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INDUSTRIAL DEVELOPMENT & TRADE PANEL  
MEETING FEBRUARY 1979

①

The World Bank Group  
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Meeting February 1979, v.1 - Industrial Development and Trade Panel

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MEETING SCHEDULE FOR INDUSTRIAL DEVELOPMENT  
AND TRADE PANEL

Thursday, February 8

8:15 p.m.

Dinner with Messrs. Balassa, Westphal, Bery at Le Bagatelle, 2000 K St. N.W. (Meet at Restaurant)

Friday, February 9

9:00 - 10:00  
[K-3700]

Discussions with Messrs. Balassa, Westphal and Bery.

10:00 - 11:30  
[K Building]

Reading period to review documents produced by the Industrial Development and Trade Steering Group.

11:30 - 12:45  
[K-3700]

Discussions with Messrs. Balassa, Westphal and Bery.

1:00

Lunch with Messrs. Balassa, Waide, Wood, Westphal, King, Bery and Gelb. (Executive Dining Room D, Room No. E.)

2:30  
[A-520]

Meeting with Internal Steering Group on Industrial Development and Trade. Membership: Messrs. H. Fuchs, R. Gulhati, B. Waide, L. Westphal, R. Richardson, F. Moore, D. Weigel. (Mr. D.J. Wood may also attend.)

Panel  
Information

## OFFICE MEMORANDUM

TO: Messrs. Lindbeck, Donges, Bacha &amp; Nelson

DATE: February 8, 1979

FROM: Suman Bery *SkB*SUBJECT: Office Space

The following offices have been reserved for your use during your visit:

Mr. A. Lindbeck	- K-3702, Ext. 60029
Mr. J. Donges	- K-3703, Ext. 60028
Mr. R. Nelson	- K-3604, Ext. 60018
Mr. E. Bacha	- K-3502, Ext. 60011

Other offices and extensions are:

Mr. Bela Balassa	- K-3411, Ext. 61998
Mr. Suman Bery	- K-3606, Ext. 60016
	or
	- F-1233, Ext. 76003

The DRC Conference Room (K-3700) has been reserved for Friday morning and Room A-520 for Friday afternoon.

Draft Chs.

1

1979.02.01

4<sup>th</sup> Draft.

REPORT BY WORLD BANK PANEL ON INDUSTRIALIZATION AND TRADE

by

Edmar L. Bacha, Gerardo M. Bueno, Juergen B. Donges, Jae-Ik Kim,  
Assar Lindbeck (chairman), Richard R. Nelson and Kirit Parikh.

February 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 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 some 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 advisory 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 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:

Supply Based

- (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 <sup>provide</sup> ~~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 research 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.

*But it is  
likely that  
his interests  
have been  
misplaced  
in the Bank  
to that extent*

*acceptance and subsequent research interests have shifted.*

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 <sup>incentive systems for and institutional</sup> ~~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.

And the consequences  
of alternative institutional  
arrangements  
Structure D

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 <sup>market and institutional structures,</sup> ~~different institutional arrange-~~ment 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, ~~for instance within the Industrial and Finance Division~~ <sup>Department</sup> (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, <sup>might</sup> ~~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 <sup>of these</sup> types 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 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?

Arrange a  
meeting  
between business  
schools research

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 important 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 research 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 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 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 one of these principles only.

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

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

ly 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 entrepreneurship, 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.

*Draft by  
W. P. H. H.  
operational  
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requires.*

There is also a strong case for the establishment of some <sup>Small</sup> ~~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 <sup>considered</sup> ~~somewhat~~ of an "academic enclave" of that organization.

Research is <sup>a</sup> ~~a full-time~~ highly specialized job, <sup>and should</sup> ~~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. 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 thought, 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.<sup>1</sup>

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.

2

## 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 protectionism; 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 leading 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 com-

ponent, 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

import substitution policies for countries to consider. Both of these notions seemed to be mentally connected with the view 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, let alone the whole field of incentive regimes. 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 documentations 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 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 effect on dialogue between the Bank and government officials of developing countries. But we are 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 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 advising 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

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.

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 "uneasy" 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 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.

Needless to say, a basic requirement in all three cases is that the Bank has, or is able to hire, highly competent researchers.

Chapter III 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 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 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.

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 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); entrepreneurship, 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 domestically 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.

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Welson - Chap. II  
Recd. 2/15/79

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 computable 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 cost effort. But the panel is unanimous that the most important research topics lie elsewhere.

We think there are several broad fields of research to which the bank should allocate more resources. These include, first, a set of topics related

to exports of the 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 saving 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 interpretation 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 deterrence 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 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 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.

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.

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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 knowledgeable about and interested in the research. Some of the new departures we suggest represent natural evolution of the research and interests of researchers currently at the Bank. But we believe that to design and carry out the research well the Bank is

going to need some new research talent with skills 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 ~~pe~~<sup>TS</sup>jects, 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.

3.1

EVALUATION OF WORLD BANK RESEARCH ON INDUSTRY AND TRADE

Incentive Policies and Economic Integration (Ch. III-1)

The Subject  
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1. The following evaluation is based on a sample of five RPO projects: (1) RPO 670-01, Development Strategies in Semi-Industrial Countries (directed by Bela Balassa); (2) RPO 670-22, Economies of Scale and Tariff Levels (Gary Pursell); (3) RPO 670-87, Industrial Policies and Economic Integration in West Africa (Balassa); (4) RPO 671-10, Promotion of Nontraditional Exports (David Greene); (5) 671-75, International Trade Policy for the Development of Bangladesh (C. Jayarajah). Four additional non-RPO projects (three by Balassa, one by Stephen Guisinger) will be referred to where appropriate. Seven out of the nine projects under review have been completed - successfully as will be shown below. RPO 671-75 is to be completed by November 1979, whereas RPO 670-22 has never produced a final report.
  
2. Most projects deal with the role of incentive systems in economic development, from both a theoretical and an empirical standpoint. They examine primarily alternative forms of import protection, export promotion and economic integration, and they analyze the effects of these measures on the allocation of resources, the balance of payments, the generation of employment and the overall growth in developing countries. Moreover, there have been attempts to evaluate incentives on a firm-by-firm basis and to appraise the impact of protection at the level of individual investment projects.

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

#### Review of Projects

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4. From both an operational and a policy-making point of view, the main contribution in the field under review is the project on Development Strategies in Semi-Industrial Countries, (henceforth "Strategies"), by which the World Bank has continued and deepened its research on (trade) incentives; work in this area has been closely connected with the Bank's main policy interests since 1967. The "Strategies" project started in mid-1971 and was completed, with a formidable volume of over 1200 (double-spaced) pages and tables, in mid-1978. Its purpose was to quantify and analyze relative incentives provided to import substitution and export expansion in countries pursuing different development strategies, which have already established a relatively diversified manufacturing sector. For the sake of comparability, the project concentrated on six countries at a similar level of industrial development: Argentina, Colombia, Israel, Korea, Singapore, and Taiwan. Ar-

gentina and Singapore are relatively new in the group of countries studied in this field, which is an important aspect in itself, because they represent the two extremes of economic policy shaping: highly interventionistic in one case, and strongly market-oriented in the other. The six country studies could no doubt profit from prior methodological and empirical research done in and outside the Bank, and so did the comparative analysis of the experience of the Six. On the whole, the policy conclusions and recommendations rest upon firm theoretical foundations and a sound factual basis.

5. The methodological framework underlying this project is the effective protection concept. It has been extended to include export subsidies as well as credit and tax preferences, in addition to import tariffs and quotas. By doing this, fortunately at a reasonable level of sector disaggregation, the "Strategies" project has increased the knowledge inside and outside the Bank about the effects of incentive regimes. This study is a good example of how analytical tools, which have proven useful in earlier analyses, can be improved in theoretical and computational terms. It is particularly noteworthy that, unlike many other studies in this field, a great effort has been made to collect comparative price data. Only price comparisons between domestic production and import values make it possible to calculate overall effective protection (i.e. including non-tariff measures) and to identify the degree of tariff redundancy which often exists.
6. That the analysis in the "Strategies" project has been carried out in a partial equilibrium framework, does not reduce its practical relevance, provided the estimates are taken as rough orders of magnitude and their economic implications are inter-



preted in a comparative way rather than in isolation. General equilibrium models tend to have a rather limited explanatory power, let alone their high sensitivity to the parameter specifications. What could have been wished from the user's point of view, however, is either that the country studies had been completed earlier or that the quantitative estimates had been more up-dated. An earlier completion of the project - originally planned for the second half of FY 1973 - was outside the realm of possibility given that various researchers were unexpectedly absorbed again and again by other duties in their own countries. Thus, the delay has to be interpreted as the price to be paid for embarking on large cooperative ventures amongst Bank's researchers and external economists; the "price" has been kept as low as possible, however, by circulating draft versions of various chapters well ahead of the project's completion. With regard to an up-dating of the results, one should remember that the quantification of the incentive measures refer to the end-sixties. While this is fine to understand the past economic performance of the countries under study, most sample countries have changed these measures, especially those related to the foreign sector, significantly since then. Had the "Strategies" project included these changes in the analysis, its results would have become even more significant than they are in any case. Of course, we are aware of the practical difficulties in maintaining up-to-date a comprehensive analysis as this one; it probably would have required a full-scale re-estimation of the effective rates of incentives. We therefore think that there is a need for the Bank when applying this research to up-date the results. The Bank is, as compared to academic institutions, in a favourable position to make such an effort; it has the staff and it can try to collect at least part of the required information through the economic missions which regularly review the LDCs.

7. The message of the research on incentive schemes is that whenever LDCs want to industrialize their economies and think that this objective requires government assistance, they should promote rather than protect industries. This implies, for instance, that import substitution should and could be

achieved without discriminating against exports, particularly nontraditional manufactured exports, as it is so often the case in reality. Neutrality between production for domestic sales and for exports is in the interest of LDCs because as the "Strategies" research shows, it will result in faster growth, more employment creation and higher levels of efficiency than when industrialization is aided only in relation to the home market. These are conclusions which can be used immediately by the operational staff. In fact, we were told that this work has been found quite helpful, particularly so by the country economists in the Regional Departments and by the IDF and IFC staff; scepticism with regard to the usefulness of this type of research prevails within the IPD staff, which has little to do with incentive systems, however. We strongly feel that one of the objectives identified for the Bank's Research Program - "to improve the Bank's capacity to give policy advice to its members" - has been accomplished by this project.

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

tiveness. Furthermore, one of the authors of the chapter on Argentina of the "Strategies" project has recently followed up the earlier findings, taking 1977 as the year for calculating effective rates of protection. We have also found for Colombia and Israel a growing amount of research resources devoted to the empirical analysis of trade policy issues, as a following-on to "Strategies" project. In Korea, Singapore, and Taiwan, where the authors have moved into prominent governmental positions, economic policy-making takes into account the recommendations which were formulated in the "Strategies" study. Thus, this work has contributed, directly and indirectly, to promote applied research in Bank's member countries, which is also a central objective of the Research Program.

9. Studies of this type are not only worthwhile because they may assist in shaping more rational incentive policies in the countries concerned. They are also important for the lessons they can provide for many other LDCs at lower stages of economic development. As one never can take for granted, however, that these externalities will be automatically internalized by those other countries, it always is helpful to extent the regional coverage of the research on incentives. The project on Industrial Policies and Economic Integration in West Africa was a useful extension of Bank's research in this field. The project was initiated in mid-1973 and completed in end-1978 (as scheduled). Its purpose was to appraise the potential of, and obstacles to, industrial development and economic growth in small, primary producing countries under alternative policies, including expansion of intraregional trade. Four countries were chosen for analysis: Ghana, Ivory Coast, Mali, and Senegal. They differ from those studied in the "Strategies" project in that they are less developed and have not yet

established a diversified industrial base. The incentive systems of these countries have been analyzed with regard to their impact, within the industrial sector, on import substitution within each country, increased intraregional trade, and expansion of exports to third countries; furthermore, the structure of (long-term) comparative advantages of these countries, in both agricultural and industrial activities, has been evaluated.

10. In measuring the incentives, basically the same methodology was applied as in the "Strategies" project. However, the rates of effective protection and effective subsidy do not reflect accurately the comparative advantage or disadvantage of particular activities since they are calculated on the basis of market prices rather than shadow prices. For this reason, this project has rightly applied the domestic-resource-cost concept as well as estimates of the cost-benefit ratio for foreign capital in measuring comparative advantages - using, as in the calculation of incentives, firm level data. While the findings provide a clear indication as to substantial incentive-generated distortions and a great diversity in the domestic resource costs of foreign exchange, they should be taken as reflecting orders of magnitude rather than exact quantification. As the authors themselves admit, the data are not completely reliable in all cases and the estimation of shadow prices, particularly of the shadow exchange rate, always involves some uncertainty. The sensitivity of the ranking of activities in domestic resource costs to the shadow price assumptions has been tested in the studies on Ghana and the Ivory Coast; such alternative estimates are still to be done for Mali and Senegal.

