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4 - INDUSTRIAL DEVELOPMENT & TRADE PANEL
Meeting FEBRUARY 1979 (3)

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

WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Messrs. Lindbeck, Donges, Bacha & Nelson DATE: February 8, 1979

FROM: Suman Bery

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.

4th Draft.

1979.02.01

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

CONTENTS

CHAPTER I

GENERAL ISSUES

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

When addressing the issue of Bank research in this field, it is important to remember that "research", as defined by the Bank, is only a small fraction of general analytical work going on in the Bank, and that research on industry and trade is only a small part of the total research effort. Broadly speaking, analytical work of various kinds - including the assessment of economic trends and policies in various countries, sectors and markets - comprise approximately 1/6 of the administration budget of the Bank. About 1/4 of this analytical work seems to be formally classified as "research", of which approximately 1/7, covering the activities of 10-11 man-years of researchers, is on industry and trade, i.e. the area that is the subject of this report.

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

I:1 Bank research - why and for whom?

An important point of departure when assessing Bank research is why the Bank is, and should be, engaged in research, and who the audience of Bank research is supposed to be. A hint on these issues is provided by a formulation in our terms of reference according to which the research objectives of the Bank include the task "to support all aspects of Bank operations ..." and "to broaden our understanding of the development process". Thus, the audience of Bank research should, according to these statements, be both Bank representatives who are responsible for general policy issues and staff members who are engaged in more narrowly defined Bank operations. However, it is obvious

that Bank research may be of great relevance also for people outside the Bank. In fact, the Bank has regarded it as a duty both to stimulate research in the less developed countries, and to produce research knowledge for people outside the Bank, including the "community of scholars" around the world in the field of development economics. In addition, research is stimulated in these countries when the Bank fulfills its advisary role.

But before looking into the implications for the research policy of the Bank of the needs to support Bank operations, and to stimulate research in the less developed countries, it is interesting to explore the implications for Bank research of its comparative advantage as a research unit, neglecting for the moment the issue of "for whom" Bank research is supposed to be performed. By applying the notion of comparative advantage we could conceive of a "ranking list" of research areas in terms of falling relative advantage of the Bank as a producer of research - a list to be "cut off" at the point where the research budget of the Bank is used up. In other words, for what kinds of research is the Bank, given its lending and policy advising activities, a particularly good location?

It would seem that this appraoch to the issue of research priorities would follow from a research philosophy according to which the Bank, in the most efficient way possible, tries to contribute to the accumulation of research knowledge in the world as a whole. Thus, the Bank would be regarded as a producer of "public goods" in the form of scholarly knowledge in the field of development economics. The choice of research priorities of the Bank would then be determined solely by the Bank's ability to produce research, and not by its internal demand for (use of) research knowledge.

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

(a) The research of the Bank should concentrate on fields where particularly competent researchers are available within the Bank, or (in a longer perspective) can be hired by the Bank.

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

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

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

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

Aspect (a) would imply that the Bank continues with roughly the same kind of research as it has successfully pursued so far (assuming that leading researchers within the Bank are not likely to leave), but also that it moves into areas that are suitable for highly competent researchers who can be hired by the Bank. In fact, the choice of the Bank's research topics will probably always reflect the interests and background of "dominating" researchers in the Bank. This has certainly been the case in

its previous research activities, as illustrated by the research on trade policy incentives, with an emphasis on effective protection and domestic resource costs (the field of Bela Balassa), growth patterns and sources of growth (the field of Hollis Chenery, Don Keesing and others), and investment programming and the range of technical choice (the fields of Ardy Stoutjesdijk and Larry Westphal). In fact, a good forecast could have been made some years ago about the "revealed" research priorities of the Bank during recent years simply by looking at the "background" of the economists who had already been hired by the Bank!

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

Aspect (b) suggests research in fields where knowledge is acquired (rather "automatically") by the Bank in connection to the operating and the policy advice activities of the Bank. In fact, the Bank offers to researchers the incentive to work in fields close to policy formulation and implementation, which makes the Bank a natural place for policy-oriented research.

One example is research on investment project evaluation. Studies of investment projects that are relevant for several countries at the same time may be particularly suitable for the Bank, as compared to other (usually national) organizations. Another example of a field where knowledge that is useful for research activities is acquired by Bank operations is probably studies of government policies and regulations. And a third quite related example is studies of institutions and incentive systems in markets for products, services, credit and labor - and the consequences of these circumstances for rates and patterns of economic development.

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

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

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

Aspect (e) finally, would suggest a shift in Bank research activities to more collection, processing and publishing of data that comes out "naturally" from the operational activities of the Bank. However, to make this activity a main task of the Bank - i.e. to turn the Bank into a dominant "data bank" in the development field - would require truly enormous resources. We know how great such a task is already on a national scale. To do the same thing for about 100 countries would therefore require a formidable effort. It may therefore be reasonable to limit demands on the Bank in this field to take a greater responsibility for the data which it actually collects and uses in its own research and surveys, and to make these data available for outsiders to a large extent. It may be noticed that this is not an issue only about publication of research results, but also about improving and controlling the quality of data, which may very well be regarded as a "research task". However, resource constraints make it necessary, we believe, to take a rather selective approach to the collection, improving, controlling, processing and publication of data.

A rather special reason why the World Bank may have a comparative advantage relative to universities in research activities such as (b)-(d) is that these activities, to be efficient, often require a rather strong and permanent research organization, which the social science departments of universities often do not have.

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

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

What would be the concrete consequences of following this principle rather than the principle of comparative advantage in production? In the light of the activities of the Bank as a lender, investor and policy adviser - and considering the often

highly distorted
relative prices in many LDCs - the heavy emphasis on studies by the Bank of effective protection and domestic resource costs could certainly be defended on the basis of the principle of "residual supplier of research". In fact, it would seem that these studies have been regarded as particularly useful by operations people, though some complaint comes out to the effect that the techniques may have been somewhat "overelaborated" for the purpose of Bank operations.

Another inference of the principle of "residual supplier of research" is probably that it motivates research concerning large investment projects (in particular where the Bank is involved as a lender), including analyses of the externalities (such as learning by doing) of the projects, returns to scale and the linkages between sectors. The research within the Bank on investment planning and programming is an example of this type of research for which an important point of contact seems to be the Industrial Projects Department (IPD). It would appear, however, that the operational staff of the Bank, particularly in the regional offices, has usually not found the research done in this field very helpful.

However, we would argue that an adherence to the principle of "the residual supplier of research" motivates studies also concerning problems about which Bank research has not so far been strongly involved, but which in our judgement reflect severe bottlenecks for economic development in many LDCs. This brings us back to a recommendation of some research that earlier was motivated by

"the comparative advantage principle", i.e. research on (1) comparative government policies and regulations, (2) institutions, and (3) incentive regimes not only in product markets (where the Bank has done considerable research efforts) but also in factor markets. The argument for more research efforts on the roles of institutions is probably particularly strong if the Bank would choose to concentrate research more than earlier on the least developed LDCs, and on the consequences of industrialization for the least favored group within individual countries.

Thus, the residual supply of research approach and the comparative advantage approach 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?

We shall later on (Chapters II and III) discuss in some detail both why more research of these types are important and how they may be conducted.

A second reason why Bank research cannot be based only on the principle of comparative advantage in the production of research is that there are important "externalities" of having researchers within an organization. For instance, in many cases research done within the Bank may be more "visible" to the operations people in the Bank than research done elsewhere in the same field. Moreover, the only efficient way of making practitioners in an organization aware of what researchers outside the organization are doing is probably to have a number of researchers within their organization (a point to be developed in the next section).

It may also be argued that the general level of sophistication of an organization, for instance in the field of policy advising and economic surveying, is influenced by the general quality of the researchers within the organization. The presence of good researchers, like competent people in general, helps to set standards of performance within an organization, which is particularly important in one that, like the Bank, performs the role of policy adviser. Moreover, the reputation of the Bank as an institution of high competence may be boosted by a high research capacity. Such a reputation may help the Bank to hire talented people in general, and hence improve the possibilities of the Bank to work efficiently, and perhaps even to "survive" in a long-run perspective.

In other words, the research of the Bank should not be subordinate only to the (relative) efficiency of the Bank as a

producer of the research, or to the immediate demands of the operating units of the Bank, but also to long-run considerations of the general competence of the Bank in various respects. In particular, it is importnat to stimulate the capacity of the Bank to translate policy objectives into instruments and actions. An application of this principle suggests that the Bank should build up research competence in many fields where it operates - for the purpose of raising the general level of sophistication within the Bank.

Thirdly, the Bank should also, in our view, be interested in the "externalities" of its resarch on the research potential of the LDCs. The adherence to this principle is an argument for choosing fields of research that are of interest for scholars and research institutions in the LDCs, perhaps in particular from some of the least developed ones. However, there is then some risk, of course, both that the Bank will not always get the most competent researcher for a specific project, and that the Bank will be criticized for "distorting" research in the LDCs ("research imperialism"). It is difficult to say anything in general on this issue, except that LDC researchers should be included to a considerable extent, though the quality of the research, in our judgement, is an overriding task which should not be jeopardized.

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

(a) a comparative advantage approach;

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

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

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

In reality, it would appear that all these four principles for research do prevail within the Bank. For instance, past Bank research has certainly been addressed both to the community of scholars outside the Bank and to staff members who are responsible for policy advising and lending within the Bank - in proportions that we cannot really pin down. Controversies over research strategies within the Bank probably often derive just from the different weights that various staff members put on these various principles. Disappointments over the research activities of the Bank are bound to be felt by those who evaluate the research efforts of the Bank on the basis of 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 entrepreneuship, innovation and technological development. For an important question in these fields is under what general conditions (policies, institutions, systems of incentives and perhaps also cultural characteristics) entrepreneurship, technological advance and innovations are likely to flourish. Studies of the determinants of "work ethic" is another topic that may require rather broad inter-disciplinary knowledge and methods of analysis.

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

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

Dissemination of research knowledge

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

The importance of the issue of dissemination and assimilation is underlined by the fact that one of the most striking findings of our interviews among people in the Bank is the enormous "gap" - one is tempted to say tension - between researchers and operating officers. This is nothing peculiar for the World Bank, however. Researchers within an organization that deals with "practical affairs" will probably always be somewhat of an "academic enclave" of that organization. Research is a full-time, highly specialized job, which has to be protected to a large extent from demands of practical and administrative duties. The enclave character of a research unit helps to give such a protection, and thus

helps to create the "community of scholars" in which high-quality research can be generated. In fact, if an organization like the Bank wants to acquire and keep competent researchers, it is necessary to let them "do their own thing" to a large extent, without too many disturbances from other activities within the organization.

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

Moreover, whereas researchers are usually interested in the accumulation of generalized knowledge, operating people are more interested in drawing on knowledge, in particular on rather specific "knowledge about time and place". The researcher often regards the search for the latter type of knowledge as "information gathering" rather than research. Operating people, by contrast, are frequently disappointed by research results because these do not always give concrete, easily accessible, ready-made and unambiguous conclusions about immediately relevant operating and policy issues.

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

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

(1) That the researchers write, and circulate within the Bank, popularly written reports on research - concerning research produced both inside and outside the Bank. To the extent that the reports summarize "outside research", the processes of import and dissemination are of course combined. In some cases it may be a good idea to ask outside consultants, rather than researchers inside the Bank, to make the surveys. However, when outside research results and paradigms are summarized, it is probably important that not only outstanding "academic" contributions are summarized. It may also be useful to try to find out what types of research that have been successfully used in other "operating" organizations.

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

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

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

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

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

(6) Moreover, the earlier suggested employment of people with research background in operating positions would not only facilitate the import of research knowledge to the Bank; it would also be a way of "disseminating" and "assimilating" research knowledge within the Bank from researchers to the operating officers. This is potentially important, as imported research may be more difficult to disseminate and assimilate than "in-house research". Both the suggestion to let researchers circulate between research and operating positions, and the suggestion to recruit (more permanently) people with research background to operating positions means that some "bridges" would be built between research and operation activities. Thus, we suggest in fact that the Bank more systematically tries to build up a staff of "bridge people" in the operating departments.

