies in labor-abundant economies should concentrate analytical work on labor-intensive types of investment. However, it is of course not less important for labor-abundant countries to economize with the scarce factor capital than to try to find labor-intensive projects.

It is important to emphasize that microeconomic investment planning models of this type, which are really tools of management decisionmaking, are quite consistent with either a market-oriented or a centrally planned macroeconomic system. In reality the research in the Bank in this field has in fact not been framed in the context of nationwide central planning models, but rather as 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 (section III:5) - a research field of great interest both in the case of centralized and decentralized versions of the development process.

Research knowledge of patterns of growth of production and trade (the field covered by section III:2) too is of considerable importance - in particular 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 by 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 the reliance of different types of policy and planning operations than in centrally planned economies. Obvious examples, beside general monetary and budget 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 for 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 (the field covered by section III:1), the same cannot be said about research on incentives and imperfections on 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 (the field covered by section III: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 decisionmaking, which of course is a dichotomy concerning the mode of organization rather than concerning the allocation of resources. In reality, the development strategies of various countries differ of course with respect to the allocation of resources as well. For instance, one important choice is between export-oriented (outward-looking) and import-substitution-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 to what particular consumer goods that 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 which are members of the World Bank are of course characterized by various combinations of centralized and decentralized decisionmaking, and with different strategies to employment creation, redistribution and the provision of "basic needs". Bank research should reflect this diversity concerning strategies of economic development. So should, of course, our recommendations as well.

A.L.

1978.12.14

Third draft

Grateful for immediate comments.

REPORT BY WORLD BANK PANEL ON INDUSTRIALIZATION AND TRADE

by

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CONTENTS

CHAPTER I

GENERAL ISSUES

The 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 of research but rather the <u>type</u> of research produced by the Bank and the <u>use</u> 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 - comprise approximately 1/6 of the administration budget of the Bank. About 1/4 of this analytical work seems to be formally classified as "research", of which approximately 1/7, covering the activities of 10-11 man-years of researchers, is on industry and trade, i.e. the area that is the subject of this report.

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 of the research. We shall thereafter make som suggestions as to how the efficiency of various research activities of the Bank - import, production and application of research - may be improved. In the next section the implications for Bank research of alternative strategies for economic development are considered. Against the background of the discussion in these various sections we shall finally make som preliminary observations about the research priorities of the Bank, as a prelude to the rather detailed discussion of that issue in the subsequent chapters of the report.

I:1 Bank research - 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 of Bank research is supposed to be. A hint on these issues is provided by a formulation in our terms of reference according to which the research objectives of the Bank include the task "to support all aspects of Bank operations ..." and "to broaden our understanding of the development process". Thus, the audience of Bank research should, according to these statements, be both Bank representatives who are responsible for general policy issues and staff members who are engaged in more narrowly defined Bank operations. However, it is obvious that Bank research may be of great relevance also for people outside the Bank. In fact, the Bank has regarded it as a duty both to stimulate research in the less developed countries, and to produce research knowledge for people outside the Bank, including the "community of scholars" around the world in the field of development economics.

When going deeper into the issues of "why" and "for whom", it is tempting to apply the notion of the comparative advantage of the Bank as a research unit, and hence construct 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. 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.

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.

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

(c) The Bank should try to produce statistical data and other information, where such information is more readily (cheaply) available to the Bank than to other organizations.

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

Aspect (a) would imply that the Bank continues 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 fact, 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 an emphasis on effective protection and domestic resource costs (the field of Bela Balassa), growth patterns and sources of growth (the field of Hollis Chenery, Don Keesing and others), 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 the "revealed" research priorities of the Bank during recent years simply by looking at the "background" of the economists who had already been hired by the Bank!

By contrast, it would seem that projects dominated by "outsiders" have usually not succeeded as well as projects 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.

Aspect (b) suggests research in fields where knowledge is acquired (rather "automatically") by the Bank in connection to the operating and the policy advice activities of the Bank. 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 that is useful for research activities is acquired by Bank operations is probably comparative studies of government policies and regulations in different countries. And a third quite related example is comparative studies of institutions and incentive systems 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 sector reviews and studies of small-scale enterprises, state enterprises and financial intermediaries, for instance within the Industrial and Finance Division (IFD). Ongoing analytical work within the Bank on trends and problems on international markets and on national economies could also be a foundation for more systematic research efforts within the Bank.

Aspect (c) would suggest a shift in Bank research activities to more collection, processing and publishing of data that comes 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 dominant "data bank" in the development field - would require truly enormous resources. We know how great such a task is already on a national scale. To do the same thing for about 100 countries would therefore require a formidable effort. It may therefore be reasonable to limit demands on the Bank in this field to take a greater responsibility for the data which it actually collects and uses in its own research and surveys, and to make these data available for outsiders to a large extent. Resource constraints make it necessary, we believe, to take a rather selective approach to the collection, processing and publication of data.

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

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 activities, to be efficient, often require a rather strong and permanent research organization, which the social science departments of universities often do not have.

A strict adherence to the principle of "comparative advantage" in the production of research would imply that types of research knowledge which are not effectively produced by the Bank - regardless of how important they are for Bank operations - should be imported rather than produced by the Bank. 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 of (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 <u>production</u> of research knowledge). One reason is the specificity of required knowledge, another is the lack of interest outside the Bank for certain types of research knowledge that the Bank needs (demands). Thus, in order to satisfy its own needs for research knowledge, the Bank certainly has to perform also a role as <u>residual supplier of research</u> 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 activities of the Bank as a 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 comes out to the effect that the techniques may have been somewhat "overelaborated" for the purpose of Bank operations.

Another inference of the principle of "residual supplier of research" is probably that it motivates research concerning large investment projects (in particular where the Bank is involved as a lender), including analyses of the externalities (such as learning by doing) of the projects, returns to scale and the 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 (IPD). It would appear, however, that the operational staff of the Bank, particularly in the regional offices, has usually not found the research done in this field very helpful.

However, we would argue that an adherence to the principle of "the residual supplier of research" motivates studies also concerning problems about which Bank research has not so far been strongly involved, but which in our judgement reflect severe bottlenecks for economic development in many LDCs. This brings us back to a recommendation of some research that earlier was motivated by "the comparative advantage principle", i.e. research on (1) comparative government policies and regulations, (2) institutions, and (3) incentive regimes not only in product markets (where the Bank has done considerable research efforts) but also in factor markets. The argument for more research efforts on the roles of institutions is probably particularly strong if the Bank would choose to concentrate research more than earlier on the least developed LDCs, 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 stronger emphasis on studies of the consequences of alternative institutional arrangements, 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 on such issues by way of its operations, or because knowledge is needed in these fields for Bank operations (and is not easily imported). A rather special reason why the Bank may be a suitable place for institutional research, relative to university institutions, is that institutional research so far has not gained a very high "status" in the academic world. Perhaps the World Bank could to some extent break out of the academic status ranking system?

We shall later on (Chapter II) discuss in some detail both why more research of these types are important and how they 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, in many cases research done within the Bank may be more "visible" to the operations people in the Bank than research done elsewhere, even when the work is identical. Moreover, the only efficient way of making practitioners in an organization aware of what researchers outside the organization are doing is probably to have a number of researchers within their organization (a point to be developed in the next section).

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. The presence of good researchers, like competent people in general, helps to set standards of performance 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 possibilities of the Bank to work efficiently, and perhaps even to "survive" in a long-run perspective.

In other words, the research of the Bank should not be subordinate only to the (relative) efficiency of the Bank as a producer of the research, or to the immediate demands of the operating units of the Bank, but also to long-run considerations of the general competence of the Bank in various respects. 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, in our view, be interested in the "externalities" of its resarch on the research potential of the LDCs. The adherence to this principle is an argument for choosing fields of research that are of interest for scholars and research institutions in the LDCs, perhaps in particular from some of the least developed ones. However, there is then some risk, of course, both that the Bank will not always get the most competent researcher for a specific project, and that the Bank will be criticized for "distorting" research in the LDCs ("research imperialism").

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 derive 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 <u>one</u> 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, that should be emphasized. A similar attempt has, of course, to be made by our panel!