11. Research undertaken in this project was, as the "Strategies" project, most useful for the Bank's immediate operational needs. Indicative of this is the fact that the project was carried out in close cooperation with the Western Africa Regional Office and with support from the Industrial Projects Department (of CPS). The operational staff found the methodology, the empirical results, and the policy recommendation useful in evaluating government policies and programs in that area. Recently, the regional office has even undertaken the initiative to follow-on the research of this project by studying the effects of trade and other incentive policies on resource allocation in Cameroon (which is the dominant member of the Union Douanière et Economique de l'Afrique Centrale). And the Agricultural and Rural Development Department (of CPS) is now sponsoring a comparative study on the economic effects of price interventions in agriculture. The West Africa research project has also influenced policy-making in the countries concerned. For instance, the analysis of Senegal has led to the preparation of a tariff and tax reform; and the Nigerian Government has invited and financed a special World Bank mission in order to obtain guidance on the costs and benefits of a revised system of incentives, using the same methodology as in the West Africa project.
12. Operational guidance with regard to an Asian nonindustrial country is expected from the ongoing project on International Trade Policy for the Development of Bangladesh, which has been initiated by the South Asia Regional Office. This project may be considered as a country application of the effective protection - domestic resource cost methodology. It aims at three objectives, according to the project proposal: (i) to assess the structure and functioning of factor and product markets with a view of identifying relative

resource scarcities at present and in five years from now; (ii) to determine the structure of comparative advantages for existing agricultural and industrial activities as well as for new industries which might be established in the future; (iii) to design a trade policy, properly synchronized with domestic incentive measures, conducive to a removal of the balance-of-payments constraint to Bangladesh's economic development by encouraging investment in activities in which the country has, or can develop, a comparative advantage. Doubtlessly, these are important subjects. Whether or not the analysis can be conducted at a high rigor will crucially depend on data availability. The project proposed displays much optimism in this regard, but there is a risk that difficulties in collecting the needed information become enormous. That the execution of the project involves researchers from Bangladeshi institutions might facilitate the access to existing though not published data. But the real problem is that the existing stock of statistical information in Bangladesh is far from being sufficient to measure shadow prices, quantify incentives, and calculate domestic resource costs. There will be an inexorable need for additional data collection by the researchers themselves. One should therefore not be surprised if the research leads to a narrower range of permissible conclusions than is expected, or if its completion will be delayed.

13. The internal needs of the Bank relating to incentive policies do not exhaust themselves with country reviews. As a lending institution, it has also a great interest in knowing whether the economic viability of individual investment projects financed by the Bank depends on protection. It also has to possess appropriate guidelines for appraising the significance of a protection element in such projects. In this respect, the non-RPO study on Industrial Protection in IFC Projects, assigned to a research consultant, could be useful - and has in fact considered helpful by an internal evaluation report.

14. Basically the same conceptual framework as in the "Strategies" project referred to above was used to examine empirically the relationship between levels of protection afforded to nine individual IFC projects and their economic efficiency. It is shown that conventional measures of effective rates of protection do not provide all information which is required for making decisions at the project level. For, the judgement about the economic viability of a new investment also depends on future changes of protection (and other incentives) which can be expected to take place over the life of the project. This is an important aspect to be taken into account by all Bank staff members involved in investment project appraisal. While this is recognized in the corresponding offices (including the IFC itself), we have also to emphasize that, from an operational point of view, both Bank's researchers and officers would face serious data problems. The "Strategies" project shows how difficult it is to obtain adequate data for making price comparisons for past periods. It might prove even more difficult to make such comparison for a period of years in the future, as required ideally in solid investment appraisal at the project level. In order to make things easier, one could have thought of applying the domestic-resource-costs methodology since then there is no need of information on future protection. The use of this method would have been desirable anyway, because the study analyzed incentives mainly with regard to product markets, while it should have been extended to include also factor markets where distortions have their repercussions on product markets. These objections notwithstanding, this project is a good example of research, including methodological research, which, if feasible at all, can probably be undertaken only in an institution like the Bank with a reasonable chance of success. Researchers

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

15. In estimating the effects of protection on efficiency, the projects reviewed hitherto were mainly concerned with the allocation of resources within the economy. But protection not only permits domestic production that would not be able to compete with imports in a free trade situation (allocative inefficiency), but also permits poor performance in the protected industries (X-inefficiency). Comparison of the types of protection-induced inefficiencies and an assessment of the relative importance of the two is proper indeed. The project on Economies of Scale and Tariff Levels was supposed to study this subject explicitly, with Colombia, Indonesia, and the Ivory Coast as case studies. The research has yielded a first conceptual draft containing a priori propositions about the X-efficiency consequences of import protection. There is also a first draft available for the case study on the Ivory Coast (including five industry analyses) and, as a by-product, a case study on Iran (with three industry analyses). The project was, however, never completed: The access to data proved to be much more difficult than anticipated, in the case of Indonesia there was no way at all; the Colombian consultants to whom the Colombian study was commissioned reportedly did not have the experience and capability for doing good work in this



field; and the Bank's responsible researcher was absorbed by other duties. In retrospect, the project, although reporting about some interesting empirical findings which are not elsewhere available, must be considered as a failure.

16. The project on Promotion of Nontraditional Exports is, by comparison, more descriptive. Carried out in cooperation with the Economic Commission for Latin America, the project aimed at evaluating export expansion policies, with particular reference to Argentina, Brazil, Colombia, and Mexico. For greater comparability, the project was extended to include also the experience of non-Latin American countries, namely India, Israel, Korea, and Yugoslavia. The initiative was taken by the Latin America and the Caribbean Regional Office. In general, the country studies have surveyed the existing stock of knowledge in this field, rather than increasing it through new empirical investigations. Their significance for policy-making is, however, great. It is shown that economic policies of developing countries have a substantial impact on the evolution of new manufactured exports, and in this regard this project has complemented the "Strategies" one. It is an example of how Bank's research findings relating to incentive policies can be diffused among both the Bank's operational staff and local researchers as well as government officials in developing countries.

#### General Evaluation

17. The studies on incentive policies and economic integration, which have been completed so far, show five distinct features: One is the high professional quality of the work. This has allowed various authors to already publish part of the

research results in highly reputable journals (see Annex, to be compiled by the Bank), and thereby to contribute to the effective dissemination of the findings. Second, in most of the cases the authors have made a substantial effort to improve the methodology for policy analysis and investment appraisal. The Bank's research has been insofar to a large extent creative rather than imitating. Third, the applied component of the research has been generally complementary to the research in the field undertaken elsewhere (UN organizations, OECD, academic research institutions). There has been, however, some overlapping with regard to the LDCs chosen for analysis, which is perhaps a reflection of the uneven distribution of useful statistical data among LDCs. And fourth, the studies have been designed with a view of responding to important needs of the Bank's operational departments. The research was empirically oriented, what is of paramount importance if soundly based policy prescriptions are to be achieved. In fact, the operational staff is to a large extent convinced of the relevance of this research, and has expressed this by applying the findings of the research in country economic reports and policy analyses, by using the methodology for in-house quantifications of trade incentives, and by initiating additional research in this area. And fifth, a great deal of the studies reviewed has involved participation from developing countries - in most cases relatively successfully. This is a significant achievement since it could help to strengthen the role of rational choices in the political decision-making process in the countries concerned - to the benefit of their economic and social progress.

18. As the research in this field places high demands on the data, the Bank has proven to be an appropriate place for under-

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

19. With regard to the relationship between research and operational activities, one important lesson can be drawn from the studies on incentive policies and economic integration: While it may not always be possible to provide the operational staff with ready-made solutions to pressing problems, the overall research work in this field can contribute, and in fact has contributed in the past, to sensitize the country economists in the regional offices and to influence lending operations. Particularly, this work helped them to fully understand the efficiency problem at both the macro- and micro-level and to explain its implications to governments in developing countries. It is noteworthy that, until the

early seventies, Bank's economic and sector missions to developing countries used to adopt benevolent attitudes as to (protectionist) import substitution policies, while they now advocate vigorously for fairly neutral incentive systems combined with reasonably liberalized trade regimes and realistic exchange rates. Moreover, specific investment recommendations are based now more often on comparative advantage criteria than it was the case until recently.

20. As the effective protection-domestic resource cost methodology can be considered as matured and understandable for the operational staff, the past research under review has established the conditions for entering the application phase according to concrete demands.

The preparation of other (comparative) studies on incentive schemes may involve, from a purely academic point of view, diminishing returns - a point emphasized by the DPS staff. But yet, many developing countries still favour policies of import substitution, maintain high levels of effective protection, and prefer quantitative interventions over pricing incentives. And the Bank will presumably continue lending to them. A regular analysis of the incentive policies pursued by member countries could increase the advisory capacity of the operational offices. Moreover, if incentive schemes are to be changed in order to make development policies in developing countries more effective in the medium and long run, the Bank has the authority to explain this forcefully to national governments, whereas individual researchers of academic institutions, including those of the countries concerned, frequently lack this influence. This is particularly important with regard to the question of how a successful transition to a more efficient incentive scheme could be made in practice. The answer to this question,

while touched upon in past work, still requires a good amount of research anyway.

Areas for Future Research  
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21. From the above it follows, that Bank's research for appraising incentive policies should not be totally discontinued, though shifted in emphasis. To begin with, it would be desirable to investigate in-depth the existing incentive regimes with regard to factor markets, the implications of these regimes for both the functioning of product markets and the changes in the structure of trade, and the avenues of eventual reforms. Second, analyses of the existing or required overall incentive systems in natural resource-rich developing countries, in countries which are just beginning their industrialization, and in very low-income countries are of interest for assessing the prospects of self-sustained economic growth and successful integration into the world economy or, by contrast, the (potential) advantages and disadvantages of pursuing the "basic needs" approach combined with a strategy of (total or selective) delinking from the international economic system. Third, the continuation of research on promotion and diversification of manufactured exports, with emphasis on marketing aspects and on the contributions which export processing zones can make, is of considerable importance to developing countries and to Bank's policy advisory work as well. In this connection, the relationship between the emergence of export industries in developing countries and the implementation of discriminatory protectionist measures by developed countries should further be analyzed and then kept under review. Furthermore, policy changes in developing countries with regard to export promotion and import substitution as reaction to external

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

22. As regards research on economic integration, there are at least three subjects where further research is of importance. One would be to follow-on the work on Western Africa's integration referred to above by investigating the effects of the Fonds Communautaire de Développement on investment patterns, the impact of the monetary union on the international competitiveness of industries in the countries concerned, and the consequences of the Taxe de Coopération Régionale for the structure and intensity of intra-regional trade. Second, the different avenues of economic integration among developing countries (i.e. regional versus inter-regional framework; mutual trade liberalization versus product specialization agreements; customs union versus free trade area) should systematically be analyzed, taking into account the factors which have contributed to the many failures of integration schemes in developing countries in the past. Third, more insights into the costs and benefits of economic integration among developing countries should be provided for operational purposes.
23. These are some illustrative issues which, in our view, have important applications in the Bank's advisory, financial and industrial sector work. Obviously, the various topics are linked together at many points. As staffing constraints might not allow to expand research into all areas outlined

above at the same time, priorities among them should be established by the Bank's Research Steering Group according to actual and potential policy and operational needs with which this Group is more familiar than we are. We strongly suggest, however, that once resources have been allocated to specific lines of investigation, the completion of the work should not be threatened by overloading the responsible researcher with too many duties (as it was frequently the case in the past). If the in-house demand for policy work increases, and if this increase is not only temporary, we recommend that the Bank enlarges its research staff accordingly.

3.2



COMPARATIVE ADVANTAGE; TRADE PATTERNS; ECONOMIC GROWTH

Edmar L. Bacha  
January 1979

Chapter III.2

1. INTRODUCTION

This chapter addresses a set of projects on general equilibrium trade models, comparative advantage structures, patterns of industrial development, and sources of industrial growth.

The next section contains a project by project review, which is followed by a general evaluation of the field. The chapter closes with a brief discussion of possible future extensions of the research under review.

2. PROJECT REVIEW

There are six RPO projects under this heading plus two non-RPO papers by Bela Balassa. There follows a brief description of title, content, organization and present status of the research projects.

670-07: International Model

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

The model was contracted out in September 1972, with the work being carried out by Victor Ginsburgh and Jean Waelbrock, both as outside consultants (Waelbrock worked for a while at the Bank). The project is supervised by Bela Balassa. The latest completion data is set for the Autumn of 1978.

The reviewer had access to two papers of a methodological nature, the first describing the computational algorithm and the second presenting the "General Equilibrium Model of World Trade". The papers appear to be very innovative. According to the authors, "this seems to be the first successful computation of a general equilibrium for a large realistic model". The use of a GE model also forces the model builder to incorporate only theoretically

significant constraints, avoiding the use of the crude heuristics which play such a large role in development planning models".

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

670-19: Expansion in manufacturing for exports in developing countries

This project proposes to analyze which industries are moving out of industrialized countries because of high wages and labor shortages into low-wage developing countries. It intends to survey export industries that have been transferred to Singarope, Taiwan, Korea (and possibly Mexico), as well as to sample international firms in Japan, the U.S., and Europe, on their outlook for further shifts of export manufacturing operations to LDCs.

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

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

An internal evaluation report is available on the two papers, concluding that while they produced some useful information, they are not of good quality. Both papers are in fact very descriptive. One at least delivers his message in a readable format, while the other paper is a poorly written dossier, with interest only to market research analysts in the electronics industry. The project was a failure from its inception. In view of its poor design, most probably

it would lead nowhere even if Jack Baranson had stayed in the Bank. This is a pity as the subject matter appears extremely important to inform the industrialization strategies of developing countries.