It is important to realize that the limits of using more research knowledge within the Bank are probably determined more by the "absorptive capacity" of research among the operating people - limited time as well as limited ability and interest to absorb research knowledge - than by the capacity of researchers within the Bank to produce and summarize research. This

means that a larger volume of research within the Bank should perhaps not be expected to have much effect on the operational side of the Bank, as long as the deficiencies of the systems of dissemination and assimilation of research within the Bank have not been removed.

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

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

For instance, if we talk about countries with a strong emphasis on central planning, research on nationwide planning models, and empirical studies of the process of central planning in various countries, would probably be a top priority of research. It would then, of course, be important to remember that there are substantial elements of decentralization of information, decisionmaking and initiatives also in "centrally planned" economies. Thus, it is of great interest to study incentives and constraints on behavior at various levels in centrally planned systems.

The reason why the Bank has not put much research resources to the acquiring of research knowledge in this field is most likely that very few member countries of the Bank are centrally planned economies, though of course elements of it exist everywhere, because of the great role of government decisionmaking in

economic matters in all countries of today. This means, of course, that the process of government decisionmaking is an important area of research for all countries.

(The macro models that have been developed by the Bank for entire economies, or even for the whole world, should probably not be regarded as tools of central planning, but rather as descriptive model or forecasting models.)

A more modest version of planning would be sectorial planning or programming of investment decisions, for instance in sectors where there are huge returns to scale, externalities, or (direct) intersectorial linkages. Then it may also be possible to consider aspects which are not usually well cought, if at all, in conventional static microeconomic investment calculations. With this approach, studies of investment planning in some sectors would be of rather high priority. It has in fact also been so for the Bank in recent years (see section III:6), in the sense of "investment programming" - mainly in cases where the optimum size of a firm is of about the same magnitude (or larger) than the entire national market. Another reason why sector planning models, or at least investment programming models, may be of interest is that in many countries, perhaps in particular in less developed countries, a number of infrastructure and process industries, for which investment programming models may be particularly useful, are in fact under rather detailed central government control.[1]

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

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

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

In most of the member countries of the Bank, the bulk of the development process is no doubt guided by decisions by decentralized units motivated by profits and stimulated and constrained by markets. However, it is important to realize that the adherence to a pronounced decentralized strategy of economic development, in the context of a market economy, does not imply the absence of central policies and planning, but rather the reliance of different types of policy and planning operations than in centrally planned economies. Obvious examples, beside general monetary and budget policies, are institutional reforms and

improvements in the systems of incentives. In countries of this type it would therefore be of interest to find out how conducive alternative institutional arrangements and incentive systems are for releasing efficient decentralized initiatives.

While research on incentives in product markets, in particular trade policy incentives for firms, has been given a high priority in Bank research (the field covered by section III:1), the same cannot be said about research on incentives and imperfections on factor markets and incentives for employees (households). By this we mean for instance the structure of interest rates, the performance of credit rationing and the mobility and flexibility in general of the credit and capital markets, but also the structure of wage rates, the incentives and possibilities for labor to move, acquire skills, and advance.

Nor has there been much emphasis in Bank research on the importance for economic development of institutional arrangements (the field covered by section III:4), though an increased interest in institutional factors can be detected in various research efforts of the Bank in recent years.

The only aspect ("dimension") of development strategies mentioned so far has been types and degrees of centralization of economic decisionmaking, which of course is a dichotomy concerning the mode of organization rather than concerning the allocation of resources. In reality, the development strategies of various countries differ of course with respect to the allocation of resources as well. For instance, one important choice

is between export-oriented (outward-looking) and import-substitution-oriented (inward-looking) strategies of economic development - a topic highlighted by Bank research on trade policy incentives and patterns of growth.

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

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

In reality, the economic systems of the LDCs which are members of the World Bank are of course characterized by various combinations of centralized and decentralized decisionmaking, and with different strategies to employment creation, redistribution and the provision of "basic needs". Bank research should reflect this diversity concerning strategies of economic development. So should, of course, our recommendations as well.

2nd draft
January 30, 1979

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 Southeast Asia), 671-05 (Patterns of Industrial Development), 671-32 (A Comparative Study of the Sources of Industrial Growth and Structural Change), and 671-79 (Sources of Growth and Productivity Change), and two non- RPO studies as well. The unifying theme is the objective of explaining the pattern of resource allocation within and between countries, economic growth, and changes in industrial and trade structures as a function of various country characteristics, including policies employed. While two projects (670-19 and 670-79) were clear failures, the other ones met, by and large, this objective. The failures are regrettably indeed, since the projects addressed the questions of how to shift manufacturing activities from developed to developing countries and of how to plan competitive export industries in the developing countries; information on these matters would have been an extremely important ingredient of any effort to shape rational industrialization strategies. The projects 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 macroeconomic formulations. The operational staff does not attach a high utility to this type of research. And it is not clear

to us that the Bank has a comparative advantage in this field.

A third cluster of research has referred to export promotion policies in the developing countries and access to markets in the industrial countries. It included RPO projects 670-20 (Industrialization and Trade Policies for the 1970's), 670-21 (Export Promotion and Preferences: A Case Study of India), 671-35 (Export Incentives in Developing Countries), 671-56 (Marketing Manufactured Exports), 671-66+67 (Effects of Increased Imports of Manufactured Goods from Developing Countries in Western Europe and in the United States, respectively), and 671-68 (Key Institutions and the Expansion of Manufacturing Exports). All but the first two projects are still under way; five non-RPO analyses have also been finished. All of these projects aim at testing the hypothesis, prominent at the Bank, that successful expansion and diversification of exports is a key characteristic of many recent development experiences. The research includes data compilations, surveys of selected industries, analyses of commodity markets, analyses of problems of import restrictions by the developed countries on the less developed countries' manufactured exports, consideration of national policies and institutions for trade promotion in the developing countries, and analyses of the overall environment for the exports of the less developed countries. As was the case with the projects discussed in the paragraph above, the research methodologies and styles of the projects considered has differed widely. The more descriptive studies on selected industries provide a great

deal of empirical information which, however, will be useful only if they are kept up to date. Among the completed RPO projects the one on India, while promising in itself as a case study of a less successful country, has been disappointing from both the methodological and the policy analysis point of view. High praise deserves the project on industrialization and trade policies; it made an important contribution in documenting the impact on industrial countries of the manufactured export expansion by developing countries and it has stimulated further research on adjustment problems in developed countries. Of great relevance to the operational staff and policy makers might turn out the ongoing project on export incentives in developing countries, which is designed to yield practical information for countries that contemplate the effective promotion of export activities. The ongoing research on key institutions holds also good promise and will fill a large gap in existing information on the marketing of exportables. The two ongoing projects on import market penetration in developed countries reflect a serious attempt for understanding the political economy of trade proteccionism; while the topic is important from the export-oriented developing countries' point of view as well, it is questionable that the Bank has a comparative advantage of conducting large-scale research in this field.

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

projects have not yet been finished and, in some cases, they are still at the conceptual stage. It is therefore difficult to provide for a definite evaluation of the research in this field. In contrast with the work on export promotion which proceeded under the presumption that the objective was clear enough and that the task was to find the appropriate instruments, these projects have faced much more uncertainty regarding what ought to be achieved. The difficulties for the researchers were compounded by the fact that the existing literature is mainly descriptive rather than analytical. While applauding the willingness to enter this important area, the panel recognizes that research on these topics is struggling towards appropriate methodologies, but has not yet securely found many. We also notice that research on capital market imperfections and public enterprises put too much emphasis on the experience of one country, namely India. And while the quality of the research done so far is good by international standards, it is still below the quality levels achieved in the other areas reviewed in this report. It may be some time before the Bank develops the capability to do first rate studies of small-scale industries, credit markets and public enterprises and their influence on development.

Like the work on incentive regimes, trade patterns, and export promotion, and unlike the research on small enterprises, credit markets, and public enterprises, Bank research on capital utilization, capital-labor substitution, and technological change (the fifth cluster) has proceeded within well-defined methodologies and has tested concrete hypotheses. Included here are the RPO projects 670-54 (Employment and Capital-Labor Substitution), 670-23 (Scope for Capital-Labor Substitution in the Mechanical Engineering Industry),

and 671-51 (Appropriate Industrial Technology), most of them are completed. Of three non-RPO studies two are completed as well. The policy thrust of the research has been provided by the observation that labor is cheap and capital expensive in less developed countries relative to developed ones, that this ought to be reflected in use of more labor-intensive techniques, but while this has been happening to some degree it still is possible and desirable that the techniques employed be more frugal in use of capital. At the same time, it is shown that despite the relative scarcity of capital in developing countries, productive capacity is not used very intensively. Research has been concerned with market and other forces that explain the prevailing situation, and with policies that could improve the environment so the choice of technique could be made more appropriately, techniques used more efficiently, and appropriate adaptation and learning proceed more effectively. The panel finds the recent work exploring in great detail the scope of capital labor substitution in particular technologies important and illuminating, but running into diminishing returns as a research endeavor. The work on appropriate industrial technology, particularly the research on the design capabilities of domestic capital goods producers, is promising, and may lead to important further research.

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

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

II:2 Overall Evaluation

The panel attempted to evaluate the research undertaken by the Bank along a range of dimensions, reflecting the multiple purposes of research at the Bank. Some of our criteria related to the Bank as a research producer and as a member of the scholarly research community. Here we attempted to assess the contribution of Bank research to the understanding of the economic development processes and policy issues relating thereto. What was the absolute quality (in some sense) of the research output of the Bank? To what extent did Bank research reflect its comparative advantages? To what extent did Bank research proceed in conscious awareness of the research that had been done and was going on elsewhere? Other criteria related to Bank research viewed as a contribution to the applied objectives of the Bank. How

useful has the research been in guiding Bank decision-making, either regarding lending operations or regarding policy advice? How useful has the research of the Bank been to policymakers in the less developed countries? What contribution has the Bank research program made to the building up of indigenous research capabilities within the less developed countries?

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

The several sections of chapter III go over these questions field by field. The panel noted significant differences in the overall quality and relevance of Bank research in the different fields, and the more fine-grained evaluations also differ from field to field. However, there were certain general and common judgements that we made. These we recount below.

By and large, we are impressed by the overall high quality of Bank research on industry and trade in economic development. Viewed solely in terms of its research output (much of which has been published), the Bank clearly ranks as one of the most distinguished development research centers in the world and certainly the leading one among international organizations. In many cases, the researchers have made a remarkable effort to improve the methodology for policy analysis and investment appraisal. The work has been to a large extent creative rather than imitating and, in its applied 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 everwhere) 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 advicing and operating activities of the Bank. This holds in particular perhaps for the studies of effective protection and domestic resource costs, and to some extent also the studies of technological choice and investment programming in process industries.

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

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

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

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

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

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

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

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

This means that recommendations regarding future research priorities must rest on subjective judgements regarding a number of matters, including the importance of different kinds of research in enhancing general understanding of development processes, the comparative advantage of the Bank in

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

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

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

We have the same judgement regarding Bank research on process industry investment programming, though some "software" development is necessary to make applications more routinized. What is needed now is for the operating departments to develop the capability to work with the models.

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

The panel is somewhat divided regarding whether or not the Bank should cut back on its research on programming models for non-process industries, and the economy-wide models based on a computable general equilibrium framework. Most of us doubt that these bodies of research will contribute much directly to understanding relevant to policymaking. We believe that an understanding of the development process requires mainly other types of knowledge than is likely to be acquired by these projects. Thus, the majority of us are rather sceptical about the fruitfulness of this type of research, relative to some other fields. On the other hand, the work is methodologically exciting and on the frontiers, and enables the Bank to attract and hold several very well-tooled economists. The arguments for continuation of these projects it seems to us must rest on the importance to the Bank of having on its research staff several economists who are technically very skilled.

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

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

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

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

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

Among production-oriented fields (points 3-8), many of us would stress problems of factor market distortions (point 3); enterpreneurship, innovation and adaptation (field 6), and technological change and appropriate technology (field 7). The reason is, in our view, that in decentralized market economies with a considerable scope for government decisionmaking, governments can do

much for releasing, or destroying, decentralized, productive incentives by way of incentive policies in a broad sense - tariffs, taxes, subsidies, wage and labor market regulations, licencing systems, training, technological research and various types of controls - as well as by helping to develop institutions that are conducive to vigorous entrepreneurship and a sharing of the fruits of economic development by broad groups of the population.