Also the <u>procedure</u> 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 organizations 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" for the operations in the Bank. A more dispersed research portfolio would then probably follow than by adhering strictly to the principle of comparative advantages 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 abition to improve the research capacity in the LDCs, participation of researchers from the LDCs 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 research portfolio of the Bank to assure reasonable efficiency. (The studies of effective protection illustrate that this can successfully be done.)

I:2 How to make import, production 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 and use 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 the Bank. We therefore suggest that the Bank strengthens its capacity 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 operating 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 operating staff. This can be achieved in several different ways: people outside the Bank with a research background may be hired to operation positions in the Bank; researchers of the Bank may move over permanently to operating positions; and researchers of the Bank may take operating positions temporarily (for instance one or a few years).

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 operating officer; knowledge is often most effectively imported "in the heads" of people already when they are employed, rather than by reading research documents or doing research later on.

Research knowledge may of course also be imported by way of consultants. If the Bank is anxious that research which is financed by the organization is highly relevant for Bank operations, i.e. if a heavy emphasis is put on the principle of "the residual supply of research", 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 that are able to communicate with bank staff members. However, to avoid "inbreeding" and "one-sidedness" of Bank research, we advise that this network of consultants include scholars with different "philosophical outlook", skills and methodological preferences.

Production of research

The most important aspect on 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 therefore crucial that the Bank hires outstanding specialists in fields relevant for such research. What this means in concrete terms has to be considered carefully by the Bank. Our understanding 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 for 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, monetary and fiscal analysis, and labor economics. If the Bank, as we believe, should analyze "success stories" and "failure stories" of various countries, there is perhaps a case also for hiring some scholars with high competence on the analysis of economic systems and modern economic history. When research on these difficult, and partly controversial, issues is launched, it is important to use scholars with some dispersion of "philosophical" outlook on the issue of economic development.

It is likely that research in these "new" fields sometimes would gain on applying rather inter-disciplinary knowledge and methodology. Examples of such fields are research about entrepreneuship, innovation and technological development. For an important question in these fields is under what general conditions (policies, institutions, systems of incentives and perhaps also cultural characteristics) entrepreneurship, technological advance and innovations are likely to flourish. Studies of the determinants of "work ethic" is another topic that may require rather broad inter-disciplinary knowledge and methods of analysis.

It is well known, however, that inter-disciplinary studies are extremely difficult to pursue successfully. Perhaps the most efficient way to integrate knowledge from different fields "simply" is to let the integration take place "in the head" of the individual scholar. This would mean that some economists with some interests and competence also outside the field of technical economics should perhaps be hired, for instance economists with some knowledge in economic history, political science, sociology or technology. An alternative is, of course, to bring in such competence into a project by way of consultants. Joint ventures of scholars from several different fields is theoretically an attractive way to make inter-disciplinary work, though in practice very difficult to implement in a fruitful way.

Dissemination of research knowledge

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

The importance of the issue of dissemination and assimilation is underlined by the fact that one of the most striking findings of our interviews among people in the Bank is the enormous "gap" - one is tempted to say tension - between researchers and operating officers. This is nothing peculiar for the World Bank, however. Researchers within an organization that deals with "practical affairs" will probably always be somewhat of an "academic enclave" of that organization. Research is a full-time, highly specialized job, which has to be protected to a large extent from demands of practical and administrative duties. The enclave character of a research unit helps to give such a protection, and thus helps to create the "community of scholars" in which high-quality research can be generated. In fact, 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 operating staff is that researchers are usually concerned with a much longer time perspective than operating officers. The production period of research is, moreover, often so long that when results do emerge, operating people may have lost interest in the question. And sometimes the empirical data which are used in research projects may no longer reflect existing conditions. (To some extent the studies of effective protection and domestic resources costs, have suffered from this dilemma.)

Moreover, whereas re-

searchers are usually interested in the accumulation of generalized knowledge, operating people are more interested in drawing on knowledge, in particular on rather specific "knowledge about time and place". The researcher often regards the search for the latter type of knowledge as "information gathering" rather than research. Operating people, 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 operating and policy issues.

Besides, many operating officers are not aware of, interested in, or able to absorb results of research 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.

Thus, for good research to be made, researchers should not, in our views, only do the things that are immediately relevant to operating officers, or which these think is immediately relevant. On the other hand, it is also obvious that it would be fruitful if researchers were better informed about the usefulness of research knowledge among operating people, and if researchers could communicate more effectively with operating officers. Unfortunately, there is no easy way out of the conflicting ideas about "enclave research" (free from disturbances) on the one hand and strong interaction between researchers and operating people on the other hand. We have to be satisfied with "uneasy" compromises between these conflicting objectives. Keeping this in mind, several reforms suggest themselves.

(1) That the researchers write, and circulate within the Bank, popularly written reports on research - concerning research produced both inside and outside the Bank. 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 that not only outstanding "academic" contributions are summarized. It may also be useful to try to find out what types of research that have been successfully used in other "operating" organizations.

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

(3) That more circulation of people between research and operating 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.

(4) 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 the most efficient way is of achieving this. One possibility would be to form joint ventures between researchers and operating people - certainly in operating activities, but sometimes perhaps also in the design and to some extent in the execution of research. It is likely, however, that dissemination of methodological knowledge is most efficiently achieved if researchers and operating people jointly apply suggested methodologies to concrete issues in the operating departments - in studies of projects, sectors, markets or countries. Perhaps it would also be possible to induce operating people to make more research suggestions. More informal - i.e. less bureaucratic - procedures when drafting and planning new research projects might increase

the possibilities of operating officers to contribute to the initiating and participating of research.

(5) The establishment of some minor research units within the operation units, such as within the regional offices, would probably also help the dissemination and assimilation of research results among the staff members of the operating units.

(6) Moreover, the earlier suggested employment of people with research background in operating 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 the operating officers. 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 operating positions, and the suggestion to recruit (more permanently) people with research background to operating positions means that some "bridges" would be built between research and operation activities. Thus, we suggest in fact that the Bank more systematically tries to build up a staff of "bridge people" in the operating departments.

It is important to realize that the limits of using more research knowledge within the Bank are probably determined more by the "absorptive capacity" of research among the operating people - 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.

I:3 Implications for research of alternative strategies for economic development

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 both a specification of what types of countries we are talking about and (b) some kind of "vision" about 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, research on nationwide planning models, and empirical studies of the process of central planning in various countries, would probably be a top priority of research. It would then, of course, be important to remember that there are substantial elements of decentralization of information, decisionmaking and initiatives 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 much research resources to the acquiring of research knowledge in this field is most likely that very few member countries of the Bank follow that kind of strategy of economic development, though of course elements of central planning exist everywhere, because of the great role of government decisionmaking in economic matters in all countries of today.

(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 model or forecasting models.)

A more modest version of central planning would be sectorial planning of investment decisions, for instance in sectors where there are huge returns to scale, externalities, or (direct) intersectorial linkages. Then it may also be possible to consider aspects which are not usually well cought, if at all, in conventional static microeconomic investment calculations. With this approach to development, or rather with this emphasis on development problems, investment programming and planning in some sectors would be of rather high priority, what it has in fact also been for the Bank in recent years (see section III:6), 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 may be of interest is that in many countries, perhaps in particular in less developed countries, a number of infrastructure and process industries, for which investment planning models may be particularly useful, are in fact under rather detailed central government control.[1]

It is important to emphasize that microeconomic investment planning models of this type, which are really tools of management decisionmaking, are quite consistent with either a

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1) It is sometimes argued that investment studies in labor-abundant economies should concentrate analytical work on labor-intensive types of investment. However, it is of course not less important for labor-abundant countries to economize with the scarce factor capital than to try to find labor-intensive projects.

market-oriented or a centrally planned macroeconomic system. In reality the research in the Bank in this field has in fact not been framed in the context of nationwide central planning models, but rather as 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 (section III:5) - a research field of great interest both in the case of centralized and decentralized versions of the development process.