670-79: Economic Development of East and Southeast Asia

As the previous project, the objective of this one is to identify and analyze the industries most likely to find it advantageous to shift all or part of their processing activities from Japan to East and Southeast Asian countries.

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

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

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

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

671-05: Patterns of Industrial Development

The main purpose of this project seems to be the organization of macroeconomic industry and trade data banks at the World Bank. The economic analysis is limited to updating the Chenery-Taylor regressions on patterns of industrial

growth, with some marginal methodological improvements.

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

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

This project seems most useful

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

Users Guides to the data bank have been written and apparently the operations staff of the Bank are making use of these files in their country and sector analyses.

671-32 : A Comparative Study of the Sources of Industrial Growth and Structural Change

This project is designed to contribute to an empirically based theory of industrialization, that Hollis Chenery and associates have been building for the last 19 years.

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

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

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

Emphasis of the analysis is placed on exports and on import substitution. The

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

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

The project started in early 1976 and completion is expected for late 1978. People responsible for the project include Sherman Robinson, Yuji Kubo, Hollis Chenery, Larry Westphal and Moyses Syrquin.

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

This project may be considered as the end-product of a twenty-year quest to establish an empirically based paradigm for modern industrial growth. It demonstrates the importance that simple ideas may have for the development of economic knowledge. Chenery's

seminal 1960 paper consisted of little more than the numerical estimation of an accounting identity. However, as his papers for the Nobel Symposium and for the Tokyo IEA Congress (the later co-authored by Syrquin) demonstrate, within an 18-year period such simple exercises allowed the organization of a considerable body of empirical knowledge regarding the nature of modern industrial growth.

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

671-79 : Sources of Growth and Productivity Change

This project is a follow-on to the previous one (RPO 671-32) with specific focus on Korea, Turkey and Yugoslavia.

Three levels of study are contemplated: (1) linking the demand oriented analysis of the previous project to the study of sources of growth from the supply side; (2) constructing price-endogenous programming models for the three countries, aiming at a "consistent, comparable and rigorous" analysis of different policy packages, and (3) complementing the previous analysis with a micro-focussed study of two common industrial sectors in the three countries.

The project started last summer and completion is expected for late 1980. Sherman Robinson directs this research project, with Kemal Dervis, Larry Westphal and Yuji Kubo being involved in planning or carrying on the work.

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

The study of sources of growth from the supply side has a limited interest. It is a useful way of organizing relevant economic data, which the demand-oriented view of the previous project leaves relatively untouched. However, after the Cambridge controversy on

capital theory and the methodological and empirical criticisms of this type of work by Frank Fisher and Griliches and Jorgenson, one perhaps should approach the study of production function decomposition with more modesty than is apparent in Appendix A to the research proposal.

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

The price-endogenous multi-sector programming models are presented in the proposal as a superior way of looking at industrialization processes. They would overcome the partiality of the "sources of growth" studies, and provide an integrated approach to the problem of industrial expansion. Experience says differently. Accumulated knowledge on the development of such models indicate that their main merit is as organizing devices for the collection of relevant data for economic policy making. The empirical results that they produce, if they make sense (which is not often) can as well be obtained much more inexpensively from simple macroeconomic formulations.

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

NON-RPO : Two papers by Bela Balassa

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

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

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

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



### 3. GENERAL EVALUATION

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

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

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

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

### 4. FUTURE RESEARCH

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

For reasons that were already mentioned, we do not think that it would be advisable to expand the project on sources of growth from the supply side beyond its present size. From the perspective of industry and trade, we also consider questionable the allocation of large sums of money to further research on general equilibrium models. Some work of this type should always go on to keep the WB researchers in contact with the state of the art in academia. Also, perhaps general equilibrium models may be found more useful for general policy analysis outside the scope of the panel's review.

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

New research lines that the Panel considers advisable to explore are discussed in detail in Chapters 1 and 2 of the report.

3.3

Chapter III.3

Evaluation of World Bank Research on Trade and Industrial Development:  
- Export promotion policies in the LDCs and access to markets in the DCs -

The Subject-Overall Comments

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

The twelve studies recently commissioned by the Bank signify an important step towards a solution of this problem that could very well develop into a political issue gravely undermining the Bank's role as a development institution. What follows is a review of these projects. Most of these contains materials falling within the following three categories: 1) surveys of selected industries, data compilations, analyses of commodity markets or the overall environment of trade; 2) problems concerning import restrictions by the DCs on the LDCs' manufactured goods (the reverse problem, and that of obstacles to inter LDC trade are given little attention); and 3) national policies and the role of institutions in promoting trade in the LDCs.

2. If the remarks given below seem critical it is because of the very high standard of research one has come to expect from the Bank. All but four of the papers were ongoing projects and only drafts and proposals were available for review. The three finished works, by Balassa, Hughes and Sharpston were of notably higher quality. Many of the criticisms levelled at the unfinished projects are doubtless ones which the authors are already in the process of correcting.

#### Review of Projects

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

4. Keasing, Plesch and Triner's survey entitled "Developing Countries' Exports of Textiles and Clothing: Perspective and Policy Choices" has an excellent summary which also deals with the Multi Fiber Agreement

complexities in a lucid fashion, and advocates the desirability of a shift to LDCs of textile manufacture. In textile and clothing, as in electronics, the dominance of a few countries is apparent. The authors raise the interesting question of windfall gains resulting from quotas but unfortunately, offer only "speculative inferences from unsystematic observations" concerning this aspect. As far as the policy choices section is concerned the recommended policies could have been more clearly differentiated. Policies recommended to the firms in textile and clothing, which assume no changes in the policies of the importing countries regarding either quota or tariff, should be distinguished from policies which take into account the impact on the importing countries' policies of any expansion of exports. Also, policy choices for governments which are willing to go through industrial adjustment have to be comprehensive, and their successful implementation require close cooperation with the exporting countries.

5. Keesing and Plesch's work "Recent Trends in Manufactured and Total Exports from Developing Countries" is essentially a commentary to a set of tables. It is invaluable as a reference work, but has a rather limited lifetime unless updated every year. (The commentary could be improved as there are considerable discrepancies between the figures given for the share of the U.S.A. in LDC manufactured trade on p. 3, Table 13 and Annex A-3 -- at least one of these must be a misprint). Again what

emerges is that 3 NICs enjoy 76% of manufactured exports from LDCs. In all the above studies, could not more attention be given to less successful countries?

6. Morawetz's study (RPO 671-56) on "Marketing Manufactured Exports" could be valuable to countries which hope to expand their clothing exports to developed markets. An analysis of the institutional and non-price aspects of the phenomenal growth in that trade from Colombia should produce useful knowledge on marketing problems which are the key to success.

7. Baldwin's proposal "The Effects of Increased Imports of Manufactured Goods from Developing Countries in the United States" (RPO 671-67) and Waelbroeck and others' "The Effects of Increased Imports of Manufactured Goods from Developing Countries in Europe" (RPO 671-66), are investigating the market share of manufactured products from developing countries and the sources of protectionist pressures in industrialized countries. The European group also proposes to study the role of the EEC in trade restrictions in the future. These proposals hold good promise and are to be joined by a study on a similar basis of Japan and other Pacific countries which was not offered for review. Although much has been written about the economic impact of manufactured imports to developed countries, few studies so far have made any serious attempt to analyze the political impact of these imports. This sort of analysis should be very valuable, and should provide important insights into the political processes that

transform these impacts into protectionist pressures and eventually into protectionist policy actions. Both studies began with identical hypotheses on protectionism. Protectionism and its successful reflection in government policies is believed to be a function of a dozen or so broadly defined factors, some quantifiable and others not. It is not clear from the proposals how weights will be assigned to each of these multiple factors. In the case of the American study, Baldwin will measure the market penetration of some 400 items, classified on a 4-digit SITC basis for a time span of 8 to 10 years. The history of the protectionist trend and its politico-economic causes will be studied. Both projects will provide empirical analyses of the structure of protectionism.

8. Of these two proposals for separate projects on the same theme, Baldwin's original project proposal was very extensive and without a clear connection between the two parts. It remains a very ambitious project setting out to uncover the 30 years history of protectionism in the U.S.A., including political pressure on congresspersons, as well as market penetration. One assumes that this project will change fairly radically as it is pulled into line with the other project and possible lines of research and methodology are identified. Waelbroek's proposal was even more vague and at the same time there was a lack of comparability with Baldwin's project. The review of research in progress was very poor. Even when completed, proposed changes within the EEC to admit three low



income European countries could make the results of limited value. A recent paper by Balassa setting out standards for methodology and comparability overcomes a number of these objections.

9. "Export Incentives in Developing Countries" (RPO 671-35), under the supervision of Balassa, evaluates the export promotion efforts of four developing countries, with a comparative framework using a cross-section investigation of major export products, and a time-series analysis of the effects of export promotion measures. The project is a major effort in data collection and is designed to yield practical information for other countries that contemplate the introduction of a system of incentives for export promotion or reform of the incentive system. This project is of the greatest relevance to operational staff and to policy makers. It is precisely the sort of work the Bank should be doing. Its grasp of methodology at the initial stages is much clearer and appropriate than either Waelbroek or Baldwin, and its potential pay-off for developing countries is likely to be much higher. In view of the importance of this topic, might not more resources have been applied to more country research, including a country that has tried to apply all the right measures, but failed to achieve spectacular success?

10. Seen from the same perspective, Keesing and Wortzel's proposal "Key Institutions and the Expansion of Manufactured Exports" (RPO 671-68)

is expected to make an equally important contribution. Its purpose is clear, its frame of investigation concrete, and its theoretical and methodological foundations sound. As any export promoter knows, marketing has been the most serious bottleneck for export expansion. Particularly for the inexperienced exporter, who usually takes low prices for granted and bases his export projections on them, a marketing bottleneck is a frustrating, and frequently invisible, roadblock. Keesing's research will therefore fill a large gap in existing information and be a valuable educational tool for exporters of consumer goods to the U.S. market.

11. This project explains the relevance of a number of the surveys previously reviewed. It frankly admits many of the criticisms a reviewer might make of those other projects (p. 4) in that 'in predicting or projecting manufactured exports, areas of uncertainty (and plain ignorance) are also evident in World Bank operational work on manufactured exports, and in the work of the team proposing this research'. The study concentrates on key institutions in marketing, which is the serious bottleneck for most LDCs. At the proposal stage the project seems very oriented towards the U.S.A., and while there is a justification for this in view of the importance of the U.S. in LDC trade, it looks as though the research department lacked a European dimension. In addition the proposal admitted an overlap with other projects. It is not immediately clear why all these projects were not part of the same single coordinated project, unless the research department suffers from managerial problems.

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

13. On the other hand Wall's "Export Promotion and Preferences: A Case Study of India" (RPO 670-21) used an inadequate methodology to attain the purpose of the project. It failed to identify the system of policy measures that would be desirable and sufficient in India to enable entrepreneurs to respond to new opportunities in foreign markets.

14. The editor, Helen Hughes, and authors of contributions to "Industrialization and Trade Policies for the 1970's" (RPO 670-20) made an important contribution in documenting recent growth in manufactured exports from the LDCs and its impact on industrialized countries. Systematic and comprehensive studies of the effects of increased manufactured exports from low income countries on employment, social policies, and investment in the developed countries were long overdue. Without effective policy measures on adjustment assistance in developed countries,

economic growth in many low-income countries will have to slow down, causing a long chain reaction. This work, which contains important statistical information based on a thorough analysis of primary data, has also stimulated other studies.

15. Balassa's recent paper, "World Trade and the International Economy: Trends, Prospects and Policies" (May 1976), is a timely study valuable to the entire development economics community. It compares trade liberalization and economic growth prior to the oil crisis with trade barriers adopted during the post-crisis recession. Recent non-tariff restrictions, government aids to industry, international cartels, and market-sharing in major developed countries are documented and their effects evaluated. Concluding that the risks of new protectionism are high, Balassa proposes policies for long-term growth, structural adjustment assistance, and an international code of good conduct. Semi-industrial developing countries were also advised to reduce existing protection, to upgrade and diversify their exports, and to gradually abandon the export of simple, unskilled-labor intensive manufactures for the benefit of countries at lower levels of development, a course of action which some developing countries in Asia have already set out to follow.

#### Overall Evaluation

16. If we consider the three categories of surveys, data compilations and analyses; problems concerning import restrictions; and national incentive policies and the role of institutions: it is with regard to the research projects in the first category that the research department seems to have some problems. In the past, Bank-financed research has been well respected for the quality of information on specific problems or situations. The country reports and industry surveys have filled large gaps in existing knowledge which other research was not able to fill because of its distance from the scene. A continuation of this "division of labor", is still a good arrangement. In research, the Bank's comparative advantage lies in its proximity to on-going development problems. It has a unique global network of information, and Bank researchers, whether in-house or commissioned, are well received by most member governments. One would expect the Bank to have evolved a satisfactory system of rapid data collection and assimilation. This expectation is not met, for it would appear that some of the researchers are heavily reliant on the U.N. Statistical Office which does not supply the necessary information fast enough; in particular domestic production statistics seem to be five years behind events and appear in a form that does not match trade classifications. Although the Bank's main purpose is not to collect data, it would seem that there might be room for improvement particularly in information flow between the research staff and operational staff.