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

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

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

Another broad set of subjects to which we think priority should be given involves mechanisms of technology transfer, adaptation of technology to better fit local economic conditions, innovation in industry in less developed countries, and the policies and institutions that support and

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

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

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

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

If the Bank contemplates a shift of emphasis of research to the new fields suggested here, it would probably be a good idea to appoint an ad hoc group of researchers with the task of undertaking a research program in some of the fields suggested here, for instance concerning factor market distortions, the functioning of labor and capital markets, entrepreneurship, innovation and technological development and adaptation. As we have

indicated, such research should probably often use the technique of comparative studies of nations. The ad hoc research planning committee should include outstanding researchers outside as well as inside the Bank. Example of types of scholars are specialists in industrial organization, technological development, innovation, credit and labor market analysis (labor economics). It is, in our judgement important to include people with a strong theoretical and analytical competence, rather than people that have studied institutions on a more descriptive way.

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

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

Against this background, it is not unreasonable to increase the number of scholars of the Bank in this field with at least a handful of highly competent persons. This is, in fact, a prerequisite for shifting research to the areas which, according to our opinion, should be given higher priority in the future than in the past. It will, of course, be the size of these new resources that sets the limits for how many new departures may be envisaged.

Juergen B. Donges

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February 2, 1979
Third Revision

EVALUATION OF WORLD BANK RESEARCH ON INDUSTRY AND TRADE

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

The Subject

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

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
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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 sensibilize 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

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.

2nd draft

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 algori-thm 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 responsability 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 and 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 discusions 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.

Paralel 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 'hormality' 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 late1978. 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 accouting identity. However, as his papers for the Nobel Symposium and for the Tokyo IEA Congress (the later co-autored 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 pther 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 and 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 apparently answer/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 conforting 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 this is 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

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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. Keesing, 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 8 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 seperate 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 1978), is a timely study valuable to the entire development economics community. It compares trade liberalization and economic growth prior to the oil crisis with trade barriers adopted during the post-crisis recession. Recent non-tariff restrictions, government aids to industry, international cartels, and market-sharing in major developed countries are documented and their effects evaluated. Concluding that the risks of new protectionism are high, Balassa proposes policies for long-term growth, structural adjustment assistance, and an international code of good conduct. Semi-industrial developing countries were also advised to reduce existing protection, to upgrade and diversify their exports, and to 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

Project No.	Project Title	Responsibility	Date of Approval	Date of Completion
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. Sharpston, M., "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?

G. Bueno
January 1979

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 responsibles 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.

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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 underdeveloped 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 exemple, 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 tented to concentrate in the growth poles of the South East Brasil. Therefore, recommended (something that the Braziliens 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 developping 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

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has a good approach and that it will contribute to diffusion of the knowledge through the participation of researchers from the developping 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 explicitely presented once

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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.Amgonst 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 responsability 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

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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 dwelve 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 mechanismes from the point of vue 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 abandonned in favor of more "proved" lines.

20.- RPO 671-69 on capital markets imperfections and economy developments owesmuch 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 preocupations 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 preocupation 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.

22.- The third subject of research is covered by two papers of which one presents a general model and the other attemps an empirical verification of some of the issues raised in the first one. But the latter, in fact, is more than an extension of the former.

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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 adequatly 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 agregate 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.

25.- Another paper examines the relationship between earnings and portfolio of the commercial banks of three Asian countries: Philipines, 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 analysis of this type are scant in developping countries.

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.

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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 previoulsy referred to on banking in the Philipines, 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.

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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

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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

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to say that research of this type is sorrowfuly 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 inocuous. 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 substancial experience in this regard and, at least, we think that it will not be substancially 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 preocupation 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 leat 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

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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 case. 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 preocupation in many LDC's about the relation between technological development and overall economic development. This preocupations, 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.

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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.

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January 1979

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 exibit 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 <u>particular products</u>, 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) <u>The absolute cost</u> of neglect of interdependence is "by no means trivial" and is "far more" then the cost of conducting studies that account for interdependence.

From a methodological point of view the most significant contribution of the research is the demonstration of the use of large mixed integer programming models. In particular, the following have been shown:

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

(b) Problem with a fairly large number of integer 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 a mixed integer programming one and break-even analysis is developed and used to reduce computational difficulties.

In addition, the allocative consequences of the results are explored in depth. An evaluation of alternative investment criteria is also made in the context of the results obtained from the model. The conclusion is reached that simple 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 "Appro Industrial Technology" (RP0 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. Usefullness 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 LDC's. 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.[1/]

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.

1/ 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 lowcost 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 mannuals 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.

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) Programming 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 implimentation and central planning of a sector such as the mechanical engineering industry characterised by a large number of products and processes is difficult to say the least.

Thus the findings of the Korean Mechanical Engineering Industry study that interdependence 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

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 methological, 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 ragne 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 signficant 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.

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Comments

Comments on Chapter I of the Draft Report

by the World Bank Panel on Industrialization and Trade

Bela Balassa

I.1 Bank research -- why and for whom

1. On the comparative advantage of Bank research, I would suggest deleting the production of "statistical data and other information." (p. 4) While this point is subsequently qualified in stating that "it may ... be reasonable to limit demand in this field to take greater responsibility for the data which it /the Bank/ actually collects and uses in its own research and surveys and to make these data available for outsiders to a large extent" (p. 6), various queries remain.

2. If one is to limit this activity to the publication of data collected in conjunction with the Bank's research activities, it does not deserve a separate consideration -- only a slightly changed emphasis in the dissemination and publication of research results. If, rather, one has in mind the establishment of an information system, permitting easy retrieval, this would indeed be a useful activity but could not be considered research. Finally, the publication of data collected in the course of the Bank's operational activities, in the form of surveys or otherwise, would involve a tremendous effort on the part of operational staff that could not be demanded from operational staff without relieving them from other responsibilities. But, again, an information system -- or data bank -- can and probably should include project data that may be subsequently used by others.

3. In turn, I would add comparative studies as an area in which the Bank has a comparative advantage. This is because of the Bank's unique access to information in developing countries around the globe and the staff resources

it can draw upon. At the same time, it would seem appropriate to separate research areas, such as government policies and regulations, from the mode of carrying out the research, such as comparative investigations (cf. p. 5).

4. Research on institutions is recommended in the report both on "comparative advantage" and on "residual supplier" grounds. As to the former, the statement that "potential for research on institutional and policy-oriented problems has also been acquired by way of sector reviews and studies of small-scale enterprises, state enterprises, and financial intermediaries, for instance within the Industrial and Finance Division (IFD)" (pp. 5-6) is hardly correct. While the Division has done work in several of these areas, this has not been of a high quality and it has rather limited potential for the future. One may also query the suggestion that "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" (p. 9). It would seem that institutional research has not acquired a high status largely because it is "soft" in the sense that it is not amenable to the use of quantitative methods. As noted in more detail below, similar considerations give rise to doubts about its introduction in the Bank.

5. Towards the end of this section, reference is made to "four principles" for research in the Bank. It is only on p. 28, however, that it becomes clear what these four principles are. Also the last one, generating knowledge and research capacity in LDCs would require more discussion, in particular as far as its benefit-cost ratio are concerned. Finally, it would be desirable to indicate the relative importance of the various principles as well as the implications of the recommendations made in the report for the size of the Bank's research program.

I.2 How to make import, production and dissemination of research more effective?

6. The short subsection on the import of knowledge partly overlaps with the subsection on dissemination. If they are to be kept separate, the preparation of "state of arts" papers should be added to the former. As regards the latter, attention would need to be given to the problem of application as distinct from dissemination and assimilation. In fact, the small research unit proposed for the Regional Offices could become a vehicle for applications that should be carried out at the next stage as the Bank's research program has "matured." (This point is taken up in the concluding section of the report.)

7. An indication of the size of the research program is especially important in view of the suggestions made for entering into several new areas, and for applying an interdisciplinary approach in some of them (pp. 15-16). At the same time, the difficulties of carrying out interdisciplinary research would need to be recognized. In this connection, it would be useful to cite *successful* cases of interdisciplinary research involving economists and other disciplines; in the United States we have a number of examples of unsuccessful efforts.

8. In general, the question arises how many new departures one can envisage in the Bank, given its budget and staff constraints and, more importantly, its absorptive capacity and ability to manage research. The existence of these constraints point to the need for limiting the number of new departures. These may be chosen with a view to the applicability of quantitative methods and the wider relevance and reproducibility of the results, so as to avoid ending up with descriptions of occurrences that do not lead to generalizations.

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

9. The central planning-decentralized decisions dichotomy may be a red herring. To begin with, this is not a dichotomy of development strategies as is the choice e.g. between inward-and outward-looking strategies or between basic needs and a consumer-oriented society. Rather, it reflects a mode of organization that is compatible with various development strategies. In this connection, see the comparison made between the outward-oriented strategies followed by centrally-planned (Czechoslovakia and Hungary) and market (Argentina and Chile) economies as against the outward-oriented strategy of the market economies of Denmark and Norway in the first two postwar decades (Bela Balassa, "Growth Strategies for Semi-Industries Countries," *Quarterly Journal of Economics*, February 1970, pp. 24-47). Since that time, Hungary has adopted a basically outward-looking strategy while decentralizing its economy. As the example of Yugoslavia also indicates, decentralization is compatible with socialist ownership.

10. Also, references to the relevance of particular research projects to centralized and decentralized economies offer little interest; practically all research projects are relevant for both. At the same time, as subsequent qualifications indicate, it is not appropriate to consider the sectoral planning of investment decisions as "a more modest version of central planning" (p. 24). At any rate, it is more appropriate to speak of sectoral programming rather than planning.

11. Instead of "central planning," "elements of central planning" and "central policies and planning," one may make reference to the process of governmental decision-making as an important area of research. Thus, Governments

intervene in production decisions by a variety of direct and indirect means, the modalities and efficiency of which would deserve to be studied. This point is a fact made in several contexts in the draft report.

I.4 Future research priorities

12. The recommendations made for the application of research knowledge and for the development of new fields of research are generally fine. At the same time, the question remains how many new departures may be envisaged and what are the priorities assigned to them. Also, the implications of the recommendations for the size of the program for research and research applications would need to be indicated. It would seem that research applications would require a separate budget as it may not be realistic to assume that these would be financed from "the budget for operations and policy formation" (p. 29).

WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Suman Bery, VPD

DATE: January 30, 1979

FROM: Larry E. Westphal, Chief-DEDND LW

SUBJECT: Report by World Bank Panel on Industrialization and Trade

I regret that it has taken me so long to get around to reading the third draft, but it could not be helped. Moreover, I have only been able to make a hasty reading; my comments are thus restricted to major points. I would appreciate your transmitting these comments to the appropriate member(s) of the Panel.

Chapter 1

Page 30: I seriously question the implied conclusion that the studies of growth patterns and sources of growth do not lend themselves to application for use by operational staff. Examples of past uses have been given to the Panel. In turn, the forthcoming industrial sector mission to the Philippines will employ both methodologies, at the initiative of the mission leader, who is a member of the operational staff.

Same page: I am very concerned about how the skepticism regarding sources of growth II and investment programming models for non-process industries will be received within the Bank, particularly with the actions that might be taken by senior management as a result. It is not beyond reason to suspect that the Panel's statement of skepticism, particularly in view of the context in which it is made, would lead to the decision to about these projects before they are completed. I will not here go into all the reasons why I distinctly do not share the Panel's skepticism; suffice it to say that a balanced statement of the projects' objectives suggests both that these are important and that they can be met. To make my point bluntly: I strenuously object to the singling out of any specific projects for such comment, because of the weight that the Panel's findings may carry with senior management. If such adverse comment is to be made, the underlying reason should be stated, and opposing views in favor of these projects should be given equal attention.

The following parenthetical remark with respect to the above may also be relevant: In terms of their academic credentials, the staff involved in the sources II and investment programming for non-process industries projects are among the top research staff in the Bank; they have also shown themselves to be very effective in operational support activities. Given that a statement of skepticism could easily be the basis for highly disruptive action by senior management, such a statement does not seem consistent with frequent commentary in chapter 1 suggesting that the researchers should, to at least some degree, be free "to do their own thing." Moreover, whether it is readily apparent or not, for the researchers involved, both projects constitute major steps toward refocusing the use of mathematical formulations to undertake research on topics (such as industrial organization and technological change) on which the Panel has placed high priority for future work.