Research knowledge of patterns of growth of production and trade (the field covered by section III:2) too is of considerable importance - in particular 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 by 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 the reliance of different types of policy and planning operations than in centrally planned economies. Obvious examples, beside general monetary and budget 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 for 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 (the field covered by section III:1), the same cannot be said about research on incentives on factor markets and incentives for employees (households). Nor has there been much emphasis in Bank research on the importance for economic development of institutional arrangements (the field covered by section III: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 decisionmaking. In reality, the development strategies of various countries differ of course in other dimensions as well. For instance, one important choice is between export-oriented (outward-looking) and import-substitution-oriented (inward-looking) strategies of economic development - a topic highlighted by Bank research on trade policy incentives and patterns of growth.

Another important dichotomy of development strategies 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, finally, is between countries that rely on a rather passive attitude to what particular consumer goods that 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 which are members of the World Bank are of course characterized by various combinations of centralized and decentralized decisionmaking, and with different strategies to employment creation, redistribution and the provision of "basic needs". Bank research should reflect this diversity concerning strategies of economic development. So will our recommendations as well.

I:4 Future research priorities*

As will be seen from the evaluations of Bank research in the subsequent chapters of the report, the bulk of the research efforts in the Bank constitutes important contributions to the "global" pool of research knowledge about development problems; in fact the World Bank is no doubt a leading institution in the field of "academic research" on development problems, in particular on the empirical side. However, a large part of the research effort has

*Note to Richard Nelson:
This section should in some way be coordinated with your draft of chapter II. I have excluded pages 22-25 in my earlier draft hoping that some of it is useful for the new chapter II.

also been directly applied in operating activities, in particular perhaps the studies of effective protection and domestic resource costs, and to some extent also the studies of technological choice and investment planning in process industries.

We have argued 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 economic systems of member countries, and of views about the development process, suggest a rather "pluralistic" research program of the Bank. We have also suggested a number of general principles (motives) of Bank research:

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

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

(c) To create externalities within the Bank in the form of "sophistication" among Bank staff.

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

As we have seen, these different principles suggest somewhat different priorities, strategies and procedures of research within the Bank. However, if we would emphasize some aspects more than others, we would suggest that research is concentrated in fields where

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

(2) the Bank in its operations acquires research competence that is unique, as compared to other organizations;

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

As a preliminary way of inserting some substance into these rather general principles it may be useful to consider first the possibilities of freeing research resources from previous research areas.

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 judgement, not be taken from the research budget, but rather from the budget for operations and policy formation of the Bank.

The studies on labor-capital substitution and investment programming for process industries are also ready for the stage of application, though some "software" development is necessary to make applications more routinized. The work on these applications should probably not be done in the regions but rather in some more centrally located unit of the Bank - considering the size and complexity of analysis of this type.

Maybe a special budget item should be allocated to the regions and the other operating units for applications of research after the "pure" research phase is over.

The studies of growth patterns, and sources of growth by input-output techniques, are also mainly completed, or near completion. However, it is not clear if these studies lend themselves to application for the use by operation staff; the studies have perhaps mainly served to improve rather general knowledge about the development process.

The situation is rather different for the projects "sources of growth II" and investment programming models for nonprocess industries. In these cases a considerable amount of research remains to be done. However, for reasons hinted at above, and developed more fully in chapter II, 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 sceptical about the fruitfulness of this type of research, relative to some other fields. We would rather recommend the Bank to put resources into the following fields - for reasons developed in the subsequent chapters:

(1) Export promotion policies of the LDCs and market access in the DCs

(2) International (global) trade patterns and inter-LDC trade

(3) Factor market conditions and distortions

(4) Comparative studies of government policies (that influence industrialization and trade)

(5) Industrial strategies in non-industrial LDCs

(6) Entrepreneurship, innovation and the adaptability of production and organization of economic activity

(7) Technological change and appropriate technology

(8) Public enterprises

2

D/KSP/ 422 /79

March 15, 1979

Professor Assar Lindbeck
(Chairman)
Professor of International Economics
University of Stockholm
Fack S-104 05
Stockholm 50
SWEDEN

Dear Assar,

I have sent separately the final draft of my chapter, one copy has also been sent to Bery. I must say it has taken a lot of time to revise it, and I had postponed the unpleasant task to as late a date as I could.

In chapter II, I have one specific point of disagreement. This is the last complete para on the page 21 were two reasons for not persueing the CGE models are elaborated. Let me express my disagreement with both the points:

1. If economic reality is complex, then models need to be complex and economists better make efforts to understand them. Also onee one has gained some experience with a model, even a complex one, it is possible to understand how it hangs together. Yesterday's complex models are today's routine tools.

2. Though the process of development is an inherently disequilibrium process, it is no reasons to not to model if as such. I view the activity of building CGE models as a necessary step towards building eventually disequilibrium models later on, CGDE models, or models of temporary equilibrium will have to follow the easier to build CGE models.

A second observation regarding our general approach that has been bothering me for quite some time is brought out in VV Bhatts reaction. In his memo dated February 15.

... 2

One of the project we have criticized as being of quality below the levels of quality ~~levels~~ in other areas is his project on credit markets and public enterprises.

It seems strange to me that we recommend innovative research in soft areas of institutional issues, say that bank should not be reluctant to go into such areas for want of methodologies and then criticise for poor quality one on the few projects in the bank which go with that direction. I find this a bit inconsistent on our part.

With best regards,

Yours sincerely,

Sd/-

Kirit S. Parikh

✓ Copy to: Ms. Suman Berry. The World Bank
U.S.A.

Kirit S. Parikh

Evaluation of Bank Research on Programming in the Manufacturing Sector

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. No. 670-24) The list of reports/draft reports which were studied for this review are given in annex.1. 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 characterised 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 manufactureof ~~which~~ may exibit economies of scale. The bank 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 affect project selection and planning for the development of a sector and offer scope for co-operation among the countries of a region.

The importance of externalities resulting from economics of scale and interdependences of various kinds have been widely recognized in the literature on development economics. In fact, the "big push" theory of development is indeed based on the recognition of these interdependences and indivisibilities. When economies of scale are predominant, a developing country's market may be too inadequate for the economical 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 benefit cost analysis in 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 programme 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 the planners' task is to select a subset of these projects. In fact, it is hardly the case that a shelf full of project reports are available. Detailed project reports are expensive to prepare, particularly in terms of the skilled manpower which is almost always scarce. Moreover, when project executing authority (or Ministry) is different from the project sanctioning body (such as Finance Ministry or Planning Commission), a lot of vested interest gets created in a project by the time a detailed project report is prepared. With the pressures of such vested interests it becomes difficult to have 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 preselection 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 No. 670.24 have 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 is termed as "process industries" and the other with "non-process industries". Those industries characterised by a manufacturing process stream which is more or less continuous has a limited number of processes and where the cost of carrying mid-stream intermediate products is large are termed process industries. Examples of such industries are gas transmission, fertilizer, 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 efforts and time 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 of such problems require 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 that 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 modesl provide a tool to estimate the benefits of regional co-operation 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 per cent 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 excersises 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 is much larger and is found to be more than 25 per cent in one variant. For individual products the savings over cost of imports in some cases exceed 40 per cent.

c) Though the loss at the sector level is small for particular products, the conventional benefit cost analysis which neglects interdependence, may lead to wrong make/buy choices when the products are a part of a sector that exhibits interdependence.

d) The absolute cost of neglect of interdependence is "by no means trivial" and is "far more" then the cost of conducting studies that account for interdependence.

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 have been shown:

a) Even without obtaining globally optimum solution, use of programming models can provide a lot of insight into the nature and the costs of the various alternatives.

b) Problem with a fairly large number of integer variable can be solved with reasonable costs.

c) With a systematic exploration of break-even analysis a number of useful decision rules can be employed to eliminate a significant number of integer variables.

2. Review of Studies

2.1 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 the neglect of uncertainties. Explicit accounting for it 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 and countries and regions. These include among others fertilizer for Asean, Andean pact and India; Forest 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 underway.

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 as judged from the number and variety of applications which have been made and are planned it has been found useful too. Sometime ago, the project has reached a stage where research ends and applications begin. 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 make 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 breakeven analysis, for otherwise, applications would need, not only trained but clever people and would be severely limited in scope. The recently initiated GAMS project (No. 671-58) should go for toward providing the necessary software.