17. Turning to category two, the question of restrictions on trade by developed countries, it is not clear whether this lies within the Bank's 'comparative advantage'. However it has to be recognized that these restrictions could do more to upset the evolution of LDCs' economies than any other action. As Douglas North has recently pointed out, neo-classical market analysis 'can only survive under the highly restrictive assumption that the costs of using the political system to alter the market structure are prohibitive' (*Journal of Economic Literature*, XVI, (Sept 1978), p 971.) If this is the case, then both the brilliant general theory of comparative advantage stages of development and specific studies on the income elasticity of manufactured exports from developing countries will have limited practical relevance. The fundamental causes of protectionism need to be better understood and developing countries and various lending agencies that must predict the expected return on investment in manufacturing for exports need to tackle this problem, although it may be amorphous and intractable to traditional methodology.

18. Turning to category three, it is here that the Bank has a clear comparative advantage. The relevance and application of the results of such research to the Bank and policy makers are clear and there is little doubt that only the Bank could pursue this line. It is noticeable that the methodology and general approach is much clearer in the proposals of Balassa (RPO 671-35) and that by Keesing and Wortzel (RPO 671-68) than

in the others submitted for review. One hopes that the general surveys in category one will be kept updated in the interest of these general projects which they are clearly intended to service.

#### Possible Directions of Future Research

19. Judging by the project descriptions, more research can be commissioned on several new areas that so far seem to have escaped professional scrutiny. Restrictions on market access for manufactured exports from developing countries, which are expected to intensify, will increase the need for research on protectionism in industrialized countries and their adjustment policies. The same trend will encourage further research on manufactured trade between developing countries. In connection with this, the Bank might initiate a series of research projects on the feasibility of industrial cooperation between developed countries and NICs with an eye on positive adjustment through intra-industry specialization.

20. Research on trade between LDCs will have to be conducted with methodological schemes that are quite different from those commonly used. The historical backgrounds of institutional deficiencies, which restrict trade between the LDCs, for example, call for a broad social-science approach. In the future when such research is commissioned the following issues should be thoroughly investigated: (1) The transportation bottlenecks to trade between the LDCs, (2) the desirability of setting up an

LDC preferential system, (3) special problems in marketing LDC-produced capital goods in developing countries, and (4) payment problems that are unique to developing countries.

21. On a similar scale, the Bank might perform a valuable technical assistance to a number of countries in South Asia, Middle East, and Africa which are interested in adopting export promotion policies based on incentives that had been tried successfully by NICs. Considering the importance of socio-cultural factors which affect a country's entrepreneurship and which vary a great deal from one country to another, it is essential that each country identify major inhibiting factors in advance that might otherwise render the transplanting of promotion policies ineffective, and then devise methods which will circumvent them. Furthermore, LDCs, whether NICs or not, may have to adopt a new perspective on access to markets. Since the main cause of the new protectionism lies in the internal political arena of individual developed countries, which are by and large beyond the LDCs' ability to change, they must look toward themselves for trade expansion. The Bank can assist LDCs by launching projects on trade restrictions placed upon each other by NICs. Trade potentials of developing countries in general need to be assessed, not on the basis of past performance records, but for their future possibilities, before the trade barriers can be traded off between developing countries.



22. Another neglected area is the service sector. Telecommunications, shipping, banking, and insurance, for example, have been considered even by developing countries to be the exclusive zone for developed countries. Research on the trade expansion effect and balance of payments impact on developing countries are not carefully studied yet.

23. An overall criticism of the papers submitted for review was a certain lack of appreciation of living within a historic process and not in a timeless economic world, and also a lack of differentiation between a small group of successful NICs and the larger group of LDCs. All the authors show some awareness of these problems but few spell them out very plainly. A few instances can be given: current price data rather than constant price data is often used. Authors may protest that this adds a further complication, but it adds a very important dimension. Similarly in much of the work on manufactured imports and protection there seems an inadequate discussion of the degree to which all LDCs live in the shadow of Japan's success. Without analysing the ongoing trade barriers to Japanese manufactured exports, the present situation affecting LDCs can hardly be understood. It might be that that the Bank research staff could benefit from a full time historical advisor.

RESEARCH PROJECTS ON INDUSTRIAL DEVELOPMENT  
AND TRADE

Market Access; Export Promotion Measures

<u>Project No.</u>	<u>Project Title</u>	<u>Responsibility</u>	<u>Date of Approval</u>	<u>Date of Completion</u>
670-20	Industrialization and Trade Policies for the 1970s	H. Hughes	June 72	Oct. 72
670-21	Export Promotion & Preferences: India	D. Wall	Mar. 72	Mar. 73
671-35	Export Incentives in Developing Countries	B. Balassa	July, 75	Dec. 79
671-56	Marketing Manufactured Exports	D. Keesing	June, 77	June, 79
671-66	Effects of Increased Imports of Manufactured Goods from Developing Countries in Western Europe	J. Waelbroeck	79	Middle of 80
671-67	Effects of Increased Imports of Manufactured Goods from Developing Countries in the United States	R. Baldwin	July, 78	Dec. 79
671-68	Key Institutions and Expansion of Manufactured Exports	D. Keesing	Jan. 78	Dec. 79

RESEARCH PROJECTS ON INDUSTRIAL DEVELOPMENT  
AND TRADE:

Market Access; Export Promotion Measures

1. Keesing, D. and Plesch, P., "Recent Trends in Manufactured and Total Exports from Developing Countries", June 6, 1977.
2. Plesch, P., "Developing Countries' Exports of Electronics and Electrical Engineering Products", February 14, 1978
3. Keesing, D., Plesch, P., and Triner, G., "Developing Countries' Exports of Textiles and Clothing: Perspective and Policy Choices, "May 31, 1978.
4. Balassa, Bela, "World Trade and the International Economy: Trends, Prospects and Policy", World Bank Staff Working Paper No. 232.
5. Sharpton, N., "International Subcontracting", March 1975, World Bank

Appendix: Checklist of Questions

1. What has been learnt? In 5 cases the work merely summarized existing knowledge though in two of these cases so brilliantly as to advance theory and practice. One was a clear failure, the other six should greatly advance knowledge if completed satisfactorily.
2. In all cases knowledge was being transmitted, but in several cases it was not clear that the authors had read one another's papers.
3. a) i. The influence on lending policy is likely to be limited in all but one case.  
ii. In 6 cases policy advice could be improved, in two other cases the policy advice relates more closely to DCs.  
b) All studies will contribute to knowledge of the development process, but in several cases this could be of historic interest.  
c) With respect to the LDC's national policies 4 will directly assist, in several other cases these sections were the weakest.
4. Excluding surveys the Bank has engaged in basic research.
5. The effect on operating people is difficult to judge.
6. In all respects the involvement of LDCs was rather low.

Truthful answers would be

- a) Virtually nil;
- b) Surprisingly low except for the export incentive study;
- c) Negligible;

7. The choice of consultants was not clear. Both Waelbroek and Keesing's proposals showed a lack of European contact. In several other papers a bias towards using U.S. data was apparent. In most cases there might have been opportunities to use LDC scholars. One feels a great deal of sympathy for Professor Bacha's suggestion of having open competitions for researchers rather than consultancy by invitation.

8. a) In 3 cases the mix of theory and empirical research was excellent, in others it was much less clear.
- b) The degree of policy orientation of research really depends on the quality of the sections which make policy recommendations and the degree to which the research staff consult with operational staff and LDC policy makers in drawing up the framework for the project.
- c) The choice of problems was reasonable.
- d) The assessment sometimes lacked a sense of being part of a historical process. Information gathering was important, but the outsider might have an unjustifiable expectation of a higher achievement.

9. The criteria on which the Bank chose its projects were not always clear when outside consultants were the main formulators.
10. a) Size of research: given finite resources there would certainly seem to be other projects which might be more profitable, and some of the work, for instance, the access to markets in DCs might have been performed by outside institutions.
- b) The composition of the research on fields, problems and methods within the limits of the replies to other answers was good.
- c) This is very unclear, and possibly as suggested in 7 unsatisfactory. What is the Bank's experience of contracting with institutions like the Brookings or comparable national foundations rather than individuals?
- d) This again appears unsatisfactory. Drafts appear to lie around getting outdated. How thoroughly do researchers and operational staff interact?
- e) How to use 'outside' knowledge and resources better?  
It would appear that the 'in-service' infrastructure is not adequate to the Bank's needs and there is no evidence of other institutions collaborating to produce data rapidly (apart from the pre-release of UN tapes).
- f) i. Certainly the Bank should be a knowledge bank.  
ii. This is difficult to judge; perhaps the best answer is 'to the extent that the operational staff fulfill the needs of all LDCs'.

- g) There might be room for improvement here; while nationals may not be the best and most candid judges of their own nations there is a lot of scope for sending economists from LDCs to other LDCs.
- h) This depends on the resources, the loads on current staff, etc. One possibility might be a form of visiting fellowship in which academics from LDCs are employed for a year as research fellows combining outside and inside virtues?

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Introduction

1. The following discussion treats each of the various subjects in a separate manner, although it could be argued that they are, in some sense, interrelated. It rests on the analysis of five RPO projects and two non-RPO projects, which are mentioned in Annex I, as well as in other relevant documents.
2. The themes are important for the Bank from the operational angle. They appear in the fora of discussion and, in particular, within the LDC's, where it seems important to have a better understanding of the problems involved for policy formulation. Progress in research in these areas by the Bank will allow it to increase its capacity to give meaningful and relevant policy recommendations.
3. The quality of research done by the Bank in these areas is, in general, relatively high in comparison with research done by other bodies. Nevertheless it should also be mentioned that the level is below the standards attained by the Bank in other areas or by certain academic circles. One also notices that a good part of the research effort in some fields, notably credit imperfections and public enterprises, appears to put too much emphasis on the experiences gained by one country (India).
4. Several reasons might explain the situation described above. We should note in the first place that all of these areas of research are relatively "new ones for the Bank. Therefore, in this case the Bank as yet has neither the experience nor the influence in policy recommendations that it has gained in other research areas. (Although, according to the operations people, borrower countries frequently demand Bank advice for just this type of situation.) Secondly, it seems that those responsible for these projects do not have the same intellectual leadership that one finds in other areas of Bank research.
5. However, it should be stressed that in no way does it mean that the quality of the research is low. It should also be kept in mind that many of the projects have not been finished and, in some cases, they are still at the conceptual stage. In any case, it appears obvious that they are promising areas of research and that to have more solid bases for policy recommendations it will also be necessary for the Bank to consider an intensification of its research efforts in these fields.

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\*This section has been drafted by Gerardo M. Bueno.

Small enterprises

6. Research on small enterprises has been to a large extent the result of a growing interest in the Bank in these questions at the policy formulation and the operative levels. Not only does one find a sectorial policy paper (Employment and Development of Small Enterprises); the subject is also frequently referred to in the latest Bank's annual reports and in the President's addresses to the Board of Governors.\* Accordingly, and although the subject does not fall squarely within the Bank's traditional lines of research, the decision was taken to support it. Thus, this appears to be one instance where the need for operative and policy formulation determined to a large extent the opening of new lines of research.
7. In other places, too, the subject is very much in vogue on account of the relations that would appear to exist between small scale enterprises (SSE's) development and the growth of employment. SSE's have become fashionable not only in international and regional organizations but also on the national level in both developed and underdeveloped countries. However, it can validly be affirmed that up to now this interest has manifested itself mainly through the formulation of programs to support SSE's and in the adoption of several specific measures to that end; on the other hand, little has so far been done in terms of policy formulation in the context of a global strategy. In short, the emphasis has been placed more on action than on reflection. Research, on the other hand, has tended to be descriptive and of a general character as well as repetitive.
8. The approach taken by the Bank is more original and appears to be more promising. In fact, it is to a greater extent in accordance with a view that considers development very much as a dynamic process that involves innovation, imitation, learning and competition in a Schumpeterian sense rather than as a movement towards competitive equilibrium. The possibility of falling into some pitfalls in following this approach is by no means negligible; but nevertheless it is the right approach and what may be suggested here is that the Bank should be more explicit about it. (This would entail, for example, more research than has been done in the past on the dynamics of SSE's.)
9. Research by the Bank in this area started more intensively with the RPO 670-77 project (Financing of Small Scale Industries); in addition there were two other non-RPO projects on Brasil and one on Malaysia. The two non-RPO projects on Brasil are economic research of good quality the main conclusions of which were that the SSE's sector had lost in relative importance and that new SSE's had tended to concentrate on the growth poles of

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

South East Brasil. Therefore, recommended (something that the Braziliens were already doing) the formulation of support programs for SSE's giving special attention to geographical distribution. The conclusions, as can be seen, were not particularly different from those reached in similar studies.