Chapter 2

Page 1: Project 670-01 is mistitled, should be Development Strategies in Semi-Industrial Countries.

Page 3, top three lines: The research guided by the more elaborate general equilibrium conception has hardly begun, hence not surprising it has not yet produced results.

Page 5: The suggestion of diminishing returns prompts three comments. First, it should be noted that much research has not yet been written up in final form; I would hate to see a moratorium imposed on research in some areas where complete write-ups are only in the early stage. Second, presumably diminishing returns to further research in an area does not imply either that no further effort is required to "move the research into operations" or that operational uses are ruled out; explicit recognition of the distinction between research and application (through various means, depending upon project) needs to be made, with separate recommendations for each. Third, there may be some unanswered questions which strongly merit research, so that diminishing returns are not across all parts of an area. Does the Panel wish to rule out all further research?

Page 13, middle of page: Note should be made of applications of "work on effective protection rates" that have already been carried out in connection with operational work of Bank. Also, note that Phase II of the Appropriate Technology Project, on choice of textile technology, is joint with relevant lending department.

Page 13, over to 14: Please see comment on Chapter 1, page 30, re expression of skepticism. At least here there is recognition of connection with staff interests.

Chapter 3, Bacha

Page 4: It would be helpful to note re 671-05 that the "analytic" phase of the project is not yet completed; that work to up-date the Chenery - Taylor study is nearing completion and that reports from this work are forthcoming.

Page 7, re micro focused analysis of two subsectors: To avoid misleading the reader, it should be noted that the initial proposal for the project indicated that a detailed proposal for subsectors studies would be forthcoming at a later date.

Page 7, last paragraph: I think the negative aspects are overplayed. On the positive side, would it be denied that static CGE models (or variants thereof a la Taylor/Black) provide better estimates of resource pushes and pulls than do effective protection rates calculated via conventional means? The effort in Sources 2 is to investigate relationships between structures of quantities and prices (and thereby price denominated incentive policies) over time -- this objective seems worthwhile and should be noted. Given this objective, to incorporate the price side more explicitly, what alternative methodology would be advocated in a multisectoral setting?

Chapter 3, Donges

Page 10: The lengthy paragraph which begins on this page and continues on page 11 needs reconsideration; I strongly question its logic. To give but the most glaring example: economic viability has little to do with future changes of protection; financial viability does. The paragraph confuses incentives with resource costs, and economic with financial viability.

Chapter 3, Kim

It would be helpful were note to be made of the severe time constraints under which the individual "surveys, data compilations and analysis" were undertaken. Indeed, work in this category is by-and-large not formal research. While I would agree with the assessment in "absolute" terms, given the severe resource limitations under which this work is done, the assessment in "relative" terms should, I think, be rather more positive. The lesson here is the need for stable resource commitment to this type of work if the Bank is to undertake it.

Page 7, bottom: What is the rationale for suggesting that all these projects should be part of one "coordinated" project? Please clarify; otherwise, perhaps best to omit.

Chapter 3, Nelson

Re the project on Appropriate Industrial Technology: This project is nearly complete, not "just beginning;" that is, within the limits of the proposal accepted by the Research Committee. (The drafts you were sent initially were now several re-drafts out of date.) Of course, further work under new projects will follow from this project, but it will be much more narrowly focused. If more than this is implied as being necessary, it should be spelled out, please.

Page 6: Re diminishing returns, please see comment on Chapter 2 suggesting note should be made that preparation of final reports (particularly under 670-23) yet remains; this will take some time and effort. Are diminishing returns also seen to further work on capital goods production in LDCs (re 671-51)?

Chapter 3, Parikh

I have major reservations concerning this chapter, sufficient to ask that it be thoroughly edited to correct a number of fundamental mis-statements. To cite only a couple of glaring examples: page 6, bottom of page -- the statement of the conclusion that simple benefit-cost criteria or measures of comparative advantage are adequate is nearly the precise opposite of the conclusion reached by the authors of the study. Page 10 -- the suggestion that case studies be carried out by a local team corresponds to procedures that have already been used in several applications to process industries; the implication that it has not been tried is thus false. Page 11 -- the application to the pulp and paper industry in Malaysia in fact does examine pros and cons of decentralized versus centralized industrial structures as well as the cost and benefit of rural based industrial development. Again on page 11 -- the application to the Egyptian fertilizer industry gave explicit attention to the efficiency with which large

projects were being operated, and considered this question both independently of and in respect to investment options. Page 12 -- the authors of Korean mechanical engineering industry study themselves state in the strongest possible terms that further studies to test the generalizability of their results are required; they even go so far as to note that their own micro level research in progress suggests that the conclusions are highly questionable and very much dependent upon the level of aggregation employed in the initial study.

The point of the immediately preceding comments is to suggest that Parikh's chapter be edited to achieve a more adequate reflection of the research that has been carried out; it would be also desirable to achieve more balanced exposition leading to a less ambiguous statement of Parikh's conclusions regarding major points, and of his recommendations. My impression from talking to Parikh is that he would strongly support further work leading to widespread implementation of the methodology, both with respect to Bank projects and more widely in LDC's; this does not come across from his write-up. Mr. Stoutjesdijk and myself would be willing to re-write the chapter if this would be acceptable; otherwise, we will provide a detailed set of comments.

LEWestphal:mm

cc: Mr. B. Balassa, DRC
Mr. B. B. King, DED
Mr. A. Stoutjesdijk, DED

WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Research Panel on Industry and Trade DATE: February 8,1979

FROM: Helen Hughes, Director, Economic Analysis and Projections Department

SUBJECT: Your Draft Report

1. We found your draft report is well conceived and to the point. Chapter I creates the relevant conceptual framework, clarifying issues that have been loosely discussed in the Bank for some time but not stated clearly, and Chapter II summarizes the research to-date effectively. In our view you point to the correct priorities.

2. Our main question is about the lack of an attempt--and we realize how difficult it would be--to make some judgments about cost/benefit issues, particularly of in-house versus RPO "research". We also find that the tone in these two chapters, particularly Chapter II, somewhat too complimentary. The Bank is a large, tough, wealthy organization with many characteristics of a monopoly. It should be able to take criticism, particularly if it is as constructive as yours.

3. I recognize that the report is yet to be edited as a whole, and so wish to point to some discrepancies between the first two chapters, and the individual research reviews concerned with two projects with which I was associated previously, and with the market penetration project. These discrepancies no doubt arise in part from the unduly short time which the Panel has had to devote to its task, and its consequent inability to discuss projects with the responsible researchers/supervisors.

Export Promotion-India (RPO 670-21)

This project was fraught with difficulties from its start. There were serious personality conflicts, stated in "methodological" terms, which I always found puzzling since all parties were wedded to neo-classical economics! It is true that the project was never completed in the sense of publication. However, it also had some successful aspects. Its conclusions, presented in a large document (of some 500 pages) issued by the Trade Development Authority (but evidently not available to the Panel) were used in a series of Bank reports on India. More importantly, they were directly critical in changing Indian policy on exports and export incentives through the influence of the Trade Development Authority staff and of Indian collaborators and critics. Its success in this respect contributed to the Indian Government's refusal to allow it to be published in its original form. (This, of course, is no excuse for the failure of some publishable output to be produced.) It was also a project in which developing country collaboration included the training of local researchers.

Small Scale Industries (RPO 670-77)

This is another project which, though admittedly not really well conceived, suffered from the start from strong opposition to its attempts

at innovation. It succumbed. However, in my opinion the view that "a 2-4 days meeting would have been preferable" (Bueno, para.10) is more representative of bias against such attempts by some members of the Bank's "research establishment" than of the facts of the case.

Market Penetration (RPO 671-66, 671-67 and 671-82)

This project (Jae-Ik Kim paras. 7 & 8) has evidently not been clearly perceived. No doubt because of the lack of aforementioned consultation time, we did not have an opportunity to explain its purpose; documentation was skimpy because the project was being formulated and defined even as the Panel was doing its work. We considered it essential to its success that while this Department would provide (and has provided) the guiding framework, the participants themselves had to be involved in its detailed formulation.

It is agreed that the industrial countries should be doing their own research in this area. Unfortunately the earlier project on market trends (RPO 670-20) did not succeed in creating a comprehensive framework of information and analysis in the industrial countries, hence this project and its purposes:

(a) To assist in the creation of the basis of an analytical data production and trade system for manufacturing (broadly defined) in the Bank that will embrace developing as well as industrial countries.

(b) To contribute to the analysis of the political economy of protectionism in industrial countries in terms that will not only assist policy formulation in these countries, but also contribute to the developing countries' export policy determination.

(c) To ensure sufficient attention to the importance of studying the adjustment process in the industrial countries, particularly in those such as Italy and Japan, which have hitherto not paid very much attention to it.

The project is now well launched in 12 industrial countries. It is creating quite a lot of interest as intended. We expect the data base to be established by the end of summer 1979, and analytical papers to start flowing in early 1980.

4. Finally, we have anticipated some of the Panel's conclusions. Together with the Development Economics Department, we are submitting a proposal for a limited research input that would enable us to establish the core industry component of the manufacturing-trade data system already mentioned. We propose

to use this data system within the Bank as the "anchor" for more specific project related data systems, and it will also be useful for more general analysis of the changing international structure of industrial production and trade related issues both within and outside the Bank. In-house we have begun work on the changing characteristics of trade flows, particularly among developing countries.

c.c. Messrs. Balassa
B.B. King
Westphal
Keesing

EPD Front Office and
Division Chiefs

HHughes/kg

Steering Group Documents

February 5, 1979

Second Interim Report of the Industry and Trade Research Steering Group*

1. The Industry and Trade Research Steering Group includes representatives from some of the major producers and consumers of Bank research in these areas. When formed in May 1978, it was given the following functions:[1/]

1. To consider and define research priorities in the industrial development/trade area for the next three to four years;

2. To make recommendations as to the operational application of Bank research on industry and trade;

3. To act as liaison with the External Panel on Research in Industrial Development and Trade.

This report summarizes the Group's conclusions concerning research priorities and the relation of research to operations. It is timed to precede the final round of discussion with members of the External Panel.

2. The Group's findings and recommendations are discussed below under four major headings:

a) Relation to Operations;

b) Topical Areas;

1/ Memorandum from Hollis B. Chenery and David L. Gordon dated May 26, 1978. The Group was also given the responsibility to advise the Research Committee as well as to provide liaison with the Industrial Development Coordinating Committee on matters concerned with research and its relation to operational needs.

* The Group consists of:

David L. Gordon, Director, IDF -- Chairman
Bela Balassa, DRC
Hans Fuchs, Director, IPD
Ravi Gulhati, Chief Economist, EAN
Frederick T. Moore, IDF
Richard W. Richardson, Director, CDD (IFC)
Bevan Waide, Chief Economist, ASA
Larry E. Westphal, DED

c) Level of Activity; and

d) Organization of Activity.

Relation to Operations

3. It is generally agreed that a great deal more could and should be done to increase the utility of research on industry and trade to operations. The problem does not relate only to RPO research; equally, if not more, important is the fact that relevant research done outside the Bank is not effectively utilized. Nor is enough being done to develop or exploit research capability in the Bank's client institutions (see below, para 29).

4. Although many research projects have been developed with operational objectives in mind and with considerable effort in dissemination, the bulk of research in the trade and industry area has aimed primarily at reaching academic standards and has mainly been disseminated through publication in journals or technical monographs. While operational applications have been limited, high standards of Bank research, along with the Bank's reputation for research quality, has enabled it to attract some of the world's best talents in areas where its research has concentrated. Adding to the attraction is the prospect of being able to do research on policy issues of operational consequence. The Bank's continuing ability to recruit and retain a research staff of high quality and motivation will depend in part on how the Bank manages its research activity.

5. Two additional points deserve mention. First, the Research Committee provides a mechanism for attempting to insure that research is directed toward appropriate ends. The mechanism has worked reasonably well, at least insofar as the Group does not consider that there has been any major misdirection of research in terms of the topics covered, although the

Committee has served primarily as a filter rather than a provider of guidance on research directions. Greater attention needs to be paid to achieving a proper balance among the mix of research activities, particularly a more effective application of research in operations. Second, notwithstanding the academic orientation of a major portion of their dissemination effort, researchers have sought in various ways to bring out the potential relevance of their research and to promote its utilization in Bank operations and in member countries' policy analyses.