2.2 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. 120 carefully selected items were analyzed. The gains in the economies of domestic production are derived from selection of technique and scale of production taking into account possibilities of sharing capital equipment for a variety of products, as also, 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. These pose not insignificant problems. In applying a micro-analytic approach to a sectorwide study, there is a danger of getting lost in details and not seeing the woods 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 cosequences 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 benefit cost 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 decision in some of the products with consequent loss to the economy. It is shown that the ~~The~~ estimated loss entailed in using the best of the benefit-cost criteria is found to be large enough to pay back for the cost of more comprehensive analysis within one to two years. # However it may be noted that ~~F~~for fully realizing 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 affect adversely the economics of not only itself but also of the others.

Moreover, if an integrated sectoral planning procedure leads to, which it need not, additional delays in clearing projects, the benefits og such planning gets negated to some extent. Yet this is not to argue for the use exclusively of simple social benefit cost analysis. This is merely to underline the context in which sectoral programming can be fruitfully applied. Since such programming earn a handsome rate of return, there is no reason not to persue 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-labour substitution in the mechanical engineering sector "(RPO 670-23) and on "Appropriate Industrial Technology" (RPO 671-51) are such projects.

3. Evaluation

3.1 Quality of Research: The research work is certainly of a high calibre. Moreover, such research is hardly carried out outside the bank. The research output is high both in its volume and quality.

3.2 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 attain economic growth through effectively allocating resources to various sectors. Yet one need not share this vision before one considers such models to be useful. Process industries such as fertilizers, cement, etc., characterised by economies of scale and relatively a small number of plants, are the industries which are usually the ones whose development are guided and promoted by most governments of developing countries. Starting a few large industrial projects is one of the easiest thing that governments do to promote developments. The planning models developed by Bank's research has the potential to improve the rationality of government decisions in developing these sectors. Some of this potential is already realized in the numerous applications already 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.

3.3 <u>Usefulness for Bank</u>

Clearly, the studies related to specific areas must have been carried out in collaboration with the operations staff of the Regional Department concerned. The results should have been useful in guiding Bank's lending operations, provided they were available in time. But clearly lot of potential is there for such work to be useful in Bank's acitivities.

Improvement in project identification methodology in the LDCs could be of considerable significance for the Bank's operation. If more attractive projects are identified before the detailed project reports are prepared, they would be prepared 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 development in East Africa and the break up 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 which facilitate the process of realizing such cooperation. As an authority which lends money to the various countries of a region, it may have access to data and policy makers in the various countries. Moreover, as a third party its analysis may be less suspect. On the other hand Bank should also be interested in promoting such cooperation that reduces need for credit in the region.

3.4 Development of Research Capacity in LDC's

Creating research capability is a time consuming task and learning by doing is an essential element of development of research skills. Significant participation of researchers from the LDC's is an absolute requisite for successful transfer of the research to application in the LDCs.

The compulsions of time bound research programes, the inconvenience of communication across large distances and the inconvenience of 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 programme 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.

4. 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 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 an 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 MIP models needs convenient and substantial access to fairly large computers if the study is to be completed in resonable time.

Development of user oriented software which 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 modeling 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 plan an industry is to be derived from training that would be imparted in following such an approach to a set of engineers and economists in thinking more systematically about the scope of technical options in developing the sector. The engineers would be less certain of their thumbrules and the economists would be more aware of the technological constraints ~~of the system~~. 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 continuing.

The foregoing is not to deny the usefulness of attempts at extensions 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 has already 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 professionals, 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 implimentation and central planning of a sector such as the mechanical engineering industry characterised 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 appear not to be of much quantitative consequence are very comforting as one can rely on implementable, decentralized procedures for developing the industry. Yet, as emphasized by the authors of the research, the finding needs to be confirmed with further research. Moreover, as they are at some pains to 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% or more of the value added in the secotr?

The quantitative dimension of 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 persue this research to its logical conclusion. It would 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 enough 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-on 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 organisation of sectors, in the difficulties of implementation, or in the realm of selection of policy instruments should be brought to the force 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 persue, one might lay an 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 these 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 consequence. MIP models can be used to examine the consequences of different efficiencies of operation of existing plants, as was done in the Egypt fertilizer case study. Nonetheless the issues here go much deeper. To what extent delays in installation of capacity and inefficient use of installed capacity in large industrial projects are due to improper organisation or due to inappropriate objectives of the management or due to inexperience and inadequate skills? Are delays and inefficiencies inherent in the organisation 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 of being able to design effective organisational 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.

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It may be useful if we spelled out the grounds for our skepticims. General equilibrium models have obvious and important attractions as instruments of economy wide economic analysis. They have the advantage of getting explicitly into view the interconnections among economic activities and the complex and not always immediately obvious relations between prices and quantities. But associated with these advantages, the large scale compatable general equilibrium models have two disadvantages, which, in the view of this Panel, weighs heavily against them.

One is that these models tend to be so complex that the quantitative conclusions that come from them are not easy to understand, thus are difficult to evaluate and modify in light of knowledge about what the model leaves out or oversimplifies. Second, the equilibrium character of these models makes it very difficult to deal in a non-mechanical way 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 mechanically. Our concern is that economic development is an inherently disequilibrium process and this is basically what the model represses.

We recognize that not all economists share our views on these matters. We also believe it important that the Bank stay 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 lost effort. But the panel is unanamous 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 LDC's, evolving patterns of international trade, and policies of LDC's that reflect trade opportunities and constraints. Second, a set of topics concerned with economic institutions and domestic economic policies that facilitate or deter development. Third, research relating to adoption of technology, innovation, and technical change in LDC's.

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 represent continuation and strengthening of work now underway at the bank.

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 especial focus on inter L.D.C. trade. (Jürgen -- You have the new language to complete this paragraph)

We also feel that there is a need for a more systematic differentiation among LDC's with regard to industrial growth paths, trade patterns and policies to support them. The analyses should focus on three groups of countries: those which are rich in natural resources, those which are just beginning their industrialization, and those which are still extremely poor and have not yet started in developing manufacturing activities. These groups of countries are characterized by significant differences in terms of domestic serving potential, labour skills, entrepreneurship, economic structure, export orientation, the role of direct foreign investment, etc. The development model of semi-industrial economics in Latin American and East Asia, extensively studied in the past, does not cover the wide range of issues which the "late starting" LDCs must face. By analyzing them in depth, 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 intepretation into the world economy; and it should 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 in connection to the reallocation of 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 that the developed countries could do for the less developed countries is just to adjust their own economies to the export efforts of the LDCs, to provide access to markets for these exports. However, we believe that the DCs should really themselves do this type of research. Our recommendation on this issue is therefore that the World Bank strongly advise the developed countries themselves to give high priority to research on reallocation of resources and adjustment policies in the DCs, rather than that the World Bank move heavily into that area.

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 deterence to economic efficiency associated with protection of domestic industry from external competition. We believe the time now is ripe for the bank to shift the focus from the domestic 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 study of industrial organization in LDC's and exploration of the effect of different kinds of policies on organizations and on economic performance. Many LDC's engage in price control activities, and other forms of price and input regulation. It would be fruitful, we believe, to examine the effect of these within the methodologies used to study 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 this research. 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 olgopolistic structures that mark many LDC industries.

Moreover, in many less developed countries, public enterprises are common in the provision of transport, power,and a variety of public services. Many countries are also employing public enterprise for the production of manufactured goods, particularly when significant economies of scale are involved. The question of the relation of public enterprise to market and

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CHAPTER II

PAST AND FUTURE RESEARCH ACTIVITIES

ON INDUSTRY AND TRADE: A SUMMARY

II:1 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 the Appendix II. For convenience in review, the panel divided up past research into six broad clusters. These are briefly described below. The several sections of Appendix I provide more detailed descriptions of the research in each of the areas.

a) 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, in spurring efficient and rapid economic growth in LDCs. 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. And finally, this research has been found quite helpful by the Bank's operational staff, who is applying both the findings and the methodology in country economic reports, policy analyses and in-house estimates of incentives, let alone initiatives for additional research in this field.

b) Comparative Advantage, Trade Patterns, 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, this objective. The failures are regrettably 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 (RPOs 670-07 and 671-79) has not yet added much to existing knowledge about the development process. Moreover, skepticism about its usefulness predominates within the operational staff. It still remains to be seen whether the multi-sector programming model applied in the ongoing RPO 671-79 will produce significant empirical results to an extent which could not have been obtained at lower cost from simpler macro-economic formulations.

c) Export Promotion Policies in the LDCs and Access to Markets in the DCs

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+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 reporting studies. 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.