10. RPO Project 670-77 was a low cost undertaking through which the Bank aimed, for the first time, at having an overall view of the importance of SSE's in different economies. The results were afterwards discussed within the Bank and, in the opinion of some, particularly in the research area, it was thought a waste of time and considered that "a 2-4 days meeting would have been preferable". Finally the decision was taken to support its main recommendation to continue doing research on these questions; but this highlights the risks that can be run when research competence does not come up to the standards that have been set elsewhere in the Bank.
11. The follow up was RPO project 671-59 (Small Scale Enterprise Development). The formulation of the project owes much to Ian Little, as well as to D. Anderson and L. Westphal, and to their dissatisfaction with the type of research that was being done, and in particular with the absence of ways and means that would allow the evaluation of different policy alternatives. The project has two main objectives. On the one hand, a review of the existing information and literature on small enterprises in developing countries and, through trial surveys, an attempt to define ways in which the information can be improved. The other, which should be the most interesting at medium term, is to develop a basis for assessing the impact on income and employment of various policy options and an attempt to estimate the costs and benefits of various policies to small enterprises.
12. The first phase of the project includes a review of the literature as well as case studies and enterprise surveys. The studies will cover India, Colombia, Philippines, Japan, Korea, Taiwan, Nigeria, and the enterprise survey India and Colombia. From this point of view, one could say that the investigation has a good approach and that it will contribute to the diffusion of knowledge through the participation of researchers from developing countries. However, it would have been desirable to include a larger number of countries, taking advantage of the increasing interest in the subject. It is true that later on difficulties would have arisen in regard to data handling and coordination of the project, but it also seems true that the advantages would have been superior.
13. RPO 671-59 was started in November 77 and the first phase is expected to be completed by September 79. Three papers have already been completed, two of which touch mainly upon methodological questions and the third one presents in draft from the preliminary results of the small and medium scale enterprises survey in Colombia.

14. On the methodological side the first two papers cover, respectively, the problems that SSE's face in the credit and financial markets and in regard to technological development. The first one has made use, though to a limited extent, of research carried out elsewhere in the Bank on credit market imperfections, and the second of some of the work done in the area of capital labor substitution. Both of them present suggestive approaches and, more particularly, in relation to the distortions in LDC's economies which SSE's must face.
15. A lot of work remains to be done and it is difficult at this stage to provide an evaluation of the whole project. Nonetheless, some insights into the possible outcome are provided by the draft paper presented on SSE's in Colombia for three sectors (producers of pumps and compressors, producers of agricultural implements and producers of cookers and ovens); although the authors emphasize again and again the partial and preliminary character of the results therein presented. But, however preliminary they might be, they show that it is not only a valid subject of research for the Bank, but also a worthwhile one in which to increase the efforts. The survey presents a good discussion as to the main characteristics of the SSE's, the origin of the firms and the entrepreneurs, their technological adaptations, their problems vis-a-vis the labor and financial markets, etc. But this said, it also shows very clearly that there are at least three areas where further thought is strongly required. The first one is factor productivity and especially in regard to the different combinations of capital and labor; the second is the overall question of technology selection, utilization and adaptation as well as the learning process; and finally, the overall effect of economic policies on the development of SSE's. The first two are frequently mentioned in the Colombian paper, while the latter is seldom referred to. But it is an important one and perhaps we could expect that at a later stage its significance will be more explicitly presented once research in this third area has progressed. It must also be taken into consideration, just as is mentioned in other Bank papers, that many of these distortions that affect SSE's have their origin in the economic policies followed by many of these countries. (And it is clear that there is a role for the Bank to play in ascribing the responsibility for many of these distortions to where it rightly belongs.)
16. Lastly, in this section we will mention some of the problems that have surfaced during the actual carrying out of the research. Here, however, only general impressions will be put forward since it is difficult to be more precise in the absence of more detailed information. Amongst them perhaps the most important is the absence of an overall encompassing methodology, such as was provided for example in the case of the incentive studies and others. Although it can be argued that the development of just such a methodology is the result of an iterative process, further work could be heavily affected by the lack of it. Secondly, and perhaps more important at this state, is that the number of participating researchers seems to be relatively small especially in comparison with the aims of the project.

## Credit markets

17. In this area we find two RPO's, one of them (671-65) on "Small Enterprise Financing", and the other (671-64) on "Market Imperfections and Economic Developments", both of them under the responsibility of the Public and Private Finance Division. The discussion that follows also took into consideration a paper prepared by this Division (Domestic Finance Studies number 43) presenting its research program. At the outset, however, it must be pointed out that in this case some of the papers provided do not attain the quality levels which prevail in other areas of Bank research.
18. RPO 671-66 has not, as yet, any relevant paper. We have, therefore, only a research proposal to study the role of informal credit markets. The project is justified on the basis of the importance of those informal credit mechanisms for financing small enterprises and trade in various Asian and African countries. It is suggested that the studies could serve to ascertain whether the real cost of lending in this market is significantly lower than in the formal financial market and if it is possible to establish some sort of link between the two.
19. A pilot study has been initiated which focuses on "a significant element of the informal credit markets in India, namely the Shroffis". (The Shroffis are indigenous style bankers who extend and create credit through the issue of hundis as well as other credit instruments). Be that as it may, there are nonetheless certain questions that arise in connection with this project and for which adequate answers are so far missing. Perhaps this may be due to lack of time to delve more thoroughly into these matters but nevertheless is it useful to state at least a few of the questions. One of them, of course, has to do with the fact that we are left in the dark as to the significance of these mechanisms from the point of view of financing and allocation of resources, and the other would concern the significance of these questions for the normative aspects of the operations of the Bank. Without aiming to cast doubts on the validity of this research proposal - which in fact did not receive overwhelming support in the Bank - the point that we would like to make here is that special care should be exercised in formulating research in these "new" fields of research in the Bank; least they may be rapidly abandoned in favor of more "proved" lines.
20. RPO 671-69 on capital market imperfections and economy developments owes much to the formulations of Dr. Sen and to previous work that was done in the Division itself. The project was initiated in January 78 and it is expected that the final report will be finished by March 79. Various papers have already been produced; one on "Interest Rate, Transactions Costs and Financial Innovations", another on "Some Theory of the Financial Intermediation in Developed Countries", and one on "Portfolio Determinant of Commercial Bank Earnings in Selected Asian Countries". These were followed by a paper on "Financial Intermediation and Economic Developments: An empirical investigation" and which should be considered as a sister paper to the earlier one on "Interest Rate, Transactions Costs and Financial Innovations". The output, thus, has not been negligible.

21. In the development of the project three types of interests are clearly discernible. The first one is the need that was felt to make an examination of the impact that transaction costs have on lending as well as on borrowing in different markets. This is a subject in which there are obvious gaps in the literature and particularly in relation to the financial systems of LDC's. The second relates to the interest in the possible effects that banking regulations in LDC's may have on the banking structure and in the long-run financial viability of the banks. The third one concerns the need to have a model that would express the possible interaction between the development of capital markets and general economic development. These will be briefly examined in reverse order.
22. The third subject of research is covered by two papers, one of which presents a general model and the other attempts an empirical verification of some of the issues raised in the first one. But the latter, in fact, is more than just an extension of the former. Their aim is to better understand the role that financial intermediation plays in the development process as well as to identify the factors which limit its effectiveness and the benefits which may accrue from improving it. This was justified, on the other hand, by the fact - which seems a valid one - that economic analysis in the past has generated "neither a body of reasonably rigorous theory nor a wealth of credible economic results".
23. The central argument of the model, which does not as yet seem adequately proven, is that for a given production function and state of non-financial technology there is a role to be played by financial intermediaries channeling resources from low to high return sectors, thereby raising the aggregate marginal productivity of capital. The limits on this role have been defined at one end by the overall rate of return which would prevail in the absence of financial institutions and, at the other, by the rate which would prevail when capital markets are perfect.
24. Together with the presentation of the model the papers include a relatively thorough review of the existing literature and, as mentioned above, an empirical verification of a limited version of the model. However, it is obvious that further work will have to be done. One obvious need concerns the nature of the data and also further examination of the results provided by the model. A possible approach could be to take countries not based solely on geographical considerations but taking also into account their level of development and the structure of their economies.
25. Another paper examines the relationship between earnings and portfolio of the commercial banks of three Asian countries: Philippines, Thailand and Singapore. The technique used was that of statistical cost accounting and followed, with some variations, the methodology previously used by Hester and Zoellner. As in other cases the research was also justified on the grounds that analyses of this type are scant in developing countries.

*Check that papers referred to here are also referred to in Billio.*

26. The investigation is well conducted and the conclusions are interesting in that both the rate of return on loans and administrative costs seem to be positively correlated with the degree of regulation in banking operations. The lowest figures relate to Singapore which of the countries examined is the one that presents the most competitive financial market and is the least regulated. This is an analysis that undoubtedly can be further refined and extended to a larger number of countries, although we should mention that data problems in several cases will prove to be quite significant.
27. The paper on the relations between interest rate and transaction costs is an interesting one and developed with technical competence. It raises a valid point in calling attention to the importance of transaction costs for the borrower and the lender and thus, in a certain sense, to the need to innovate in the process of financial intermediation. However, the significance of this factor alone is not clear yet. The study previously referred to on banking in the Philippines, Thailand and Singapore estimated the administrative costs of lending on the average at 0.1-0.5, 1.4-2.0 and 0.77 percent respectively. It seems, therefore, that there are other more important factors to take into consideration than only the size of the borrower.

#### Public Enterprises

28. RPO project 671-11 (Managerial Structures and Practices: Public Manufacturing Enterprises), is up to now the only research project by the Bank on this area. It was started in March 78 and it is expected that the final report will be submitted in January 1980. In addition it is also being considered in a certain sense as an extension of some previous work and in particular reconnaissance missions that were previously made to Egypt, India and Yugoslavia. As yet, the only report available is a case study of a public enterprise: India's Swaraj Tractor.
29. The research proposes to examine a set of hypotheses which it is said, "are best described by the general proposition that the performance of public manufacturing enterprises is dependent upon their organizational and managerial structures and practices as well as the policy environment within which they operate". It is justified on the basis of the fact that a large part of the lending operations of the Bank concerns public enterprises and that they are a growing sector in LDC's industries. And, in addition, they are a type of enterprise about which not much is known and where in general, little research has been done.
30. As mentioned before, the only report available up to now is on the Swaraj tractor plant in India. The report is interesting, particularly the point which shows the significance that indigenous technical developments and adaptations had in the shaping of and the improvements in the competitive position of this plant. But one could also say that it does not allow one either to evaluate the overall approach taken by the Bank on this question or to arrive at definite conclusions.

31. In view of this it might appear convenient for the management of the Bank to give more reflection to the research work done in this area. The area itself is of prime importance for the Bank from the points of view of (1) allocation of resources, since a large part of its loans goes to public enterprises and (2) its advisory role to LDC's. It appears obvious that in the absence of a coherent body of thought on these questions there is a great risk for dispersion of the research efforts. This would be unfortunate since it appears to us that on account of its experience and the insights that could be provided by research executed in other areas, it is difficult to think, worldwide, of any institution other than the Bank that could do it.

#### General Evaluation

32. It is quite clear that the subjects pertaining to small enterprises, credit markets and public enterprise, have great relevance for Bank operations and for LDC's. However, it is also clear that here we have a case where relevance is not matched by the research effort actually carried out.
33. There are several factors which could account for this situation. One of them, perhaps, is the fact that in the past the Bank thought that the best way to approach these problems was on an ad-hoc basis through the operational units and, thus, that there was no need to support their actions with a research basis. This is what would account for the additional fact that all these areas of research are relatively "new" within the Bank. Indeed, one could go even further and argue that there was some sort of resistance towards including these and other types of institutional research within the body of research carried out in the Bank.
34. Even now, one can discern the prevalence of two types of attitudes. On the one hand, there is the attitude of considering research on institutional questions as "interesting" but devoid of academic rigorousness and, on the other hand, the attitude towards institutional matters of some researchers who tend to consider other research by the Bank as frequently too academic and of little relevance either to the Bank or, worse, for the real world. This is by no means a unique situation in research institutions, but there is not doubt that, as has been proven by the Bank itself, there is a lot to be gained through a better coordination of efforts.
35. All of this, obviously, is not something that can be attained overnight. But it is probable that a large part of the problem could be solved if the Bank could attract high caliber researchers with solid reputations for this area, just as it did in others. It seems to us, in short, that as yet one does not have in these fields of Bank research the kind of intellectual leadership that prevails in the other more traditional areas of research. The presence of such a leadership would be very useful because it would allow the Bank, on the one hand, to have a better analytical framework to give greater coherence and sense of direction to the



research and, on the other hand, to establish a better working relationship with research in other parts of the Bank and elsewhere. In the absence of this we think that there may indeed be a great risk of either a dispersal of efforts or of conducting research which could be of small relevance for the Bank and for policy formulation. Eventually, this could lead to the gradual abandonment of this type of research. This would be a pity because, as has been emphasized, we think that research in these areas is of significance for Bank operations and for LDCs' policy formulation.