6. In addition to circulation and discussion of research reports, a principal means of dissemination within the Bank has been through the involvement of researchers in operational missions; such involvement increasingly takes the form of a "demonstration effort", designed to provide an example of how to approach a particular problem or set of issues. This form of dissemination is hampered, however, by our having too few researchers to "spread around;" the demand for the specialized expertise embodied in the Bank's researchers far exceeds the supply, and there is no budget provision for research applications. Understandably, operational staff seeking help for a particular country or problem want more than reference to a past, present, or forthcoming demonstration effort done elsewhere. Episodic consultation by research staff has worked well in some cases, but has so far been practiced rather infrequently.

7. Research to academic standards requires long gestation periods for production of the initial output. There is often failure to communicate results in what operational staff consider to be an accessible form. Both problems may be traced in part to the way in which research is managed, which is to say that they are not inherent in the nature of much of the RPO research that is done. In many cases, research is preempted by other non-RPO

tasks given high priority in the short run. This interrupts the continuity and interferes with the timely completion of research. If more time and effort is to be devoted to dissemination within the Bank, either the research and related support staff (see below) must be expanded, current forms of publication will have to be given lesser priority, or the number of projects undertaken must be cut back.

8. The problem is not simply one of dissemination. Operational staff are frequently not as familiar as they should be, judged by their own criteria, with research products that are targeted specifically to their needs. Here too the problem is one of time pressure in the face of multiple tasks. Moreover, the absence of feedback from operational to research staff impedes progress in designing efficient means of communication and eventual assimilation.

9. Effective assimilation of research into operations requires more than the preparation of communicative reports of findings. Much of the case study research focused on policy issues could usefully be replicated in areas outside the original sample, in the context of operational work; this may call for design of short-cut procedures and would be greatly facilitated by the preparation of "manuals" to expound various aspects of appropriate methodology.

10. In the current situation, operational staff do not have the time to replicate policy-focused case study research; or to apply quantitative methods developed through research; or even to identify issues on which probing analysis is both required and feasible, or -- given the issue -- to select the proper mode of analysis and supervise its implementation. Under existing staff constraints, most of the deeper analysis that should be done would have to be carried out either by non-operational staff or by consultants.

Moreover, operational staff are often not well informed regarding studies of specific policy issues that would be highly relevant to operations in particular countries.

11. Operational staff naturally have a strong desire that research staff do more to increase the assimilation of world-wide research and the utilization of external research resources in the Bank's country economic and sector work. Given staff constraints this objective competes with direct research efforts. The Group agrees that a substantial increase is warranted in the resources devoted to assimilation and utilization. But if this is not to be at the expense of direct research, it will require considerable expansion of the staff working on industry and trade.

12. Individual RPO research undertakings fall along a spectrum, in terms of their distance from potential operational application. But closeness to such application is not a wholly valid criterion for research project selection. Operational application may be circuitous, the Bank using work done elsewhere that was prompted, made possible or reinforced through original research by the Bank. Moreover, not all issues of fundamental concern are amenable to research at a short distance from operational application; research to improve methodologies and to test conventional assumptions about underlying phenomena is considered furthest from operational application, but may have a high long-term payoff.

13. Finally, it must be recognized that RPO research is but one of a number of related activities that support operations through one means or another. The most notable example of other support activities felt to be greatly beneficial to operations is information gathering, synthesis, and reporting in the areas of trade in manufactures, keeping tabs on a range of specific industries, and (now being initiated on a systematic, world-wide

basis) the compilation and analysis of data on DFC subprojects. In relation to the Bank's own operational needs, insufficient resources are allocated to this type of activity.

Topical Areas

14. The Group has reviewed past research and priorities for future research according to the topical headings used by the External Panel. Memoranda prepared for discussion are available, as is the summary contained in the Group's interim report for discussion with the External Panel. A somewhat different set of topical headings is employed in this report.

15. The Group does not feel it should try to identify individual, high priority RPO projects for the future -- which generally require considerable further work to define them satisfactorily -- so that this report deals primarily with emphasis or allocation among topical areas. The Bank's understanding of the issues under the various headings has progressively evolved. Detailed priorities are clearest in those areas where work has progressed the farthest, but priorities among broad areas tend to favor those in which the work has progressed least (if at all). The background documents referred to above may be consulted for more specific discussion of some project priorities. By the same token, there is a critical need to crystalize researchable projects in other areas.

16. The following discussion consists of a listing of topical areas, with a brief description of the contents of each and an indication of priority for future work. A summary is provided at the end.

17. Industrial Organization/Management: The Bank has only recently initiated substantial research under this heading. Moreover, out of a vast field, only two sub-topics are being addressed at present.

(a) Small Scale Enterprises: A major effort is underway to obtain an analytically-focused description of small scale enterprises, their dynamics and relationships with other industrial or financial entities, in a number of countries. The project is seriously

understaffed, especially in view of demand for the participating researchers' involvement in operational missions. There is need for further research to a rigorous professional standard, particularly on the relationship of small/medium with large enterprises.

(b) Public Enterprises. Problems of public enterprise performance are widespread and serious, a continual source of concern in many areas of Bank operations. It is not easy, however, to define research topics that would yield results of general application. A modest project is in progress, focused on managerial efficiency. As a prelude to any new research initiatives in this field -- apart from the continuing attention it received in country economic, sector and project work -- it would be desirable for the Bank to review in ~~some~~ More depth the research going on elsewhere; and perhaps perhaps to convene a panel, including outside specialists, to identify specific needs and comparative advantage for research by the Bank.

18. Industrial Technology: There has for some time been a small but stable commitment of research resources to technological issues. The smallness of the research staff has impeded interaction with operational staff, to the detriment of the dialogue essential for effective dissemination. Steps have been taken to rectify the situation, but the problem of insufficient manpower remains.

(a) Capacity Utilization: No further research per se is proposed in this field. However, the results of past Bank and outside research are not well known to operational staff. There is a need to produce one or several reports summarizing past research in terms that will maximize its potential usefulness in the conduct of country economic and sector work.

(b) Technology Policy: In addition to several intensive studies of technological choice in a few selected industrial activities, a review of worldwide research has been undertaken to determine policy implications, especially as regards employment. While identification of priorities for further research awaits final discussion of this review among Bank staff, it seems likely that new research should emphasize the means of supporting institutions (especially those solidly based within developing countries) to acquire access to appropriate technologies and to strengthen the indigenous technological base. In any event, top priority in this area attaches to strengthening the dialogue between research and operational staff.

(c) Technological Change: The Bank has not sponsored research in this area, though some operational work aims at fostering technological change. The design of appropriate topics for research that could eventually guide Bank lending in this field, deserves priority attention.

19. Industrial Support Institutions: To date, there has been little research within the Bank on the design of supporting institutions which provide credit, technical assistance, and the like; until recently the focus of most of this work has been on credit.

(a) Industrial Financing: Much of the Bank's work on credit markets is relevant, although not focused on industry per se. Research on the role of informal credit institutions has recently begun. Likewise research on transaction costs for industrial financing of different categories, and simplified credit scoring procedures. More work on credit institutions, criteria and procedures, and their results, will undoubtedly be indicated.

(b) Technical Assistance and Advisory Services: A study has been started to examine the functioning of the Technical Consultancy Organizations established in India by financial institutions. On the other hand, a proposal to study the need for a "Technology Referral Service" was recently turned down by the Research Committee. The SSE research project (17(a) above), which will also throw light on access to and choice of technology (18(b)) should help to define the role of industrial extension services and the needs for further research in this area.

(c) Entrepreneurial Development: The Bank has undertaken no studies on this subject, but clearly it is an important factor in the industrialization process. Here too (as with 17(b) above) it appears that a review of work done elsewhere is needed to determine what is the proper role for Bank research.

20. Trade in Manufactures: The Bank has an extensive body of work under this heading, including both formal research and informal collecting, synthesis, and reporting. Continuation of such work deserves priority, to take advantage of the analytical base established and to provide important information to the operating staff.

(a) Trend Reporting: There is a clear need to allocate staff on a regular basis to monitor recent and past trends, and to make projections, owing to the absence of adequate up-to-date information from other sources. There is equally a need to evaluate medium-run trends among industries and across countries. To the extent possible, continued efforts should be made to document trends in trade in relation to those in capacity and production on a global basis. Further work in this area needs to be closely

coordinated with that on industry studies (para 23 below), on which it must rely heavily for detailed analysis of individual industries.

(b) Market Access: Several major projects are in progress, while a welcome effort is being taken to establish up-to-date monitoring capability. Pending results, no further initiatives seem to be needed except the work implicit in the conduct of industry studies.

(c) Institutions/Marketing: Research has recently been started on the institutional circumstances that facilitate manufactured exports. Further work may be warranted, depending upon the outcome of this research.

(d) Inter-LDC Trade: There are several possible areas of research on economic integration, among which the experience of developing countries with product specialization areas is the most promising. Greater interest attaches, however, to the future prospects for trade among developing countries in general that would take place in response to market incentives. Research in this area may be carried out in the framework of a projected study on world trade in manufactures to be considered below.

21. Industrial Strategy/Policy: This has been the subject of most of the Bank's past research on industry and trade. As regards the role of trade and trade policies, further research does not appear to be urgent. Rather, priority should be placed on making use of past research in country economic and sector work -- which entails further case study replication, using short-cut methods when appropriate. There is, however, need for research on other aspects of industrial strategy and overall policy, in particular

those relating to employment and labor markets as well as technology.

(a) Incentive Policies/Domestic Resource Costs: This is doubtless the best researched of all sub-topics in the Bank's portfolio. However, the work needs to be extended to cover countries rich in natural resources, and those just beginning the process of industrialization. These and other extensions, and especially further application of the research, can now best take place in the context of country economic and sector work.

(b) Export Promotion: The on-going project concerned with export incentives and the welfare effects of export promotion may be considered to fall under the previous sub-topic; and there should be continued application through country economic and sector work.

(c) Employment and Labor Markets: Insufficient attention has been given to the operation of labor markets in developing countries. Work on small scale enterprises represents a very modest start toward understanding of possible employment enhancement. Attention should further be given to the existence of labor market distortions, in particular those caused by government action, and their effects on employment opportunities.

22. Comparative Advantage, Patterns of Industrialization and Trade, Economic Growth: Work is nearing completion on two projects which provide information regarding patterns of industrial growth and structural change. Apart from the updating that is implicit in trend reporting, no further research appears warranted to follow up these projects. A more ambitious project involving general equilibrium modeling was recently initiated; the

scheduled review of progress at mid-term will provide an opportunity to assess its promise more concretely. Finally, there is need to examine the factors determining comparative advantage, with a view to analysing prospective changes in the structure of world trade in manufactured goods.

23. Industrial Programming: There has been only one RPO concerned with methodologies for project selection specifically in the industrial sectors.

(a) Project Programming: A long standing RPO has refined the use of mixed integer programming to analyze alternatives as regards location, scale, timing and design of inter-connected projects within individual sub-sectors. Application is furthest advanced for fertilizers. The methodology appears to have proven relevance for sub-sectoral analysis, and it is time that provisions be made for application within as well as outside the Bank.

(b) Project Appraisal Criteria: While various limitations in the use of conventional criteria are recognized and criticized, proposals for further research into methods of project identification, design, and/or appraisal have not been forthcoming. Lacking specific operational staff proposals for work in this area, the Group, while accepting its potential importance, is inclined to postpone its consideration.

24. Industry Studies: Industry studies to keep up-to-date on developments in particular sub-sectors constitute an on-going activity of the Industrial Projects Department. Coverage is greatest and documentation for general staff use is most extensive for fertilizer. Other units have worked episodically on a few selected industries, including steel, textiles, clothing,

electronics, and machinery. Policy advice in regard to specific sub-sectors, not to mention project work, requires continuing analysis of changes in technologies, cost, market and business strategies. Additional resources are needed to put industry studies on a more regular basis, with more documentation for general use. Extension of the work on basic intermediate products, such as steel, chemicals and pulp and paper, deserves high priority. There is a wide consensus also that the engineering industries deserve a strong research effort.