As was the case with the projects discussed under the second cluster, the research methodologies and styles of the projects considered here has 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. High praise deserves, by contrast, the project on industrialization and trade policies. 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 for 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 as a residual supplier of research in this field.

d) Small Enterprises, Credit Markets, Public Enterprises

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

A large part 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. 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 struggling towards appropriate methodologies, but has not yet securely found many. We also notice that research on capital market imperfections and public enterprises put too much emphasis on the experience of one country, namely India. And while the quality of the research done so far is good by international standards, 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.

e) Capital Utilization, Capital-Labour 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 well-defined methodologies and has tested 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 670-54 being the weakest one. Three non-RPO studies are completed as well.

The policy thrust of the research has been provided by the observation that labor is cheap and capital expensive in developing countries relative to developed ones, that this ought to be reflected in use of more labor-intensive techniques, but while this has been happening to some degree it still is possible and desirable that the techniques employed be more frugal in 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.

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. The Panel finds the recent work exploring in great detail the scope of capital labor substitution in particular technologies important and illuminating, but running into diminishing returns as a research endeavor. The research on the design capabilities of domestic capital goods producers (RPO 671-51) is promising, and may lead to important further research.

f) Industrial Programming; Studies of Process Industries

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 both process and non-process industries.

The research has estimated the size of scale economies in certain process industries such as fertilizers, cement, pulp and paper, forest products, energy etc. (which is 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 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's 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 the extention attempts. As far as research on non-process industries (namely the study of the Korean Mechanical Engineering Sector) is concerned, its generalizability has not yet been proved.

II:2 Overall Evaluation

The Panel attempted to evaluate the research undertaken by the Bank 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 processes 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 advantages? To what extent did Bank research proceed in conscious awareness of the research that had been done and was going on elsewhere? Other criteria related to Bank research viewed as a contribution to the applied objectives of the Bank. How useful has the research been 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 probe at 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?

The several sections of Appendix I go over these questions field by field. The Panel noted significant differences in the overall quality and relevance of Bank research in the different fields, and the more fine-grained evaluations also differ from field to field. However, there were certain general and common judgements 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 imitating and, in its applied component, 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 that the outward looking development policies were both feasible and highly effective. More recently research at the Bank has contributed importantly to understanding of changing patterns of LDC exports. Work at the Bank has shown how resource allocation patterns within a country relate to the country characteristics including its income level, market size, and policy orientation. Research at the Bank on intensity and efficiency of use of capital and labor has significantly enriched understanding of the forces and work on those variables; more recent work at the Bank has illuminated and 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; 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 a little bit, but not very much. 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 of the basic ideas and understanding that emanate from a research tradition on their own thinking regarding the applied problems they faced, but to discuss the contribution of research in terms of detailed pieces of analysis, or data, that were used concretely and specifically in decision-making. In our judgement 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 bank operations aimed to influence 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, as well as 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 have influenced the way Bank officials view appropriate economic development policy-making and set their positions in bargaining with LDC officials.

We feel ourselves in an even weaker position regarding the ability 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 (and it is certainly not everywhere) 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 able to acquire very little direct confirmation of these conjectures. On the other hand, we noticed that some shifts in Bank's 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 that has come about because of participation of local scholars and research institutions in completed or ongoing Bank projects, we found some cases in which further research in the countries concerned was stimulated. Generally speaking, the Bank policy of working with researchers and institutions of developing countries when this advances the research should also be recognized as enhancing of 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 suggests 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 which have been motivated largely by "pure" interests without much in the way of clear-cut connections with important policy questions, nor have we been much impressed with Bank projects that appeared to have been motivated largely by a particular policy interest or concern but which did not involve much 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, 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 prerequisite 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.

II:3 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, suggest a rather "pluralistic" research program of the Bank.

We have suggested four general principles (motives) of Bank research:

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

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

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 judgements 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 of course 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

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

(2) the Bank in its operations acquire research competence that is unique;

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

Needless to say, a basic requirement in all three cases is that the Bank has, or is 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 rather general and less detailed statement of research priorities. As a preliminary way of inserting some substance into the rather general principles presented above, 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 now is 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 judgement, 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 applications 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 operations work.

Similarly, while Bank research on patterns of growth and sources of growth, based on regression and input-output analysis, have 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 if these studies lend themselves to application for the use by operation 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 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 applicants.

We have the same judgement regarding Bank research on process industry investment programming, 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.

In the case of both labor capital-substitutions 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 the economy-wide models based on a computable general equilibrium framework. Most of us doubt that these bodies of research will contribute much directly to understanding relevant to policymaking. 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 and the complex and not always immediately obvious relations between prices and quantities. But associated with these advantages, the large scale compatible general equilibrium models have two disadvantages, which, in the view of this Panel, weighs heavily against them.

One is that these models tend to be so complex that the quantitative conclusions that come from them are not easy to understand, and thus are difficult to evaluate and modify in light of knowledge about what the model leaves out or oversimplifies. Second, the equilibrium character of these models makes it very difficult to deal in a non-mechanical way 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 mechanically. Our concern is that economic development is an inherently disequilibrium process and this is basically what the model represses.

We recognize that not all economists share our views on these matters. We also believe it important that the Bank stay 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 lost 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 facilitate or deter development. Third, research relating to adoption of technology, innovation, and technical change in developing countries.

a) 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 represent continuation and strengthening of work now underway at the Bank.

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.

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. The analyses should focus on three groups of countries: those which are rich in natural resources, those which are just beginning their industrialization, and those which are still extremely poor and have not yet started in developing manufacturing activities. 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 American and East Asia, extensively studied in the past, does not cover the wide range of issues which the "late starting" LDCs must face. By analyzing them in depth, 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 integraton into the world economy; and it should 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 in connection to the reallocation of 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 that the developed countries could do for the less developed countries is just to adjust their own economies to the export efforts of the LDCs, to provide access to markets for these exports. However, we believe that the DCs should really themselves do this type of research. Our recommendation on this issue is therefore that the World Bank strongly advise the developed countries themselves to give high priority to research on reallocation of resources and adjustment policies in the DCs, rather than that the World Bank move heavily into that area.

b) 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 deterence to economic efficiency associated with protection of domestic industry from external competition. We believe the time now is ripe for the Bank to shift the focus from the domestic 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 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 effect of these within the methodologies used to study 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 this research. 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 less developed countries, public enterprises are common in the provision of transport, power, and a variety of other public services. Many countries are also employing public enterprise 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.

c) 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 self-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 going on within a 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 in the past done well and poorly 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 knowledgable 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 presently 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 over into a field, the appointments can be made at the junior level. 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 which 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 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. However, we believe that this would be 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 of highly competent persons. This is, in fact, a prerequisite for shifting research to the areas which, according to our opinion, should be given higher priority in the future than in the past. It will, of course, be the size of these new resources that sets the limits for how many new departures may be envisaged.

2nd draft

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CHAPTER II

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

II:1 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. For convenience in review, the panel divided up past research into six broad clusters. These are briefly described below. The several sections of chapter III provide more detailed descriptions of the research in each of the areas.

One cluster of Bank projects has been concerned with incentive policies and development strategies; 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. Most projects were completed successfully. The study on economies of scale and tariff levels was never completed, what is a pity as the subject matter is complementary to the research done in the other projects. An 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 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, in spurring efficient and rapid economic growth. 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. And finally, this research has been found quite helpful by the Bank's operational staff, who is applying both the findings and the methodology in country economic reports, policy analyses and in-house estimates of incentives, let alone initiatives for additional research in this field.