36. One other aspect that we would also like to touch upon is the question of how normative this type of research should be. In our opinion it ought to be high. Through its research the Bank should be able to discern which types of policies and programs for development in a Schumpeterian sense are successful and under what conditions, and likewise which are unsuccessful and why. This, it must be said, is one frequently mentioned objective in the research proposals but which doesn't come across quite clearly in many of the papers under revision. It may be due to the fact that many of the RPO-Projects are still at a preliminary stage and therefore it is not easy, at this moment, to extract this type of conclusion. But, be that as it may, it is nevertheless clear that this is an area where the Bank would be utilizing, to the fullest, one of its comparative advantages in research vis-à-vis other institutions.
37. To finalize this section, we will touch briefly upon some of the more relevant questions put to the members of the panel. Firstly, we would like to stress that many of the papers under revision show a high technical competence. If one does not find in this area the same number of publications in specialized journals that one finds in other areas of research in the Bank, it would be wrong to jump to the conclusion that the prevailing quality standards are much lower in one part than in the other. In fact, there might be a host of factors to explain this situation; one of them, of no small significance, being the short period in which research in this area has been done. It is clear that many of the papers, duly revised, could be amenable for publication in many of these journals. Secondly, as to the usefulness for the Bank of the results from research, it appears to us that, indeed, it should be high once the projects are more advanced. One could go even a step further and say that research of this type is woefully needed by the Bank if it wants to allocate its credit resources more in accordance with a "basic needs" philosophy of development. And this, as the experience of some countries is beginning to show, is by no means an easy task. It must be mentioned also, however, that contrary to what has happened with other research, this might not be - and perhaps should not be - politically innocuous. Thirdly, there are the questions on basic research and on the degree to which research by the Bank duplicated that of other institutions. With regard to the first part, the answer is that there has been some basic research, especially in the area of small enterprises and on the role of financial development in the development process. But our recommendation here would be in favor of more,

rather than less, basic research. There are a number of aspects about which the existing knowledge is imprecise and especially when dynamic factors are taken into consideration. As to the second part of the question it simply doesn't seem that the Bank has duplicated work by other institutions. In practically all RPO's a point has been made to undertake overall reviews of the literature on the different subjects. This has assured to a certain extent that research duplication was avoided. In fact, and to be more precise, far from duplicating, in many cases the research done by the Bank on these topics has been original and pathbreaking. This is the case, for example, in research on SSE's and there are obvious possibilities of doing likewise in connection with public enterprises.

38. As to the impact of research on LDC's, the questions are somewhat more difficult to answer. We would like to start by pointing out that in all RPO's under revision there is participation of researchers from LDC's, though in some more than in others. As to the influence that this may have upon LDCs' policy formulation and relations with the Bank, it is difficult at this time to define what their precise character will be in the future. But the Bank has already quite a substantial experience in this regard and, at least, we think that it will not be substantially different from other experiences; i.e., that if there is substantial interest in the problem under consideration, many of the recommendations filter through to the policy decision-making bodies. Here, our attention does not so much concern the latter question, but rather the already expressed fact that perhaps the Bank from the outset could have widened the scope of some of the studies in order to cover a larger number of countries. There is in many countries a real need to know more and to receive better advice in most of the subjects covered by this chapter. And this is valid even for the Bank itself. We were impressed in our conversations with Bank staff and in reading Bank memoranda of the difficulties that many Bank missions have in answering questions that are frequently posed to them by government officials, such as what should be a sensible policy vis-à-vis public enterprises or SSE's development programs and so forth, and for which up to now only very vague or general answers could be provided.
39. We come now to the question of allocation of resources. Here we can make only two considerations. First, within the area itself, our impression is that it would have been preferable to concentrate most of the resources on SSE's and Public Enterprise, rather than on SSE's and credit markets, as has been the case. To us the former two appear to be more significant from the point of view of the development process, policy formulation and Bank operations. But it could be argued that insufficient factors have been taken into consideration and that the choice reveals personal preferences. Indeed, this may be so. But secondly, and perhaps more important, there is the problem of the financial resources that have been allocated for research in these areas. There is a need to increase them and, in particular, to obtain the concourse and collaboration of highly qualified people.

Recommendations regarding further research

40. The work done by the Bank on capital utilization, capital/labor substitution, SSE's and on technical change itself has provided evidence of the need to undertake more thorough studies of the role that this latter factor, technical change, plays in economic development. This, by the way, coincides very much with a growing preoccupation in many LDC's with the relation between technological development and overall economic development. This preoccupation, in fact, has to do with a number of factors. In the past, for example, it had much to do with the question of negotiating international transfers of technology, but nowadays it is clear that this is only one of the aspects involved and that perhaps more important are the aspects concerning the selection, adaptation and absorption of technology. Likewise, the importance of developing a domestic scientific and technological capability in order to better cope with the problems posed by the development process, particularly for the utilization of renewable and non-renewable resources, is coming more and more onto the fora of discussions. Certainly it is not an easy task but it seems to us that questions of this sort should be covered by research in the Bank.
  
41. It appears that the area of public enterprises has not been sufficiently studied in the Bank or, for that matter, anywhere else. The recommendation was made above for the Bank to essay what would amount to a fresh approach for research in this area. There are many questions here for which answers are, at best, only fragmentary. Amongst them, we should like to point out the relations between public enterprises and the public sector, or with the private sector of the economy or their role in technological development, etc. It is, to a certain extent, a virgin area though its importance for economic policy formulation or for Bank's operations is well recognized.

EVALUATION OF BANK RESEARCH ON INDUSTRY AND TRADE

Small Enterprises; Credit Markets; Public Enterprises

Chapter III.4

Introduction

- 1.- The following discussion treats each one of these in a separate manner, although it could be argued that the different subjects are, in some sense, interrelated. It rests on the analysis of five RPO projects and two non-RPO projects, which are detailed in Annex I, as well as in other relevant documents.
- 2.- The themes are important for the Bank from the operational angle. These themes appear in the fora of discussions and, in particular, within the LDC's, where it seems important to have a better understanding of the problems involved for policy formulation. The progress in the research done in these areas by the Bank will allow it to increase its capacity to give meaningful and relevant recommendations for policy.
- 3.- The quality of the research done by the Bank in these areas is, in general, relatively high in comparison with the research done by other bodies. But, nevertheless it should also be mentioned that the level is below the standards attained by the Bank in other areas or in certain academic circles. Further, one also notices that a good part of the research effort in some fields, notably credit imperfections and public enterprises appears to put too much emphasis in the experiences gained by one country, namely India.
- 4.- There are several reasons that might explain the situation described above. We should note, in the first place that all of them are relatively "new" areas of research in the Bank. Therefore, in this case the Bank has not yet either the experience or the influence in policy recommendations that it has gained in other research areas. (Although, according to the operations people, borrower countries frequently demand the Bank stuff for just that type of advise). Secondly, for reasons which aren't yet very clear, except in some cases, it seems that the responsables of these projects do not have the same intellectual leadership that one finds in other areas of Bank research.
- 5.- Those appear to be the two main factors to take into consideration. However, it should be stressed that in no way it means that the quality level of the research is low. It should also be kept in mind that a large part of the projects have not been finished and, in some cases, they are still at the conceptual stage. In any case, it appears obvious that they are promising areas of research and that to have a more solid base for policy recommendations it will also be necessary for the Bank to consider an intensification of its research efforts in these fields.

Small Enterprises

- 6.- The research on the small enterprises area has been to a large extent the result of a growing interest in the Bank in these questions at the policy formulation and the operative levels. Not only one finds a sectorial policy paper (Employment and Development of Small Enterprises) but also the subject is frequently referred to in the latest Bank's annual reports and in the President's addresses to the Board of Governors.\* Accordingly, and although the subject does not fall squarely within the Bank's traditional lines of research the decision was taken to support it. This appears to be, thus, one instance in which the operative and policy formulation needs determined to a large extent the opening of new lines of research.
- 7.- In other parts also, the subject is very much in vogue on account of the relations that seem to exist between SSE's development and the growth of employment among other things. SSE's have become fashionable not only in international and regional organisations but also on the national level on both, developed and under-developed countries. However, it can be validly affirmed that up to now this interest has manifested itself mainly through the formulation of programmes to support SSE's and in the adoption of several specific measures to that effect; on the other hand, little has been done yet in terms of policy formulation in the context of a global strategy. In short, the emphasis has been placed more on action than on reflexion. Research on the other hand, has tended to be a descriptive and general character and, to a certain extent, repetitive.
- 8.- The approach taken by the Bank is more original and appears to be more promising. In fact, they are more in accordance with a view that considers development very much as a dynamic process that involves innovation, imitation, learning and competition in a Schumpeterian sense rather than as a movement towards competitive equilibrium. The possibility of incurring in some pitfalls in following this approach are, by no means, negligible; but never-

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

theless is the right approach and what may be suggested here is that the Bank should be more explicit about it. This would entail, for example, more research than has been done in the past on the dynamics of SSE's; but this belongs more to the recommendations section.

- 9.- Research by the bank in this area started more intensively with the RPO 670-77 project (Financing of Small Scale Industries); in addition there were two other non-RPO project on Brasil and one in Malaysia. The two non RPO project on Brasil are econometric research of a good quality whose main conclusions were that the SSE's sector had lost in relative importance and that new SSE's had tended to concentrate in the growth poles of the South East Brasil. Therefore, recommended (something that the Brazilians were already doing) the formulation of support programmes for SSE's giving special attention to its geographical distribution. The conclusions, as can be seen, were not particularly different from those reached in similar studies.
- 10.- RPO Project 670-77, was a low cost undertaking (37 500 dollars) through which the Bank aimed, for the first time, to have an overall view on the importance of SSE's in different economies. The results were afterwards discussed within the Bank and, in the opinion of some, particularly in the research area it was thought a waste of time and considered that "a 2-4 days meeting would have been preferable" to that research project. Finally the decision was taken to support its main recommendation to continue doing research on these questions; but this points well the risks that can be run when research competence does not fit the standards that have been set elsewhere in the Bank.
- 11.- The follow up was RPO project 671-59 (Small Scale Enterprise Development). The formulation of the project owes much to Prof. Ian Little, as well as to D. Anderson and L. Westphal, and to their insatisfaction with the type of research that was being done; and, in particular with the absence of ways and means that would allow one to evaluate different policy alternatives. The project has two main objectives. On the one hand, a review of the existing information and literature on small enterprises in developing countries, and through trial surveys, attempt to define ways in which the information can be improved. The other which should be the most interesting at medium term, is to develop a basis for assessing the impact on income and employment of various policy options and attempt to estimate the costs and benefits of various policies towards small enterprises.
- 12.- The first phase of the project includes a review of the literature as well as case studies and enterprise surveys. The case studies will cover India, Colombia, Philipines, Japan, Korea, Taiwan, Nigeria, and the enterprise survey India and Colombia. From this point of view, one could say that the investigation

has a good approach and that it will contribute to diffusion of the knowledge through the participation of researchers from the developing countries. However, in my opinion it would have been desirable to include a larger number of countries taking advantage of the increasing interest on the subject. It is true that later on some difficulties could have arisen in regards to data handling and coordination of the project, but it seems also true that the advantages would have been superior.

- 13.- RPO 671-59 was started in November 77 and the first phase it is expected that it will be completed by September 79. Three papers have already been completed, two of which touch mainly upon methodological questions and the third one presents in draft form the preliminary results of the small and medium scale enterprises survey in Colombia.
- 14.- On the methodological side the two papers cover, respectively, the problems that the SSE's face in the credit and financial markets and technological development. The first one has made use, though to a limited extent carried out elsewhere in the Bank on credit market imperfections and the second on some of the work done in the area of capital labor substitution. However, it must also be said that these links are not so explicit as it would have been desirable. Both of them present suggestive approaches and, more particularly, in relation to the distortions present in LDC's economics to which SSE's must face.
- 15.- A lot of work remains to be done and it is difficult at this stage to provide for an evaluation of the whole project. Nonetheless, some insights as to the possible outcome are provided by the draft paper presented on SSE's in Colombia for three sectors (producers of pumps and compressors, producers of agricultural implements and producers of cookers and ovens); although the authors emphasize again and again the partial and preliminary character of the results therein presented. And, however preliminary they might be, they show that it is not only a valid subject of research for the Bank, but a worthwhile one in which to increase the efforts. Also, that it is a difficult one. The survey presents a good discussion as to the main characteristics of the SSE's, the origin of the firms and the entrepreneurs, their technological adaptations, their problems vis a vis the labor and financial markets, etc. But this said it also shows very clearly that there are three areas where further thought is strongly required. The first one is factor productivity and especially in regard to the different combinations of capital and labor; the second is the overall question of technology selection, utilization and adaptation as well as the learning process; and finally, the overall effect of economic policies on the development of SSE's. The first two are frequently mentioned on the Colombian paper, while the latter is seldom referred to. But it is an important one and perhaps we could expect that at a latter stage its significance will be more explicitly presented once

that research in this latter area progressed. It must also be taken into consideration, just as is mentioned in other Bank's papers that many of these distortions that affect SSE's have their origin on the economic policies followed by many of these countries. (And it is clear that there is a role for the Bank to play in adscribing the responsibility for many of these distortions where it rightly belongs).

- 16.- Lastly, in this section we will mention some of the problems that have surged up carrying out the research itself. Here, however, only general impressions will be put forward since it is difficult to be more precise in the absence of more detailed information. Amongst them perhaps the most important is the absence of an overall uncompassing methodology, such as was provided for example in the case of the incentive studies or others. Although it can be argued that the development of just such a methodology is the result of an iterative process further work could be heavily affected by the lack of it. Secondly, and perhaps more important at this stage, that the number of participating researchers seems to be relatively small and especially in comparison with the aims of the project.