25. Country Studies: Several in-depth analyses of particular issues concerned with industrialization, extending and intensifying normal country economic and sector work, have been undertaken, a few financed by the Research Committee. Intensive country studies focusing on industrial sector conditions and issues do not always require Research Committee funding, but they do require resources -- particularly Bank staff, at least to identify issues and oversee and/or absorb the studies.

Level of Activity

26. The appropriate level of resources to be allocated to research needs to be viewed in the context of all activity that supports country economic, sector and project work in one way or another. It cannot be assessed in the absence of a Bank strategy for the continual improvement of the informational and analytical basis of its operations. This strategy must, on the one hand, consider how the Bank can make use of worldwide research; and, on the other, take account of the Bank's role in assisting its member countries to improve the basis for policy formulation and project implementation.

27. The Group cannot undertake to pose and evaluate alternative strategies with respect to the role of the Bank vis-a-vis the efforts of its member countries. It simply assumes a modest increase in Bank resources devoted to undertaking studies, in the context of country economic, sector and project work, to improve the basis for policy formulation and project implementation in specific member countries. It urges, moreover, that opportunities be explored to enlist and enhance the research interest, capability and resources that exist in a number of the Bank's more sophisticated DFC and other clients, by helping them to design and carry out studies of high professional standard and relevance for industry and trade policy.

28. The Group is most concerned with what can be done to improve the informational and analytical basis for the Bank's own operations. Over the past decade, the Bank has successfully established a staff that does research, but too few resources have been devoted to achieving the effective use of research (whether Bank or otherwise) in operations. Owing to the pressures on operational staff, most of the burden of promoting the use of research has fallen on the research staff. The research staff working on industry and trade

is too small simultaneously to do research of high academic quality, to engage in other support activities including the a-plication of research, and to promote access to and use of world wide research in these areas; consequently, additional staff to assist in the latter two functions are required.

29. It is the view of the Group that highest priority should be placed on providing access to and facilitating use of research--which might entail a sharp reduction in research to an academic standard if the current level of central staff (and complementary) resources cannot be increased. However, the Group recommends that resources should be increased, to permit continued high standard research in specific areas of primary concern to the Bank's operations. This recommendation is based on the Bank's strong comparative advantage in this kind of research, which derives ultimately from the physical proximity of researchers to operational staff and the interchanges of information and ideas that are thereby promoted.

Conclusions

30. The Group has recommendations relating to particular areas in the industry and trade field. These recommendations are summarized in Table 1 where an indication of the staffing implications of the recommendations for central staff (DPS and CPS) is also provided. Furthermore, Table 2 shows the size of existing and proposed staff for research-related activities in central units.

31. The recommendations would entail maintaining the size of existing professional staff in research on industry and trade, with a reallocation taking place among the individual areas. The Group does not suggest that any ongoing research be abandoned abruptly. Indeed, it assigns highest priority to completing research now underway, so that the lessons it has to offer may be learned and put into practice. Correspondingly, the pace at which the

shift in focus can be achieved depends upon the rate at which existing research is completed.

32. At the same time, increases are recommended in the size of central professional staff that is engaged in research-related activities in support of Bank operations. All in all the total number of professional staff engaged in research-related activities on industry and trade and located in central units would rise from 20 to 30.

33. Under the recommendations made, research-related activities would also be undertaken by the Regional Offices. Such work might relate to any of the areas of industry and trade considered above. As the needs of the individual regions vary, the staffing requirements are indicated only for the industry and trade area as a whole. Providing the resources are actually earmarked for these activities, two professional staff members per region might be appropriate.

34. The Group urges that greater use be made of special panels (with external representation) in determining priorities and defining research topics within the broad categories listed earlier. Experience suggests that researchers have an understandable tendency to underestimate the feasibility and potential benefits of research in areas of which they have little specialized knowledge. Thus the recruitment of researchers having backgrounds new to the Bank has stimulated the extension of research into new areas. For areas about which there is uncertainty as to the need or appropriateness of Bank research, special panels could objectively assess whether the Bank would have a comparative advantage; on the other hand, where the need is clear, panels can help to define detailed priorities.

35. Finally, the Group also urges that, where appropriate, research be more directly related to country economic, sector, and project work. At a minimum, efforts should be made to distill major findings from country studies

Table 1

Summary of Recommendations

Topical Area	Principal Recommendation	Staffing Implications
A. Industrial Organization/Management		
Small Scale Enterprises	Expand research, operational support	Increase present staff
Public Enterprises	Consider need for additional Bank research	None - only one staff at present
B. Industrial Technology		
Capacity Utilization	Targeted report for operational staff	Use consultant of YP
Choice of Technology	Awaits outcome of current discussions	None in short run
Technological Change	Prepare research program	Requires additional staff
C. Industrial Support Institutions		
Industrial Financing	Consider need for additional Bank research	)
Extension Services	- same as above -	) Likely to require increase in present staff
Entrepreneurial Development	Evaluate need for Bank research	)
D. Trade in Manufactures		
Trend Reporting	Expand present program, put on regular basis	Increase present staff
Market Access	Await completion of on-going research	None in short run
Institutions/Marketing	- same as above -	- same as above -
q Inter-LDC Trends	Consider need for additional Bank research	- same as above -
E. Industrial Strategy/Policy		
Incentive Policies/Domestic Resource Costs	) Shift to country studies	) Change staff functions; provide additional
Export Promotion	)	) staff for applications as needed
Employment/Labor Markets/Technology	Evaluate relevance of existing research	Change staff orientation
F. Comparative Advantage, Industrial Structure	Institute research on comparative advantage	Reallocation of staff
G. Industrial Programming		
Project Programming	Determine need for Project Programming Unit	Unit would require additional staff
Project Appraisal Criteria	No research foreseen at present	None
H. Industry Studies	Expand, put on a more regular basis	Increase present staff
I. Country Studies	Support Regional Offices' Initiatives	Requires additonal staff

Table 2

Professional Staff Needs in Central Units[1/]
(professional man-years)

	Existing Staff			Proposed Staff		
	Research	Other	Together	Research	Other	Together
Industrial Organization/Management (incl. small scale enterprise)	2.5	1.0	3.5	4.0	2.0	6.0
*Industrial Technology (incl. technological change)	1.0	-	1.0	2.0	1.0	3.0
*Industrial Support Institutions	0.5	0.5	1.0	2.0	1.0	3.0
Trade in Manufactures	0.5	3.5	4.0	1.0	4.0	5.0
Industrial Strategy/Policy	2.0	1.0	3.0	2.0	3.0	5.0
Comparative Advantage, Pattern of Industry	3.5	-	3.5	1.0	-	1.0
*Industrial Programming	0.5	0.5	1.0	-	2.0	2.0
Industry Studies	-	3.0	3.0	-	5.0	5.0
TOTAL	10.5	9.5	20.0	12.0	18.0	30.0

1/ Exclusive of regional allocations; country studies are therefore omitted. For details see Annex Table 2.

under particular topical areas and according to a useful typology. As regards project work, closer links may take any number of forms: for example, research ideas may evolve out of selective involvement by researchers in the project cycle, or research may be directed toward learning from past project experience through in-depth ex post evaluation. In the past, the research staff has tended to relate more to country economic and sector work than to project work; the slow trend toward greater balance in this respect could usefully be accelerated.

36. Additional staff in the amount of the greater number shown in Table 2 would permit approximate doubling of existing work in each of the areas except those that are starred, where the increment would be far greater in percentage terms. In all areas, additional staff would be required to put existing work on a regular, sustained basis. At present, a critical mass of research and related support staff resources is lacking in all these areas.

37. The recommended increment to staff working on industry and trade should be allocated to fill the gap between research and its use. Continuing research to an academic standard would then be roughly unchanged in staff resources assigned, while its concentration by topical area would shift over time in accord with the priorities outlined earlier.

Organization of Activity

38. The final report of the Group will contain recommendations on the organization of responsibility for the activity. At this point, the Group has not reached final agreement on the recommendations.

Annex

Allocation of Resources Devoted to Research-Type Activity in the Areas of Industry and Trade

This annex provides information regarding the magnitude and composition of Bank resources allocated to research-type activities in the areas of industry and trade in manufactures. The first section discusses the allocation of Research Committee funds; the second, the allocation of professional staff. As regards staff time, separate data are given for RPO research and for other activities that are not directly operational in the sense of being linked to either country economic and sector or project work, but which add to the informational and analytical base of Bank operations.

Research Committee Funds

Research Committee funds pay for consultants and "temporary" research assistants as well as for many other categories of expenditures, such as the cost of undertaking surveys, computer charges, and non-Bank secretarial assistance. To avoid mixing apples and oranges, it would be desirable to separate expenditures from Research Committee funds into at least three categories: consultants and research assistants, each in man-week terms, and other, in money terms. Among other things, this would permit an assessment of the extent to which Bank staff are supplemented by the use of outside researchers. Unfortunately, a breakdown along these lines could not be obtained, owing to the time it would take to search through the individual project files.

Annex Table 1

Allocation of Research Committee Funds[1/]
(In percent)

		Pre-FY78	FY78 on	Total
A.	Industrial Organization and Management	-	11.5	11.5
	Small Scale Enterprises	-	8.2	8.2
	Public Enterprises	-	3.2	3.2
B.	Industrial Technology	10.5	4.5	15.0
	Capacity Utilization	3.5	-	3.5
	Choice of Technology	7.0	4.5	11.5
	Technological Change	-	-	-
C.	Industrial Support Institutions	1.1	3.5	4.6
	Industrial Financing	1.1	3.5	4.6
	Extension Services	-	-	-
	Enterpreneurial Development	-	-	-
D.	Trade in Manufactures	3.8	28.4	32.2
	Trend Reporting	-	-	-
	Market Access	2.3	25.3	27.6
	Institutions/Marketing	-	3.1	3.1
	Economic Integration	-	-	-
	Other[2/]	1.5	-	1.5
E.	Industrial Strategy/Policy	16.7	8.2	24.9
	Incentive Policies/Domestic Resource	9.2	.3	9.5
	Export Promotion	2.4	3.7	6.1
	Comparative Advantage	5.1	4.2	9.3
	Employment/Labor Market/Technology	-	-	-
F.	Industrial Programming	6.6	1.2	7.8
	Project Programming	6.6	1.2	7.8
	Project Appraisal Criteria	-	-	-
G.	Industry Studies	-	-	-
H.	Country Studies	-	3.9	3.9
	Total	38.7	61.3	100.0

Source: Annual Reports to the Executive Directors on the World Bank Research Program.

1/ Based on figures in current dollars.

2/ RPOs 670-07 (International Model) and 670-19 (Expansion in Manufacturing for Exports in Developing Countries).

The Research Committee has alloted a total of $3.030 million to research on industry and trade. Annex Table 1 shows the allocation of this total among topical areas, with no distinction being made among types of expenditure.[1/] The data are in current rather than constant dollars. Expenditures are, however, shown separately for pre-FY78 and for FY78 and beyond, to permit an assessment of the shift over time among major topical areas.[2/]

The data in the table reveal a marked shift over time in the composition of Research Committee funding. A pronounced fall in the shares devoted to industrial technology, industrial/policy, and industrial programming is being offset by a rise in the shares going to industrial organization and management, industrial support institutions, and trade in manufactures. Considering all Research Committee approved projects, one finds that roughly one third of the total funding has been allocated to trade in manufactures and one quarter to industrial strategy/policy. Industrial technology (15 percent), industrial organization and management (11 percent), and industrial programming (8 percent) account for the bulk of the remainder.

1/ Included are only those RPOs that fall under the purview of the External Panel on Research in Industrial Development and Trade.

2/ For FY79 and beyond, the figures pertain to budget allocations rather than actual expenditures.

Professional Staff Time

A total of approximately 820 professional manweeks per annum is currently being devoted to RPO and other research-type activity in the areas of industry and trade.[1,2/] As shown in Annex Table 2, RPO research accounts for slightly more than one half of this total. The remainder is spent on other research-type activities, as discussed further below.

RPO Research: Over half of the total professional staff time going into RPO research is focused on industrial strategy and policy. Another quarter is allocated to work on industrial organization and management. Industrial technology (9.3 percent), industrial programming (5.7 percent), trade in manufactures (3.5 percent), and industrial support institutions (3.1 percent) account for the remainder.