Another cluster of projects has been concerned with comparative advantage, trade patterns, and economic growth. These projects include RPOs 670-07 (International Model), 670-19 (Expansion in Manufacturing for Exports in Developing Countries), 670-79 (Economic Development of East and South-east 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, this objective. The failures are regrettably 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 differ greatly in their methodologies. Some of the projects 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 comprehensive of the projects were those that employed and elaborated an accounting framework based on sector-specific supply-demand identities for analyzing the nature of modern industrial growth. The projects that worked within a less formalized framework did not yield interesting conclusions. The research guided by the more ambitious general equilibrium conception has not yet added much to existing knowledge, nor has it produced sensible empirical results to an extent which could not have been obtained at lower cost from simple macro-economic formulations. The operational staff does not attach a high utility to this type of research. And it is not clear to us that the Bank has a comparative advantage in this field.

A third cluster of research has referred to export promotion policies in the developing countries and access to markets in the industrial countries. It included 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+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). All but the first two projects are still under way; five non-RPO analyses have also been finished. All of these projects aim at testing the hypothesis, prominent at the Bank, that successful expansion and diversification of exports is a key characteristic of many recent development experiences. The research includes data compilations, surveys of selected industries, analyses of commodity markets, analyses of problems of import restrictions by the developed countries on the less developed countries' 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 the less developed countries. As was the case with the projects discussed in the paragraph above, the research methodologies and styles of the projects considered has 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 promising in itself as a case study of a less successful country, has been disappointing from both the methodological and the policy analysis point of view. High praise deserves the project on industrialization and trade policies; 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 turn out 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 will fill a large gap in existing information on the marketing of exportables. The two ongoing projects on import market penetration in developed countries reflect a serious attempt for understanding the political economy of trade proteccionism; while the topic is important from the export-oriented developing countries' point of view as well, it is questionable that the Bank has a comparative advantage of conducting large-scale research in this field.

Research in a fourth cluster relates to 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), 671-69 (Capital Market Imperfections), and 671-11 (Manufacturing Structure and Practices in Public Manufacturing Enterprises), in addition to a number of non-RPO studies. The importance of these topics for the Bank's lending operations and advisory role in developing countries is obvious. A large part 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 export promotion which proceeded under the presumption that the objective was clear enough and that the task was to find the appropriate instruments, these projects have faced much more uncertainty regarding what ought to be achieved. 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 struggling towards appropriate methodologies, but has not yet securely found many. We also notice that research on capital market imperfections and public enterprises put too much emphasis on the experience of one country, namely India. And while the quality of the research done so far is good by international standards, 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.

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 (the fifth cluster) has proceeded within well-defined methodologies and has tested concrete hypotheses. Included here are the RPO projects 670-54 (Employment and Capital-Labor Substitution), 670-23 (Scope for Capital-Labor Substitution in the Mechanical Engineering Industry), and 671-51 (Appropriate Industrial Technology), most of them are completed. Of three non-RPO studies two are completed as well. The policy thrust of the research has been provided by the observation that labor is cheap and capital expensive in less developed countries relative to developed ones, that this ought to be reflected in use of more labor-intensive techniques, but while this has been happening to some degree it still is possible and desirable that the techniques employed be more frugal in 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. Research has been concerned with market and other forces that explain the prevailing situation, and with policies that could improve the environment so the choice of technique could be made more appropriately, techniques used more efficiently, and appropriate adaptation and learning proceed more effectively. The panel finds the recent work exploring in great detail the scope of capital labor substitution in particular technologies important and illuminating, but running into diminishing returns as a research endeavor. The work on appropriate industrial technology, particularly the research on the design capabilities of domestic capital goods producers, is promising, and may lead to important further research.

The sixth cluster of Bank research is on investment programming and has many connections with research on the topic considered in the paragraph 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 both process and non-process industries. The research has estimated the size of scale economies in certain process industries (which is important in some cases). Moreover, it has tested the utility and feasibility of using formal programming models in guiding investment decisions (with encouraging results). The research also has considered some of the implications of economies of scale and strong inter-activity interdependence for regional cooperation. By and large, the research has been successfully carried out. It made a significant methodological contribution, it has the potential to improve the rationality of government decisions in developing large process industries, and it may have been useful in guiding Bank's ledning operations (provided they were available in time and could be understood by the operational staff). It is our impression that a stage has been reached where the methodology developed for process industries has to be disseminated effectively for application.

II:2 Overall Evaluation

The panel attempted to evaluate the research undertaken by the Bank 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 processes 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 advantages? To what extent did Bank research proceed in conscious awareness of the research that had been done and was going on elsewhere? Other criteria related to Bank research viewed as a contribution 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 policymakers in the less developed countries? What contribution has the Bank research program made to the building up of indigenous research capabilities within the less developed countries?

Finally, we attempted to probe at 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?

The several sections of chapter III go over these questions field by field. The panel noted significant differences in the overall quality and relevance of Bank research in the different fields, and the more fine-grained evaluations also differ from field to field. However, there were certain general and common judgements 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 imitating and, in its applied component, generally complementary to the research in the field undertaken elsewhere. Bank research, being mainly empirically oriented, has made outstanding contributions to knowledge about the structure of incentives bearing on business firm decisionmaking 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 that outward looking development policies were both feasible and highly effective. More recently research at the Bank has contributed importantly to understanding of changing patterns of exports from the developing countries. Work at the Bank has shown how resource allocation patterns within a country relate to the country characteristics including its income level, market size, and policy orientation. Research at the Bank on intensity and efficiency of use of capital and labor has significantly enriched understanding of the forces and work on those variables; more recent work at the Bank has illuminated and 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 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. 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 decisionmaking, or on policymaking in the developing countries, or upon the strength of the research communities in the less developed 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 a little bit, but not very much. 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 elsewhere) not to talk about the influence of the basic ideas and understandings that emanate from a research tradition on their own thinking regarding the applied problems they faced, but to discuss the contribution of research in terms of detailed pieces of analysis, or data, that were used concretely and specifically in decisionmaking. In our judgement 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 bank operations aimed to influence 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, as well as 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 confirmation of these conjectures. On the other hand, we noticed that some shifts in Bank's 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 the developing 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 that has come about because of participation of these countries' scholars and research institutions in completed or ongoing Bank projects, we found some cases in which further research in the countries concerned was stimulated. Generally speaking, the Bank policy of working with researchers and institutions of developing countries when this advances the research should also be recognized as enhancing of 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 suggests 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. By and large we have not been impressed with the success of Bank projects which have been motivated largely by "pure" interests without much in the way of clear-cut connections with important policy questions, nor have we been much impressed with Bank projects that appeared to have been motivated largely by a particular policy interest or concern but which did not involve much 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, 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 prerequisite 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 ends 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.

II:3 Future research priorities

As will be seen from the rather detailed evaluations of Bank research in the last chapter of the report, the bulk of the research efforts of the Bank on industry and trade constitutes important contributions to the "global" pool of research knowledge about development problems. Moreover, a considerable part of the research of the Bank has also been applied in policy advicing and operating activities of the Bank. This holds in particular perhaps for the studies of effective protection and domestic resource costs, and to some extent also the studies of technological choice and investment programming in process industries.

We have argued 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, suggest a rather "pluralistic" research program of the Bank.

We have suggested four general principles (motives) of Bank research:

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

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

(c) To create externalities within the Bank in the form of "sophistication" among Bank staff.

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

(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 judgements 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

In the case of research on trade policy incentives, it is resasonable 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 judgement, 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 applications 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 operations work.

Similarly, while Bank research on patterns of growth and sources of growth, based on regression and input-output analysis, have 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 if these studies lend themselves to application for the use by operation 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 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 judgement regarding Bank research on process industry investment programming, 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.

In the case of both labor capital substitutions 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 the economy-wide models based on a computable general equilibrium framework. Most of us doubt that these bodies of research will contribute much directly to understanding relevant to policymaking. 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 sceptical about the fruitfulness of this type of research, relative to some other fields. On the other hand, the work is methodologically exciting and on the frontiers, and enables the Bank to attract and hold several very well-tooled economists. The arguments for continuation of these projects it seems to us must rest on the importance to the Bank of having on its research staff several economists who are technically very skilled.