#### Credit Markets

- 17.- In this area we find two RPO's one of them (671-65) on "Small Enterprise Financing" and the other (671-64) on "Market Imperfections and Economic Developments" both of them under the responsibility of the Public and Private Finance Division. The discussion that follows also took in consideration a paper prepared by this Division (Domestic Finance Studies number 43) presenting its research program. At the outset, however, it must be pointed out that in this case some of the papers provided do not attain the quality levels which prevail in other areas of Bank research.
- 18.- RPO 671-65 does not count, as yet, with any paper. We have therefore, only a research proposal to study the role of informal credit markets. The project is justified on the basis of the importance of those informal credit mechanisms for financing small enterprises and trade in various Asian and African countries. It is suggested that the studies could serve to ascertain whether the real cost of lending in this markets is significantly lower than in the formal financial market and if it is possible to establish some sort of a link between the two.
- 19.- A pilot study has been initiated which focuses on "a significant element of the informal credit markets in India, namely the Shroffis". (The Shroffis are indigenous style bankers that extend and create credit through the issue of hundis as well as other credit instrument). Be that as it may, there are non the



less certain questions that arise in relation with this project and for which there don't seem to be sufficient answer as yet on the research proposal. Perhaps this may be due to the lack of time to dwell more thoroughly on this matters but nevertheless, it is convenient to state at least a few of these questions. One of them, of course has to be with the fact that we are left in the dark about the significance of these mechanisms from the point of view of financing and allocation of resources. The second would concern the significance of these questions for the normative aspects of the operations of the Bank, and so forth. Without aiming to cast doubts on the validity of this research proposal - which in fact did not received an overwhelming support in the bank - the point that we would like to make here is that special care should be exercised in formulating research in these "new" fields of research in the bank; least they may be rapidly abandoned in favor of more "proved" lines.

- 20.- RPO 671-69 on capital markets imperfections and economy developments owes much to the formulations of Dr. Sen and to previous work that was done in the Division itself. The project was initiated in January 78 and it is expected that the final report will be finished by March 79. Various papers have already been produced. One on "Interest Rate, Transactions Costs and Financial Innovations". Other on "Some Theory of the Financial Intermediation in Developed Countries" and one on "Portfolio Determinant of Commercial Bank Earnings in Selected Asian Countries". These were followed by another on "Financial Intermediation and Economic Developments: An empirical investigation" and which should be considered as a sister paper to the early one on "Interest Rate, Transactions Costs and Financial Innovations". The output, thus, has not been a negligible one.
- 21.- In the development of the project three types of preoccupations are clearly discernible. The first one is with the need that was felt to make an examination of the impact that transaction costs have in lending as well as in borrowing in different markets. This is a subject in which there are obvious gaps in the literature and in particular in relation with the financial systems of LDC's. The second reponds to the preoccupation of the possible effects that banking regulations in LDC's may have on the banking structure and in the long run financial viability of the banks. The third one concerns the need to have a model that would express the possible interaction between the development of capital markets and general economic development. These will be briefly examined in reverse order.
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Their aim is to better understand the role that financial intermediation plays in the development process as well as an identification of the factors which limit its effectiveness and the benefits which may accrue from improving it. This was justified on the other hand by the fact - which seems a valid one - that economic analysis in the past has generated "neither a body of reasonably rigorous theory nor a wealth of credible economic results".

- 23.- The central argument of the model, which does not as yet seem adequately proven, is that for a given production function and state of non financial technology there is a role to be played by financial intermediaries channeling resources from low to high return sectors thereby raising the aggregate marginal productivity of capital. The limits on this role have been defined at one end by the overall rate of return which would prevail in the absence of financial institutions and, at the other, by rate which would prevail when capital markets are perfect.
- 24.- Together with the presentation of the model the papers include a relatively thorough review of the existing literature and, as mentioned above, an empirical verification of a limited version of the model. However, it is obvious that further work will have to be done in the future. One obvious need concerns the nature of the data and also further examination of the results provided by the model. One possible approach could be to take countries not based solely on geographical considerations but taking also into account their level of development and the structure of their economies.
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- 26.- The investigation is well conducted and the conclusions are interesting in that both, the rate of return on loans and administrative costs seem to be positively correlated with the degree of regulation in banking operations. The lowest figures correspond to Singapore which of the countries examined is the one that presents the more competitive financial market and is the less regulated. This is an analysis that undoubtedly, can be further refined and extended to a larger number of countries although we should mention data problems in several cases will prove to be quite significant.

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

#### Public Enterprises

- 28.- RPO project 671-11 (Managerial Structures and Practices: Public Manufacturing Enterprises), is up to now the only research project by the Bank on this area. It started on March 78 and it is expected that the final report will be submitted in January 1980. In addition it is also being considered in a certain sense as an extension of some previous work and in particular reconnaissance missions that were previously made to Egypt, India and Yugoslavia. As yet, the only report available is a case study of a public enterprise: India's Swaraj Tractor.
- 29.- The research proposes to examine a set of hypothesis which it is said, "are best described by the general proposition that the performance of public manufacturing enterprises is dependent upon their organizational and managerial structures and practices as well as the policy environment within which they operate". It is justified on the basis of the fact that a large part of the lending operations of the Bank concern public enterprises and that they are a growing sector in LDC's industries. And, in addition, a type of enterprises about which not much is known and where in general, little research has been done.
- 30.- As mentioned before the only report available up to now is on the Swaraj tractor plant in India. The report is interesting and particularly the point which shows the significance that indigenous technical developments and adaptations had in the shaping and the improvements in the competitive position of this plant. But one could also say that it does not allow one either to evaluate the overall approach taken by the Bank on this question or to arrive to more definite conclusions.

- 31.- In view of this it might appear convenient for the management of the Bank to give more reflexion to the research work done in this area. The area itself is of prime importance for the Bank from both points of view; allocation of resources, since a large part of its loans go to public enterprises and its advisory role to LDC's. It appears obvious that in the absence of a coherent body of thought on these questions there is a great risk for dispersion of the research efforts. This would be unfortunate since it appears to us that the Bank, on account of its experience and the insights that could be provided by research executes in other areas, it is difficult to think, worldwide, of any other institution that could do it.

#### General Evaluation

- 32.- It is quite clear that the subjects pertaining to small enterprises, credit markets and public enterprise, have a large relevance for Banks operations, and for LDC's. However, it is also clear that here we have a case where relevance is not matched by the research effort actually carried out.
- 33.- There are several factors which could account for this situation. One of them, perhaps, is the fact that in the past the Bank thought that the best way to approach these problems was in an ad-hoc basis through the operational units and, thus, that there was no need to support their actions with a research basis. This is what would account for the additional fact that all these areas of research are relatively "new" within the Bank. Indeed, one could go further and even argue that there was some sort of resistance to include these and other types of institutional research within the body of research carried out in the Bank.
- 34.- Even now, one can discern the prevalence of two types of attitudes. On the one hand, there are those that consider research in institutional questions as "interesting" but devoid of academic rigorousness and on the other, those of some researchers on institutional matters that tend to consider other research by the Bank as frequently too academic and of little relevance either to the Bank or, worst, to the real world. By no means this is an unique situation in research institutions, but there is no doubt that, as has been proven by the Bank itself, there is a lot to be gained through a better coordination of efforts.
- 35.- All of this, obviously, is not something that can be attained overnight. But it is probable that a large part of the problem could be solved if the Bank could attract high caliber researchers for this area and with a solid reputation, just as it did in other areas. It seems to us, in short, that as yet, one does not have

in these fields of Bank research the kind of intellectual leadership that prevails in the other more traditional areas of research. Their presence, aside of prestige reasons would be very useful because it would allow the Bank, on the one hand, to have a better analytical frame work to give a greater coherence and sense of direction to the research and, on the other, to establish a better working relationship with research in other parts of the Bank and elsewhere. In the absence of this we think that there may indeed be a great risk of either a dispersal of efforts or of conducting research which could be of small relevance for the Bank and for policy formulation. Eventually, this could lead to the gradual abandonment of this type of research. This would be a pity because, as has been emphasized we think that research in these areas is of significance for Bank operations and for LDC's policy formulation.

- 36.- One other aspect that we would also like to touch upon is the question of how normative should be this type of research. In our opinion it should be high. Through its research the Bank should be able to discern which types of policies and programmes for development in a Schumpeterian sense are successful and under what conditions, and likewise which are unsuccessful and why. This, it must be said, is one frequently mentioned objective in the research proposals but which doesn't come across quite clearly in many of the papers under revision. It may be due to the fact that many of the RPO-Projects are still on a preliminary stage and therefore it is not easy, at this moment to extract this type of conclusions. But, be that as it may, it is nevertheless clear that this is an area where the Bank would be utilising, at its fullest, one of its comparative advantages in research vis a vis other institutions.
- 37.- To finalize this section, we will touch briefly upon some of the more relevant questions put forward to the members of the panel. Firstly, we would like to stress that a large part of the papers under revision show high technical competence. If one does not find in this area the same number of publications in specialized journals that one finds in other areas of research in the Bank, it would be wrong to jump to the conclusion that the prevailing quality standards are much lower in one part than in the other. In fact, there might be a host of factors to explain this situation; one of them, of no small significance being the short period in which research in this area has been done. It is clear that many of the papers, duly revised, could be amenable for publication in many of these journals. Secondly, as to the usefulness for the Bank of the results from research it appears to us that, indeed, it should be high once the projects are more advanced. One could go even a step further

to say that research of this type is sorrowfully needed by the Bank if it wants to allocate its credit resources more in accordance with a "basic needs" philosophy of development. And this is, as the experience of some countries starts to show, by no means an easy task. It must be mentioned also, however, that contrary to what has happened with other research, this might not be, and perhaps should not be, politically innocuous. Thirdly there are the questions on basic research and on the degree to which research by the Bank duplicated those of other institutions. With regard to the first part, the answer is that there has been some basic research and especially in the areas of small enterprises and in the role of financial development on the development process. But our recommendation here would be in favor of more, rather than less, basic research. There are a number of aspects about which the existing knowledge is imprecise and especially when dynamic factors are taken into consideration. As to the second part of the question it simply doesn't seem that the Bank has duplicated work by other institutions. In practically all RPO's a point has been made to undertake overall reviews of the literature on the different subjects. This has assured to a certain extent that research duplication was avoided. In fact, and to be more precise, far from duplicating, in many cases the research done by the Bank on these topics has been original and pathbreaking. This is the case, for example, in research about SSE's and there are obvious possibilities of doing likewise in connection with public enterprises.

- 38.- As to the impact of research on LDC's the questions are somewhat more difficult to answer. We would like to start by pointing out that in all RPO's under revision there is participation of researchers from LDC's, though in some is greater than in others. As to the influence that this may have upon LDC's policy formulation and relations with the Bank it is difficult at this time to define their precise character in the future. But the Bank has already quite a substantial experience in this regard and, at least, we think that it will not be substantially different from other experiences; i.e., that if there is substantial interest in the problem under consideration many of the recommendations filter themselves to the policy decision making bodies. Here, our preoccupation doesn't concern so much the latter question but rather the fact, already expressed, that perhaps the Bank could have widened, from the beginning, the scope of some of the studies in order to cover a larger number of countries. This would certainly have created some problems particularly for coordination and data handling but it appears, at least superficially, that the advantages would have been greater. There is in many countries a real need to know more and to receive better advice in most of the subjects covered by this chapter. And this

is valid even for the Bank itself. We were impressed in our conversations with Bank staff and in reading Bank memoranda of the difficulties that have many Bank's missions in answering questions that are frequently posed to them by government's officials such as what should be a sensible policy vis a vis public enterprises or SSE's development programmes and so forth and for which up to now only very vague or general answers could be provided.

- 39.- We come now to the question of allocation of resources. Here we can make only two considerations. First, within the area itself, our impression is that it would have been preferable to concentrate most of the resources on SSE's and Public Enterprise, rather than on SSE's and credit markets as has been the use. To us the former two appear to be more significant from the point of view of the development process, policy formulation and Bank's operations. But it could be argued that not sufficient factors have been taken into consideration and that the choice reveals personal preferences. Indeed, this may be so. But, secondly, and perhaps more important, there is the problem of the financial resources that have been allocated for research in these areas. There is a need to increase them and, in particular, to obtain the concours and collaboration of highly qualified people.

#### Recommendations regarding further research

- 40.- The work done by the Bank on Capital Utilization, capital labor substitution, SSE's and in technical change itself have put in evidence the need to undertake more thorough studies of the role that this latter factor, technical change, plays in economic development. This, by the way, coincides very much with a growing preoccupation in many LDC's about the relation between technological development and overall economic development. This preoccupations, in fact, have to do with a number of factors. In the past, for example, it had much to do with the question of negotiating international transfers of technology, but nowadays it is clear that this is only one of the aspects involved and that perhaps more important are the aspects concerning the selection, adaptation and absorption of technology. Likewise, the importance of developing a domestic scientific and technological capability in order to better cope with the problems posed by the development process and in particular for the utilization of renewable and non-renewable resources is coming more and more in the forum of discussions. Certainly it is not an easy task but it seems to us, that questions of this sort should be covered by research in the Bank.

- 41.- It appears that the area of public enterprises has not been sufficiently studied in the Bank or, for that matter, anywhere else. The recommendation was made above for the Bank to essay what would amount to a fresh approach for research in this area. There are many questions here for which answers are, at the most, only fragmentary. Amongst them, we should like to point out the relations between public enterprises and the public sector, or with the private sector of the economy or their role in technological developments, etc. It is, to a certain extent, a virgin area though its importance for economic policy formulation or for Bank's operations is well recognized.