The allocation among topical areas of professional staff is not the same as that of Research Committee funds (compare Annex Tables 1 and 2). There are two principal reasons for this. First, staff time continues to be put into projects well after the completion of most (sometimes all) expenditures financed by Research Committee funds, owing to lags in the preparation of draft reports and publications. Second, there is greater reliance on non-Bank researchers in some areas than in others. The use of consultants to substitute for Bank staff is particularly great in RPO research on trade in manufactures and country studies.

1/ It was not possible within the time available to obtain staff time estimates for the period preceding FY78.

2/ This includes only direct time. Thus, to convert to approximate full-time equivalent manyears, divide by 42.

Annex Table 2

Allocation of Professional Staff to Research-Type Activity
Average for FY78/9: Manweeks

	RPO Research 1/	Other Research Activities 2/	Total
A. Industrial Organization/Management	12.3	5.2	17.5
1. Small Scale Enterprises	11.3	.8	12.1
2. Public Enterprises	1.0	3.9	4.9
3. Other	-	.5	.5
B. Industrial Technology	4.8	1.7	6.5
1. Capacity Utilization	.4	-	.4
2. Choice of Technology	4.4	.5	4.9
3. Technological Change	-	1.2	1.2
C. Industrial Support Institutions	1.6	3.3	4.9
1. Industrial Financing	1.6	1.5	3.1
2. Extension Services	-	1.8	1.8
3. Entrepreneurial Development	-	-	-
D. Trade in Manufactures	1.8	15.9	17.7
1. Trend Reporting	-	11.7	11.7
2. Market Access	.1	4.2	4.3
3. Institutions/Marketing	1.7	-	1.7
4. Economic Integration	-	-	-
E. Industrial Strategy/Policy	27.7	5.6	33.3
1. Incentive Policies/DRCs	4.5	.3	4.8
2. Export Promotion	6.8	.5	7.3
3. Comparative Advantage, Etc.	16.4	.9	17.3
4. Technology	-	3.9	3.9
F. Industrial Programming	2.9	.8	3.7
1. Project Programming	2.9	.2	3.1
2. Project Appraisal Criteria	-	.6	.6
G. Industry Studies	-	16.4	16.4
H. Country Studies	-	-	-
Total	51.1	48.9	100.0

Sources: RPO Research -- Time Reporting System.
Non-RPO -- Information provided by individual units.

1/ Includes only RPO's that fall under the purview of the External Panel on Research in Industrial Development and Trade.

2/ Includes some RPO research; see accompanying notes.

Other Research Activities: Regardless of the perspective chosen, RPO research is not the only means whereby Bank activities add to the stocks of knowledge and tools of analysis. Determining where to draw the line between operations and research is, however, not easy. Moreover, the broader one's perspective, the more difficult it becomes to assemble data in meaningful breakdowns. With this difficulty specifically in mind, it was decided to exclude all activities of the Regional units, and to concentrate only on the Central units. Correspondingly, work undertaken by the Regional offices on country studies is omitted, whether in the context of country economic or sector analyses. Equally, involvement in Regional work by the Central staffs is excluded, even though it is complementary to the latter's research activity.

Activities that add to the informational and analytical base of Bank operations need not necessarily result in reports written with a more general audience in mind. For example, the Industrial Projects Department (IPD) is continually engaged in industry studies, through information gathering undertaken in connection with project work. But only for a few selected industries (notably fertilizer) are reports prepared that provide ready access to this information by non-IPD staff. Equally, there is a great deal of learning from past project experience that is nowhere recorded in readily accessible form. However, for our purposes, activities are considered to improve the information and analytical base of Bank operations only if they result in written output (not excluding memoranda) that are disseminated for use outside the originating Department.[1/]

1/ Note that the definition of non-RPO research-type activities used here is somewhat broader than that used to identify "Departmental Studies" in the annual reports to the Executive Directors on the World Bank Research Program.

The "Notes to Annex Tables 2 and 3," which appear at the end of this Annex, identify the activities included as non-RPO, or other, research activities. The professional staff time put into these activities is nearly equal to that put into RPO research, but its composition differs considerably. In particular, non-RPO research is more heavily concentrated on industrial support institutions, trade in manufactures, and industry studies. The last two categories each account individually for about one third of the total professional staff time spent on non-RPO research, with industrial strategy and policy (11.5 percent), industrial organization and management (10.6 percent), and industrial support institutions (6.7 percent) constituting the bulk of the remainder.

Location of Activity: Annex Table 3 shows the proportion of total professional staff time accounted for by the various organizational units. As it clearly demonstrates, major involvement in research-type activity is found in several units. The Economics of Industry Division, in the Development Economics Department (DED), contributes slightly more than one third of the total; its activity ranges across most of the topical areas shown in the previous tables. Other major contributors include: the Economic Analysis and Projections Department, largely concentrated on trade in manufactures; the Employment and Rural Development Division (DED), reflecting work on employment and labor markets; the Industrial Projects Department, focused on industry studies; the Public Finance Division (DED), concerned with public enterprises and industrial support institutions; and the Development Research Center, an important locus of work on industrial strategy and policy.

Annex Table 3

Staffing of Research-Type Activity
Average for FY78/9: In Percent

Development Policy Staff	
Development Economics Department	
Economics of Industry	36.1
Employment & Rural Development	11.7
Public Finance	8.6
Economic Analysis and Projections Department	19.6
Development Research Center	7.9
Central Projects Staff	
Industrial Development & Finance Department	4.5
Industrial Projects Department	10.4
Project Advisory Staff	
Science & Technology Advisor	*
Other	
International Finance Corp.	1.2
Total	100.0

* Denotes involvement based largely on consultant inputs.

Caveat

The extent of activity within the Bank which adds to the informational and analytical base of Bank operations in the industrial area is obviously broader than that reflected in the estimates presented here. Our estimates exclude research assistant and consultant inputs, which play an important role in several areas. For example, consultants employed by the Science and Technology Advisor have done a major share of the work on industrial technology policy within the Bank. Equally, our estimates exclude work focused on individual countries as well as the much of that which aims to learn from past project experience. In regard to the latter, for example, post-evaluation undertaken by the Operations Evaluation Department is omitted.

We are nonetheless confident that our estimates reflect reasonably well the extent of resources allocated to providing information usable (and used) across a wide variety of operational needs. By and large, the activities that are excluded result in information that is either obtained primarily for use in the originating unit or is not effectively targeted to a broader set of interests. In turn, we are equally confident as regards the indicated concentration among topical areas.

Notes to Annex Tables 2 and 3

Provided below are shorthand descriptions of the principal activities included as non-RPO research-type activities in Annex Tables 2 and 3. (Note: time spent on inputs to the FY79 World Development Report is not included except in the case of activities that would be on-going in any event.)

Economics Department (Economics of) Industry Division

B3. Research project generation

D1 & D2. Non-RPO information collecting, synthesis, and reporting. Work on trade among less developed countries.

E4. Supervision of papers on employment by J. Stern for Urban Poverty Force.

G. Trade focused studies of textiles, clothing, footware, electronics, machinery industries.

Economics Department, Employment (& Rural Development) Division

A3. Work on rural industrialization.

E4. RPOs not identified as falling under Industry and Trade Research, but having relevance to questions of industrial strategy, especially as regards employment; rough estimate to reflect only that share of total time spent on industry.

Economics Department (Public) Finance Division

A1. Research assistant working on RPO 671-59, financial aspects; several small miscellaneous tasks.

B2. Work on choice of technology by public sector.

C2. In-house study -- financial structure and technology policy.

(Economic) Analysis and Projections Department

B1. Continued work by H. Hughes; monograph forthcoming.

D1. Trade data system and related statistical reporting, analysis. Work on trade among less developed countries.

D2. Monitoring of protectionist policies in developed countries; supervision of related RPOs.

Notes - Continued

E. Analysis of primary commodity processing in LDCs; work on trade among less developed countries; joint IBRD-UNIDO monograph on Industrial Development Policy.

G. Statistical reporting on prices of manufactures.

Development Research Center

D (all). Continued work by B. Balassa.

F2. Work on various aspects by G. Pursell

Industrial Projects Department

A2. Continued work by B. Walstedt.

B2. Supervision (joint with DED Industry Division) of RPO 671-77.

G. Studies of fertilizer, pulp & paper, steel mechanical engineering and other industries.

Industrial Development and Finance Department

A1. Criteria and project identification guidelines for small scale enterprises lending; other work on small scale enterprises, including study of sub-contracting, government procurement, choice of technique in Bank sub-loans, etc.

A3. Subproject data system.

B1. Continued work by F. Moore.

C2. Work on export zones, construction industry, RD&E institutions.

E. Sector paper on employment and small scale enterprises; industrial policy paper (forthcoming); interest rate issues; involvement on Steering Group.

F1. Supporting inputs to operational extensions of RPO 670-24.

G. Studies of mechanical engineering, petroleum, and electronics industries.

Project Advisory Staff

E5. Work on various aspects of science and technology policy related to industry, mostly by consultants.

Industrial Finance Corporation

B2. In-house studies of technology choice, including that by M. Amsalem.

Interim Report of the Industry and Trade Research Steering Group

A. Introduction

1. This memorandum attempts to summarize discussions of the Industry and Trade Research Steering Group over the past three months, on the major blocks of subject matter comprising Bank research in these areas (schedule of meetings in Annex I). For each of these topical blocks, one or more memoranda were prepared as a basis for discussion; these are also attached for reference (Annex II), along with a more general initial memo by the Chairman of the Steering Group, dated July 13, 1978 (Annex III).

2. A matter of general concern is the basic orientation of the Bank's research on industry and trade. The research objectives cited in the terms of reference for the External Panel include, inter alia, "to support all aspects of Bank operations . . ." and to "broaden our understanding of the development process." To meet these objectives the research program will necessarily involve a mix of work on innovative methodologies with work of immediate relevance to specific operational problems. The time required to develop new methodological approaches and to perceive operational "payoff" may be lengthy. Ultimately, however, all such work should be related to and of value for policy and operational purposes, and in areas where the Bank has some comparative advantages over academia.

3. This relationship needs to be continuously reviewed, its definition refined, in order to avoid a widening gap between researchers and their work on the one hand and the practitioners in the Bank's project and industrial policy work on the other, and worsening of the already serious problem of dissemination/assimilation. At present, the staff of both the research units and those engaged in operations are fully committed to ongoing programmed activity. There is virtually no leeway to permit the researchers to communicate in systematic fashion the results and potential applications of their studies, nor for the practitioners to receive and digest any such information or to formulate their needs for further research in a way that would enable the researchers to try to meet these needs. Opportunities for operational applications and feedback are thus probably wasted in substantial part, or exploited only sporadically.

4. It would be highly desirable also for the research staff to work closely with the operational and sector departments and have a participatory role in the Bank's industry sector work and policy dialogue. Staff constraints, however, also severely limit such participation; and even so limited, it entails a substantial cost to their primary function.

B. Incentives and Domestic Resource Costs; Economic Integration in Developing Countries; and Export Promotion and Market Access

5. Incentives and Domestic Resource Costs -

(a) Following the publication of the book The Structure of Protection

in Developing Countries, the research project 670-01 "Development Strategies in Semi-Industrial Countries" analyzed relative incentives provided to exports and import substitution in countries following different development strategies. The findings, reported in several working papers and in a volume, have influenced the orientation of Bank policy analysis and, in some instances, policy-making in the countries studied.

(b) 671-87 "Industrial Policies and Economic Integration in Western Africa" provides detailed estimates on incentives and domestic resource costs in industrial and agricultural activities in four Western African countries. The research project has led to the preparation of a tariff and tax reform in Senegal and an investment project in Mali; also, the methodology employed has found several applications in the Bank.

(c) While future research on semi-industrial countries is likely to bring diminishing returns, demands for country applications of the effective protection-domestic resource cost methodology should be met. Furthermore, it would be desirable to undertake research on industrial strategies in non-industrial and natural resource-rich countries (on this point see also Paras 23 and 24 below). Research on an efficient import substitution export promotion sequence and on adjustment problems involved in policy changes may also be desirable.