However, regardless whether the Bank wants to continue research in this field or not, we recommend the Bank to shift the emphasis of research to some other fields, such as the following ones:

(1) Export promotion policies of the LDCs and market access in the DCs
(2) International (global) trade patterns and inter-LDC trade
(3) Factor market conditions and distortions
(4) Comparative studies of government policies (that influence industrialization and trade)
(5) Industrial strategies in non-industrial LDCs

(6) Entrepreneurship, innovation and the adaptability of production and organization of economic activity
(7) Technological change and appropriate technology
(8) Public enterprises

We are not sure which of these sector fields that should be given the highest priority. Among the trade-related fields (points 1-2), perhaps the second one - trade patterns and inter-LDC trade - should be given the edge. The reason is that we forecast the possibility of a considerable attempt to expand inter-LDC trade in the 1980s, and that this type of trade is likely to encounter new and poorly understood problems. For instance, while the successful expansion of export of manufacturing goods of some LDCs to developed countries has largely been promoted by "ready-made" marketing firms in the DCs, efficient marketing systems for inter-LDC trade do not yet exist.

We have also seriously considered the idea that the Bank should launch a major research effort to the question of adjustment mechanisms in connection to the reallocation of 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 that the developed countries could do for the less developed countries is just to adjust their own economies to the export efforts of the LDCs, to provide access to markets for these exports. However, we believe that the DCs should really themselves do this type of research. Our recommendation on this issue is therefore that the World Bank strongly advise the developed countries themselves to give high priority to research on reallocation of resources and adjustment policies in the DCs, rather than that the World Bank moves heavily into that area.

Among production-oriented fields (points 3-8), many of us would stress problems of factor market distortions (point 3); enterpreneurship, innovation and adaptation (field 6), and technological change and appropriate technology (field 7). The reason is, in our view, that in decentralized market economies with a considerable scope for government decisionmaking, governments can do much for releasing, or destroying, decentralized, productive incentives by way of incentive policies in a broad sense - tariffs, taxes, subsidies, wage and labor market regulations, licencing systems, training, technological research and various types of controls - as well as by helping to develop institutions that are conducive to vigorous entrepreneurship and a sharing of the fruits of economic development by broad groups of the population.

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.

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.

Moreover, in many less developed countries, public enterprises are common in the provision of transport, power, and a variety of public services. Many countries are also employing public enterprise for the production of manufactured goods, particularly when significant economies of scale are involved. The question of the relation of public enterprise to market and to higher political authority, and more general issues relating to the motivation systems influencing decisionmaking in public enterprises, strikes us as important to study, probably on a comparative basis. The World Bank has initiated some research in this field. We urge that the field be given quite high priority.

Another broad set of subjects to which we think priority should be given involves 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 these 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 going on within a country's engineering schools.

It would be very interesting 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 domstically owned firms and subsidiaries of foreign corporations? Between private and public firms? We think it of high priority that the Bank begins to study these questions.

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 contemplates a shift of emphasis of research to the new fields suggested here, it would probably be a good idea to appoint an ad hoc group of researchers with the task of undertaking a research program in some of the fields suggested here, for instance concerning factor market distortions, the functioning of labor and capital markets, entrepreneurship, innovation and technological development and adaptation. As we have indicated, such research should probably often use the technique of comparative studies of nations. The ad hoc research planning committee should include outstanding researchers outside as well as inside the Bank. Example of types of scholars are specialists in industrial organization, technological development, innovation, credit and labor market analysis (labor economics). It is, in our judgement important to include people with a strong theoretical and analytical competence, rather than people that have studied institutions on a more descriptive way.

To avoid that research in the new fields which 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 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. However, we believe that this would be 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 of highly competent persons. This is, in fact, a prerequisite for shifting research to the areas which, according to our opinion, should be given higher priority in the future than in the past. It will, of course, be the size of these new resources that sets the limits for how many new departures may be envisaged.

First Draft

CHAPTER II

PAST AND FUTURE RESEARCH ACTIVITIES ON INDUSTRY AND TRADE AT THE WORLD BANK -- A SUMMARY

I. 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. For convenience in review, the panel divided up past research into six broad clusters. These are briefly described below. The several sections of Chapter III provide more detailed descriptions of the research in each of the areas.

One cluster of Bank projects has been concerned with incentive policies, and development strategies; we also placed in this group studies of economic integration. Included here are 670-01 (economies of scale and tariff levels), 670-87 (industrial policies and economic integration in West Africa), 671-10 (promotion of non-traditional exports) and 671-75 (international trade policy for the development of Bangladesh), and several other 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 effect of various government policies on incentive regimes. They examine 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 overall growth in developing countries. Considerable use has been made of the concepts of effective protection rates and domestic resource costs in examining various government policies. Incentive structures in a number of different countries have been analyzed and compared employing these concepts. There have 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. These projects aimed to provide government officials of less developed countries with guidelines for appraising the allocative, growth, distributional, and balance-of-payments impacts of various incentive regimes. By and large the conclusions of the projects can be viewed as supporting the efficacy of keeping import protection moderate and export incentives strong in spurring efficient and rapid growth.

Another cluster of projects has been concerned with comparative advantage, trade patterns, and economic growth. These projects include 670-07 (the 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). The unifying theme is the objective of explaining the pattern of resource allocation within and between countries, and trade patterns, as a function of various country characteristics, including policies employed. The projects differ greatly in their methodologies. Some of the projects 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 and useful of the projects were those that employed and elaborated the simple quantitative framework that Chenery and his colleagues have developed.

The projects that worked within a looser framework did not yield interesting conclusions. The research guided by the more elaborate general equilibrium conception has not yet produced interesting empirical results.

The research area "export promotion policies in the LDC's and access to markets in the DC's" included projects 671-56 (marketing manufactured exports), 670-20 (industrialization and trade policy for the 1970's), 670-21 (export promotion and preferences: India), 671-35 (export incentives in developing countries), 671-67 (effects of increased imports of manufactured goods from developing countries in the United States) and 671-68 (key institutions and the expansion of manufacturing exports). All of these projects reflect belief, prominent at the Bank, that successful achievement of exports is a key characteristic of many recent development experiences. As was the case with the projects discussed in the paragraph above, the research methodologies and styles of the projects considered has differed widely. The research includes data compilations, surveys of selected industries, analysis of commodity markets, analyses of problems of input restrictions by the developed countries on the less developed countries' manufactured goods, consideration of national policies and institutions for trade promotion in the LDC's, and analyses of the overall environment for the exports of the less developed countries. The panel considered that while the quality of the work has been uneven, most of the projects have generated interesting and useful results. The current portfolio of projects looks particularly promising, and should be followed through forcefully.

Research under the topic "small enterprises, credit markets, and public enterprises" shows a similar pragmatism regarding methodology. The projects here include 670-77 (financing of small-scale industries), 671-59 (small-scale enterprise development), 671-69 (capital market imperfections), and 671-11 (manufacturing structure and practices in public manufacturing enterprises). In contrast with the work on export promotion which proceeded under the presumption that the objective was clear enough and that the task was to find the appropriate instruments, these projects have proceeded with much more uncertainty regarding what ought to be achieved (e.g. whether small industry ought to be supported or not). They represent efforts by the Bank to explore questions that previously have not been very well considered in the general literature on economic development. While applauding the willingness to probe these important issues, the panel recognizes that research on these topics is struggling towards appropriate methodologies, but has not yet securely found many. It may be some time before the Bank develops the capability to do first rate studies of economic institutions and their influence on development.

Like the work on export promotion, and unlike the research on institutional conditions and institutional reforms, Bank research on capital utilization, capital-labor substitution, and technological change has proceeded within well-defined methodologies, and with a well-defined point of view. Included here are project 670-54 (employment and capital-labor substitution), 670-23 (scope for capital-labor substitution in the mechanical engineering industry), 671-51 (appropriate industrial technology). The policy thrust of the research has been provided by the belief that labor is cheap and capital expensive in less developed countries relative to developed ones, that this ought to be reflected in use of more labor-intensive techniques, but while this has been happening to some degree it still is possible and desirable that the techniques employed be more frugal in use of capital. Research has been concerned with market and other forces that explain the prevailing situation, and with policies that could improve the environment so the choice of technique could be made more appropriately, techniques used more efficiently, and appropriate adaptation and learning proceed more effectively. The panel finds the recent work exploring in great detail the scope of capital labor substitution in particular technologies important and illuminating, but running into diminishing returns as a research endeavor. The work on appropriate industrial technology, particularly the research on the design capabilities of domestic capital goods producers, is promising, and may lead to important further research.