3.5

Evaluation of Bank Research on  
Programming in the Manufacturing Sector

Chapter III.5

1. The Nature 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. 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 manufacture of which may exhibit economies of scale. It 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.

These issues are explored 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 in these 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. A number of procedures have been developed 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.

(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 and or benefits of particular policies may be significant.

(c) Programming models provide a tool to estimate the benefits of regional co-operation to individual countries and help in designing schemes for sharing of benefits.

(d) The cost of complete neglect of interdependence in choosing between production and imports is 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 sector-wise total supply cost of no more than 3 percent of the total value added for the products involved in the study.

(e) 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.

(f) The absolute cost of neglect of interdependence is "by no means trivial" and is "far more" than 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 variables 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 co-operation in fertilizer sector development for the three countries of the region (Uganda, Kenya and Tanzania).

The formulation of the models for the fertilizer sector are conventional and straightforward. However, because of the shortcuts developed to reduce the size of the problem through pre-analysis, the models have been considerably more detailed, and consequently, operationally more meaningful.

An obvious limitation of the models as developed is the neglect of uncertainties. Explicit accounting for it might make the computation problem even more formidable. Yet it would be useful to explore to what extent 'pre-analysis' and sensitivity analysis can 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, Petrochemicals for Portugal and Mexico, Energy for Nigeria, and Chemicals for Turkey.

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 it has been found useful too. The project has reached a stage where research ends and applications begin. But the technology developed has to be successfully disseminated and transferred. 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. Thus the software development project (GAMS, RPO 671-58) has to be welcome.

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 endogeneous. 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 model for the mechanical engineering sector is not easy to formulate 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 <sup>a</sup> mixed integer programming one and break-even analysis is developed and used to reduce computational difficulties.

In addition, the allocative consequences of the results are explored in depth. An evaluation of alternative investment criteria is also made in the context of the results obtained from the model. The conclusion is reached that simple benefit cost criteria or the measures of comparative advantage, though they do not discriminate unambiguously between make-buy choices, are adequate and entail a small loss compare to analysis which account for interdependence.

It is also claimed that the loss entailed in using the best of the benefit-cost criteria is still large enough to pay back for the cost of more comprehensive analysis within one to two years. The validity of the claim, however, depends upon the assumption that the indicated solutions from these two methods can be implemented with equal efficiency.

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 "Approach to 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 in the quantity.

3.2 Usefulness for LDCs: Large programming models and particularly economy wide 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. Usefulness for Bank

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 activities.

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 LDCs: Though country specific sector studies should involve participation of local persons, the research in this area does not seem to have involved adequate number of persons from the LDC's and it seems doubtful if even in the countries in which case studies have been carried out, there would be any capability to either update and/or improve the particular sector study or to carry out a similar study for another sector.<sup>1/</sup>

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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 seems to be inadequate. The compulsions of time bound research programmes, the inconvenience of communication across large distances and the convenience of access to computers and xerox machines are understandable. And yet the outcome is regrettable. An effective programme has to be designed to facilitate participation of researchers from the LDCs.

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<sup>1/</sup> Even Korea where substantial local involvement in Bank research has taken place over a number of years, has recently requested assistance in the formulation of investment programmes in fertilizer, pulp and paper and basic metals. To what extent this is just a way of obtaining help of competent researchers from the Bank at lowest cost and to what extent this reflects on the difficulties of skill transfers is anybody's guess.

4. Suggestions for Research in Future

(a) Extention: In order to bring the research results and the methodology developed to practioners, planners and policy makers in the LDCs considerable amount of "extension" work would be required.

Writing manuals accessible to technical persons, even when the manuals are written to be accessible to non-specialists, is only a beginning. Short training courses would also be inadequate. 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 co-ordinated 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 reasonable time.

"Only a first step" needs

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.

(b) Programing models to Evaluate Appropriate Technology for Rural Industrial Development: The studies in industrial programming under RPO 670-24 have viewed 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 have been beyond the scope of these studies.

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.

(d) Consequences of Interdependence for Planning and Promoting Industrial Development: If economies of scale and interdependence are important then the development of that industry would benefit from some form of central planning or coordination. On the other hand, effective implementation and central planning of a sector such as the mechanical engineering industry 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 is not of much quantitative consequence is very comforting as one can rely on implementable, decentralized procedures for developing the industry. Yet the finding needs to be confirmed with further research. 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

3.6

Chapter III.6

WORLD BANK PROJECTS ON CAPITAL UTILIZATION, CAPITAL LABOR SUBSTITUTION,  
AND TECHNOLOGICAL CHANGE

I. DESCRIPTION OF THE TOPIC

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

The policy thrust of the research has been provided by the belief that 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 that while this was happening to some degree it still was possible and desirable that the techniques employed be more frugal on use of capital. Throughout, the research has been concerned with market and other forces that explain the prevailing situation, and with policies that could improve the environment so that choice of technique would be made more appropriately, techniques used more efficiently, and adaptation and learning proceed more effectively. The analysis has been concerned both with contexts in which decision-making with respect to technique is made largely by private profit-oriented entrepreneurs, and with situations in which the government is the decision-maker or has a great deal of direct control over private decisions. The specific topics studied have evolved over the years as work done under one project generated insights and puzzles which influenced work under a subsequent project. The overall impression is that of a cumulative research program that is contributing significantly not only to thinking in the Bank about industrialization problems, but which is influencing thinking within the development economics community more broadly.

## II. PROJECT REVIEW

The specific projects reviewed here are 670-23, 670-25, 670-54, 670-95, and 671-51. Three of these projects now are closed out, and have received earlier reviews. The Bank research on industrial capacity utilization (670-25 and 670-95), now completed, was concerned with assessing carefully and empirically casual observations that despite the scarcity of capital in less developed countries, capital was not being used very intensively. If the proposition held up under such scrutiny, the project aimed to contribute to understanding of the phenomena and of ways to improve the situation. The basic ideas originated outside of the Bank, and scholars had been working on the problem before the Bank got seriously into the study of capacity utilization. But the Bank work was of considerable scope, and contributed significantly to the growing appreciation that indeed in many cases capital was not being worked very intensively in LDC industry. Bank research identified a number of factors, associated with imperfect input markets and various legal and institutional restrictions, that seemed to lie behind the non-economical use of plant and equipment. Project 670-95, which was completed largely by or under the close supervision of bank employees, was more successful than 670-25, which was farmed out to a consultant.

The project on employment and capital labor substitution (670-54), now also completed, was more of a mixed bag. It was related to the work on capacity utilization in that this project also was concerned with examining ways in which capital could be used more frugally. The research under the project did not have the coherence of work under the capital utilization project, and by and large the output of the project has had nowhere near the impact. However, the questions considered under the project still are of prime importance to the Bank, and their exploration continues under another project, 670-23.

The two major projects currently underway at the Bank are the above mentioned 670-23, "Scope for Capital - Labor Substitution in the Mechanical Engineering Industry", and 671-51, "Appropriate Industrial Technology". The former project has been underway for much longer than the latter, more resources have been invested in it, and the output is much more impressive. The latter project is just beginning.

The research tack taken by the current project on capital labor substitution is to explore in microeconomic detail the nature of the production processes involved in particular areas of manufacturing, and to examine the range of capital - labor substitution available in each. The project has had to face methodological, empirical, and theoretical questions of considerable difficulty. The struggle has been valiant, and the project has provided an analyses of production processes and the range of choice that is more detailed and more sophisticated than other work probing similar questions. Other studies under the project have explored the choices of technique actually made by firms, and the market and other institutional factors which have influenced those choices (which in a number of cases have been much more capital intensive than would make sense from an economists point of view). The project appears to be having considerable impact within the Bank, and while academics tend to lag in the development of their appreciation of work of this sort, the importance of the project is beginning to be recognized by the academic development economics community.

As mentioned above, the project on Appropriate Industrial Technology is just beginning. The project has two thrusts: measuring the gains to less developed countries from adopting more appropriate technologies, and examining the capital goods sector in less developed countries as a possible



major determinant of the availability of appropriate technologies. A few illustrative numbers have been put together on the first issue, and some literature review, casual empiricism, and thought have been directed at the second. The work, particularly on the domestic capital goods industries, shows promise, but it is too early to tell how successful the endeavor will be.

Projects 671-51 and 670-23 complement and support each other. The relevance to good policy making of better understanding of choice, or (more sharply) understanding why more capital using techniques are chosen when less capital using ones are available, is obvious. The more recent reports on both projects reveal the authors' growing concern about innovation, as contrasted with choice among "obvious and available" alternatives, and a complementary interest in institutional structure. These intellectual developments will be discussed later.

### III. GENERAL EVALUATION

The work reviewed above generally merits a high rating. The research has contributed and is contributing to knowledge. By and large the Bank has exploited its comparative advantage, has avoided duplicating work that was being done elsewhere, and has proceeded in good awareness of that work. The research certainly has influenced the thinking within at least some parts of the Bank regarding the importance of the capital labor substitution problem and the scope of possible substitution. At the present time efforts are being made to implement this appreciation in project decision-making. The work seems to have influenced thinking regarding both the importance of having the right prices, and the importance of having institutions that channel funds and information effectively. Particularly the empirical work has involved researchers in less developed countries. By and large the work has been led by people full-time at the Bank. Where outsiders or part time consultants led, the results were much less satisfactory.

#### IV. RECOMMENDATIONS REGARDING FURTHER RESEARCH

We think that Bank research exploring and documenting the range of possible capital-labor substitutions in particular technologies is now running into diminishing returns. The general point that there is a wide range of choice is now well substantiated. The techniques involved in identifying the relevant substitution possibilities in particular technologies may now be ready for practical application. We note that practical application almost certainly will involve less detailed breaking down of processes than was relevant when the endeavor was viewed as a research study. We think this research endeavor should be phased down and effort placed in helping the operative divisions and departments to be able to themselves do the relevant analyses.

The project on appropriate technology presently is being conducted at a modest level, and in an exploratory manner. We think that particularly the part of the project exploring the design capabilities of domestic capital goods producers has considerable promise. If that endeavor begins to yield interesting findings, suggesting that a considerable design capability exists or can be encouraged, then the project would be a serious candidate for more resources.

The Bank's research under the current two projects increasingly is highlighting that effective capital-labor substitution, the adoption of appropriate technology more generally, and over the long run the development of an efficient industrial sector, is not adequately described merely in terms of "picking things off the shelf". The research has documented the wide range of catalog items available. But it also has documented that it is no trivial matter for a firm to be aware of more than a small portion of the range of choice. Increasingly

the research is showing that at the least adaptation and in many cases innovation is involved in effective choice of technique. Recognition of the importance of adaptation and innovation calls attention to the entrepreneurial dimension in business leadership, and to the institutional structures that encourage, support, constrain, and deter entrepreneurship.

We detect an ambivalence on the part of the Bank regarding putting their research commitments where their own research findings indicate they ought to be put. The tradition of economic research at the Bank has stressed neat, quantitative, formally specified models; there has been a reluctance to delve into areas of economic research where precise models and econometric technique can not serve as the primary tools. And in fact the batting average in projects where there was little analytic structure has not been high. Issues of innovation, entrepreneurship, and institutional structure tend to be viewed as "unresearchable" or at least not amenable to rigorous research. But we propose that the logic of the Bank's own past research endeavors has led it inexorably to a requirement to engage in this kind of research. And the methodological situation is not as bad as some people in the Bank may think. Over the last decade both the interest and the rigor of research on industrial organization has increased greatly. A considerable body of good and rigorous research on the economics of technical change has evolved. However, for the most part, research in these fields has been focused on issues and phenomena in advanced countries, not developing ones. We think the Bank should accept the obvious challenge.

It may be useful to give several examples of the kind of work on industrial organization and technical change that might serve as models for research at the bank. Over the past half dozen years a considerable

amount of research has been done, principally by American economists, aimed at exploring the costs of government regulation. A good reasonable overview of much of that work is provided in A. Phillips (ed) Promoting Competition in Regulated Markets . While we are nervous that much of the research by American economists on regulation has stressed the costs and downplayed the benefits, the identification and attempted quantification of the costs has been illuminating, and such work has had a significant effect on policy.

The work of Edwin Mansfield provides good examples of quantitative studies on R & D, technical advance, and productivity growth. A good collection of Mansfield's earlier work is contained in his Industrial Research and Technological Innovation . A recent study of his (and his students) in which he attempted to measure both private and social rates of return on industrial innovation was published as "Social and Private Rates of Return on Industrial Innovation", QJE, 1977. There also has been a very good work on the economics of technical change in industry undertaken by Christopher Freeman at the University of Sussex, often in collaboration with other members of Science Policy Research Unit there. A portion of that work is presented, in capsule form, in Freeman's book, The Economics of Industrial Innovation:

There has been some work on technical change in less developed countries, but not much. There are several interesting studies focused on agriculture. Hayami and Ruttan's book, Agricultural Innovation, is a useful reference. At the present time Jorge Katz is directing a study on innovation in Latin American industry under the joint auspices of the IDB and ECLA. These studies represent a start. We think the time is ripe for the Bank to get into the field in a big way.