Bank research on investment programming has many connections with research on the topic considered in the paragraph above, but has focused on the development of optimization models. The major project included here is 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 both process and non-process industries. The research has provided evidence of the importance of scale economies in certain process industries, and indicated the utility and feasibility of using formal programming models in guiding investment decisions. The research also has considered some of the implications of economies of scale and strong inter-activity interdependence for regional cooperation. But this research, like that on capital-labor substitution, may be running into diminishing returns.

II. Overall Evaluation

The panel attempted to evaluate the research undertaken by the Bank 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 understanding of the economic development processes 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 advantages? To what extent did Bank research proceed in conscious awareness of the research that had been done and was going on elsewhere? Other criteria related to Bank research viewed as a contribution to the applied objectives of the Bank. How useful has the research been in guiding Bank decisionmaking, either regarding lending operations or regarding policy advice? How useful has the research of the Bank been to policymakers in the less developed countries? What contribution has the Bank research program made to the building up of indigenous research capabilities within the less developed countries?

Finally, we attempted to probe at 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?

The several sections of Chapter III go over these questions field by field. The panel noted significant differences in the overall quality and relevance of Bank research in the different 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, the Bank clearly ranks as one of the most distinguished development research organizations in the world. Bank research has made outstanding contributions to knowledge about the structure of incentives bearing on business firm decisionmaking in less developed countries, particularly regarding the incentive effects of tariff regimes. Bank research has been in the forefront of scholarship positing and supporting that outward looking development policies were both feasible and highly effective. More recently research at the Bank has contributed importantly to understanding of changing patterns of exports from the less developed countries. Work at the Bank has contributed to understanding of how resource allocation patterns within a country relate to the country characteristics including its income level, size, and characteristics of its policies, and thus has facilitated ability to predict, at least in rough form, how economies evolve. Research at the Bank on intensity and efficiency of use of capital and labor has significantly enriched understanding of the forces and work on those variables; more recent work at the Bank has illuminated and 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 and the implementation of these methods, while not yet bearing much fruit, has explored and pushed forth the state of the art. Bank research on institutions, 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.

By and large Bank research has reflected its comparative advantage. Much of what was done could not have been done at all, or would have been very difficult to do, in a university setting. 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 decisionmaking, or on policymaking in the less developed countries, or upon the strength of the research communities in the less developed 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 a little bit, but not very much. 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 of the basic ideas and understandings that emanate from a research tradition on their own thinking regarding the applied problems they faced, but to discuss the contribution of research in terms of detailed pieces of analysis, or data, that were used concretely and specifically in decisionmaking. In our judgement the influence of ideas and concepts on policy making usually is much more important than the influence of particular facts or numbers 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 bank operations aimed to influence 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, as well as well as the specific numbers, 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 for countries to consider. Both of these notions seemed to be mentally connected with belief that decisionmakers 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. It is our conjecture that this kind of research may in fact have been more influential regarding decisions on particular loans than the more general analyses done by Bank researchers. However, if the focus is on the influence of Bank research on the way Bank officials view appropriate economic development policymaking and set their positions in bargaining with LDC officials, as stated above we believe that it is the more sweeping ideas and documentation for these that has had the greatest influence.

We feel ourselves in an even weaker position regarding the ability to judge the impact of Bank research on policymaking in the less developed countries. We would conjecture that all of our remarks above obtain. Where (and it is certainly not everywhere) 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 effect on Bank-LDC dialogue. But we are able to acquire very little direct confirmation of these conjectures.

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 the less developed 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 LDC's, or case studies of particular industries or policies, have involved researchers in the LDC's being studied. 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. We have no way of assessing the importance of the contributions to the growth of LDC research capabilities that has come about because of participation of LDC scholars and research institutions in ongoing Bank projects. But the Bank policy of working with LDC researchers and institutions when this advances the research should also be recognized as enhancing of the research capabilities in the less developed countries.

Our relative assessments of the research projects that have been undertaken by the bank suggests 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 policy questions. By and large we have not been impressed with the success of Bank projects which have been motivated largely by methodological interests without much in the way of clear-cut connections with important policy questions, nor have we been much impressed with Bank projects that appeared to have been motivated largely by a particular policy interest or concern but which did not involve much analytic structuring. We recognize that the Bank's research portfolio should contain a diverse mix of projects, involving different degrees of farming out. We would call attention, however, to the fact that quite detailed attention and involvement of a senior Bank researcher in a project has in the past been almost a prerequisite 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 ends of that spectrum, but rather in projects where intellectual interests and policy concerns have come together.

III. Future Research Priorities

Recommendations regarding future research priorities must rest on subjective judgements regarding a number of matters. These include the importance of different kinds of research in enhancing 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 decisionmaking 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. Judgments about the relative weight to place on the different criteria are also involved. Chapter III presents our views about the kinds of research that 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 a more general and less detailed statement of research priorities.

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 sharply diminishing returns. These include such traditional and successful Bank research fields as research on rates of effective protection or subsidy, and study of patterns of growth and development. In both of these fields Bank research has broken new ground, but the ground now is well broken. The idea of effective rates of protection now is well established, and the broad numbers involved widely recognized. The concept and its empirical implementation now are widely recognized as useful in policy analysis. The regional studies departments of the Bank should have the capabilities and budgets to do the particular studies that they think important for their own analyses. Similarly, while Bank research on patterns of growth based on regression and input-output analysis have 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.

A bit more hesitantly, we 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 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 judgement regarding bank research on process industry investment programming. Waht is needed now is for the operating departments to develop the capability to work with the models.

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 the economy-wide models based on a computable general equilibrium framework. The panel doubts that these bodies of research will contribute much directly to understanding relevant to policymaking.

But the work is methodologically exciting and on the frontiers, and enables the Bank to attract and hold several very well-tooled economists. The arguments for continuation of these projects it seems to us must rest on the importance to the Bank of having on its research staff several economists who are technically very skilled.

The panel thought that there were certain broad areas where the Bank ought to place more research resources. One of these was the broad arena of export promotion and market access. Included here would be more study of the evolving patterns of intra-LDC trade, and possibilities for enhancing this sort of trade flow and making it more productive. We think it also fruitful for the Bank to study issues relating to the ability of LDC's to significantly enhance their manufacturing exports to developed countries without invoking protection. While these studies might involve assessment of economic impacts of certain types of exports upon developed countries, we do not think the Bank ought to get into the business of financing or undertaking research on adjustment policies in the developed countries. We do think, however, that the Bank ought to be pushing the developed countries themselves to be doing more of such research. More generally, it would seem possible and desirable to increase the research done at the Bank on how trade patterns are likely to evolve, under various policy regimes, as a number of the present less developed countries develop relatively sophisticated manufacturing capabilities.

A second broad arena where we think the Bank should put more research resources involves comparative studies of government policies and institutions and their effect on development processes. As research on effective rates of protection is phased out, research could well be increased on the nature and effectiveness of various government policies, that have been employed in different countries, on export promotion. 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.

Among the important policy and institutional topics for study, examination of a set of issues relating to public enterprises strikes the panel as particularly important. In developed countries as well as less developed ones, public enterprises are common in the provision of transport, power, and a variety of public services. Many countries are also employing public enterprise for the production of manufactured goods, particularly when significant economies of scale are involved. The question of the relation of public enterprise to market and to higher political authority, and more general issues relating to the motivation systems influencing decisionmaking in public enterprises, strikes us as important to study, probably on a comparative basis. The World Bank has initiated some research in this field. We urge that the field be given quite high priority.

A third broad set of subjects to which we think priority should be given involves mechanisms of techanology 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 these 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 going on within a country's engineering schools.

It would be very interesting 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.

We think that the three broad areas described above -- LDC exports and export incentives, comparative study of policies, strategies and institutions, and study of processes of adaptation and innovation particulatly in industry -- delineate the broad areas to which the Bank should be allocating more of its research resources.