6. Economic Integration in Developing Countries

(a) Apart from 671-87 and a few general papers on the subject, research on economic integration concentrated on the practical application of the project approach. This has been done in the Fertilizer Study for East Africa, carried out in the framework of 670-24 "Programming in the Manufacturing Sector", and in subsequent applications on regional specialization in fertilizer in ASEAN and in the Andean Common Market.

(b) Several possible areas of research on economic integration in developing countries have been identified, including experience with integration through trade liberalization and product specialization schemes, future possibilities for economic integration, and the benefits and costs of inter-regional trade in the framework of preferential agreements.

7. Export Promotion and Market Access

(a) Following the research project 671-10 "Promotion of Nontraditional Exports", work has been undertaken on the extent of export incentives and the social profitability of exports (671-35 "Export Incentives in Developing Countries"), on institutional and marketing problems involved in exporting various consumer goods (671-68 "Key Institutions and Expansion of Manufactured Exports"), and on market penetration in developed countries by the exports of developing countries (671-67 "Effects of Increased Import of Manufactured Goods from Developing Countries," the manufacturing part of 671-66 on Western Europe, and 671-87, covering Australia, Canada and Japan.

(b) Research results in this area have been utilized to prepare background papers for World Development Reports 1978 and 1979. At the same time,

the staff requirements for preparation of these papers have adversely affected the work on trade in electronics and electrical machinery, textiles, and clothing. This work is of considerable importance, however, and should be continued and possibly extended to other industries. Also, it would be desirable to extend the study of the institutional and marketing aspects of export promotion to capital goods.

(c) Work on the future demand and supply situation in intermediate products, such as steel, fertilizer, and petrochemicals, would also be useful, as it would help to indicate future possibilities for LDC exports in this area. More generally, it would be desirable to examine future changes in the comparative advantage of the developing countries.

C. Comparative Advantage; patterns of Industrialization and Trade; Economic Growth

8. Work is nearing completion on two projects -- Patterns of Industrial Development (RPO 671-05) and A Comparative Study of the Sources of Industrial Growth and Structural Change (RPO 671-32). These projects provide useful information regarding systematic patterns in industrial growth among two-digit industrial sectors; they offer an analytical perspective that could be more generally utilized in sector mission work. Unfortunately, only the latter project, dealing with fewer countries but in greater detail, deals with employment patterns.

9. A third project -- "The Sources of Growth and Productivity Change: A Comparative Analysis" (RPO 671-79) -- is far more ambitious, as it involves a general equilibrium analysis of trade-offs among policies with respect to their effects on growth, distribution, etc. In anticipation of the research, the approach has been tested in country economic work on Turkey, where the analysis of current policy options was favorably received. The research consists of ex-post analysis of policy changes that affect industrial structure; it will cover Korea, Turkey, and possibly a third country. The analytical difficulties are formidable, due to data problems and the complexity of the models.

10. Owing to its experimental nature, the third project has been funded for only the first year, after which progress will be reviewed and a decision made regarding further allocations. In turn, as part of the same project, a proposal will be forthcoming for parallel analysis in greater depth of two industrial sectors in each country. This is intended to permit a deeper analysis of the sources of productivity change. Further initiatives in this general area must await the completion of on-going work and evaluation of its results.

D. Employment Enhancement and Industrial Development -- Issues and Institutions; Small Scale Industry, and Interaction with Large

10. This topic clearly is important in the context of the Bank's concern

for alleviating urban and rural poverty and its emphasis on creating not just more jobs but more productive (i.e., higher paying) jobs. Papers by J. Stern made a beginning on this problem, and highlighted the significance of both direct and indirect effects and the wide differences among various industrial subsectors. The second Stern paper also explored employment effects and implications in countries at various stages of development. Specific research topics that result in improved methods of measuring or enhancing employment through industrial investment, or that bear on the choice of industrial subsector priorities or mix, or that affect the design of projects, etc., would be much to the point.

11. The status report of the Small Scale Industry Project (671-59) suggests that the analysis and synthesis of the massive data being gathered will be difficult and will require substantial input by the regional and functional staff concerned. It also indicates that several further research topics may deserve priority attention:

(a) analysis of suitable financial institutions and policies, and assessment of institutional arrangements in support of SMEs (para 14);

(b) interaction and balance between large and small industries, based on comparative country analysis; and

(c) research focussed on one or more industrial subsectors (e.g., the machine-building industries, para 22).

Work on technology transfer to SMEs might best be fitted into a more general study of the technological institutions for industrial development (para 18).

12. There is need for more systematic analysis of institutions supporting SME development -- for technology, management, marketing, export promotion, product design, etc., as well as various kinds of finance. What could be undertaken almost immediately is a review of experience in SME support, including criteria for judging institutional effectiveness, analytical descriptions of those support systems which are regarded as particularly strong, and possible means for closer linking of technological assistance with project appraisal in Bank DFC lending, with a view to taking greater account in such lending of capital saving devices and processes; some aspects of such a review are already underway. The examination of the record of experience of Technical Consultancy Service Centers and Technological Research Centers in several Indian states, now being initiated, should also be instructive. The modalities of rural industrialization may require separate analysis, for which the case study method used in 671-59 should be supplemented by other approaches.

E. Industrial Financing Systems and Institutions; Credit Markets; Public Enterprises

13. There is considerable interaction between the availability and conditions of finance and the pattern of industrialization. The Bank has a 28-year history of lending through financial intermediaries, much more

diversified since about 1970; it has also financed some capital market and venture capital institutions on a limited scale. A fresh look is needed at the relationship between the development of these entitites and industrial growth requirements. What kinds of specialization are productive? What strains and problems arise at various stages of expansion and diversification? (The panoply of industrial financing agencies promoted in recent years in Korea -- including merchant banks, venture capital and equity funding institutions, leasing companies, a potent Guarantee Fund, etc., -- which is being emulated in the Philippines and elsewhere, deserves in-depth analysis by an operationally-oriented unit, perhaps the Capital Markets Department of IFC.) How can more effective use by made of the commercial banking system to provide longer-term finance for industry? What incentives and policies will stimulate efficient financial sector development? How can future Bank programs foster such development?

14. Of the studies underway the following are relevant to financial and industrial sector work: Commercial Bank Behaviour (RPO 671-25); Capital Market Imperfections (RPO 671-59); Role of Informal Credit Markets (RPO 671-65); Financial Structure and Technology Policy for Small Enterprise Promotion -- a Case Study of India; and the review of the Consultancy Services and Research Centers cited in para 12. Other financial sector work underway or planned will have implications for further research in this field -- e.g., the assessment of means for increasing financial resources for industrial development in the Philippines. The findings so far of the Small Industry Study also call for further work on the transaction cost of financing SMEs and the role of alternative institutional channels. The special Bank study "The World Bank's Approach to Interest Rate and Credit Allocation Issues" (May 1976) also called for follow-up work in this area. Steps to define new priorities in this field would be (a) an assessment of the relevance for other country situations of the extensive research done on India, and (b) commissioning of a study designing a more applied research approach toward institutional and policy solutions for mobilizing longer-term finance for industrial growth.

15. Public Enterprises. The research project underway on this subject may provide some insights into problems and evaluation criteria specific to public enterprises as such. But while general diagnoses of these problems are often similar, it seems likely that many of the prescriptions must be more country- or industry-specific. The Bank's sector and economic missions are continuously addressing, in various ways, the problems of public enterprise in a country/operational context -- e.g., management, investment planning, relationships with other government departments and pricing and incentive policies. This approach seems potentially more productive than general or comparative studies.

F. Capacity Utilization; Capital-Labor Substitution; and Technological Change

16. Capacity Utilization: Two projects (RPOs 670-25 and 670-95) have been completed, though it remains to provide adequate reports of their findings. The research indicates that a variety of policies across a number

of fields (e.g., licensing) are in part responsible for low utilization rates, but that changes in these policies alone should not be expected to lead to greatly increased utilization. The research has important applications in industrial sector mission work. While no new research is planned, further work is needed to facilitate operational analysis of the prospects and means for improved utilization in specific circumstances, through the briefing of missions and subsequent review of their findings.

17. Capital-Labor Substitution: Detailed investigation of the scope for substitution has been carried out for mechanical engineering (RPO 670-23) and is linked to research on industrial investment analysis (RPO 670-24). The preliminary finding is that wide scope exists for substitution, but more so at low than at high output levels. Work is also in progress on technology in textiles (RPO 671-77), where the focus is explicitly on project design and operational staff are involved. This work is significant in that it may directly affect future Bank projects, by reducing costs and increasing benefits through changing the choice of technology. A wider coverage of industries is provided by the survey of recent case studies under the "Appropriate Industrial Technology" (RPO 671-51) project. Discussion of this survey, along with an evaluation of detailed investigations carried out within the Bank, will be needed to determine priorities for further research.

18. Technological Change: This is a high priority area for future research. The Bank has undertaken no research specifically in this area, though a good deal of operational work has the objective of fostering technological change. This work has led to projects based on pragmatic considerations in a number of countries, but there is an evident need for improving the knowledge on which technical assistance and R&D lending is based. Some present research is tangentially concerned with the mechanisms through which technological change occurs, and there are tentative suggestions for more directly focused studies. Nonetheless, generation of research topics that would lead to useful results will require a good deal of work. Exploratory research to investigate the nature of technological change and factors conducive to different types of technological change is likely to be required. Emphasis must be given both to the role of institutions and the effect of the general industrial policy environment. Research ought to be tailored so as eventually to contribute to the design of Bank lending, aimed at fostering technological change, in specific sub-sectors as well as small and medium scale industry more generally. Attention should also be given to learning from past Bank projects in this area.

G. Industrial Programming; Studies of Process Industries

19. Research under the "Programming in the Manufacturing Sector" (RPO 670-24) project has sought to develop a methodology for applied industrial investment analysis. Work on the process industries is farthest along, and comprises model formulation and specific applications for fertilizer (in several countries and regional groupings), cement, pulp and paper, and steel. For these sectors, the methodology focuses on the selection of the location, scale, timing and design of inter-connected projects. Work is also being done

on mechanical engineering, on the analysis of comparative advantage and on choice of technology.

20. Particularly in the process industries, this work has definite operational usefulness, but the extent and direction of further work is now uncertain owing to staffing constraints and questions regarding the proper locus of responsibility for operational applications. Possibilities for further work include: a) further applications for industries already covered but in other locales; b) extension to new industries, with chemicals being a prime candidate; c) development of algorithms (e.g., CAMS); d) technical assistance to users in country planning offices or consultant involved in project work; e) further research to incorporate aspects heretofore neglected (e.g., uncertainty). Consideration also needs to be given to translating the work done on mechanical engineering into an explicit framework for project selection.

21. Continuance of work in this area would require allocating staff resources to replace staff who have moved on to other responsibilities. Given the investment already made and the results to date, creation of a small (two-to-three man) unit for continuing work in this field may be justified. This would make feasible assignment of full-time responsibility for informing and educating operating staff regarding potential applications of programming methodology, for monitoring its use and the resulting feedback to refine the methodology and update technological information, and for further extensions in both application and research.

H. Specific Industries Studies

22. In discussion of several of the above topics it has been suggested that they might most usefully be studied in the context of a systematic analysis of one or more industrial subsector(s) in a few selected countries -- including the industry's structure, product linkages and sequence of development, relative efficiency and interaction of large and small scale units, technology choices, transfer and innovations, employment effects, etc. An obvious candidate is the machine building industry, given its central role in the industrialization process and in technology development, its varied character, and its potential for generating exports and employment. It is also an industry on which data are available, in the Bank and elsewhere, in considerable quantity for several countries. An intensive effort over several months will be needed to design a study and identify perhaps three countries for initial attention; to carry it out would involve a major investment of manpower, but it would seem to offer promise of illuminating a wide variety of issues and relationships.

J. Industrial Investment Strategy and Policies for Different Country Situations

23. The diversity of country situations calls for different designs of industrial growth paths and policies to support them. The categories listed

below are illustrative of possible differentiations. Country sector studies typically include considerable data and descriptive material that suggest significant familial characteristics; analysis of their findings in greater depth, in a more consistent manner, might provide further insights into the special potential and problems of these "types" of developing countries, and operational guidance for dealing with them. The Steering Group has not yet considered possible studies under this rubric; and no common analytical framework to guide ongoing sector work has yet been developed

24. A typology of industrial development patterns might identify the following:

(a) countries rich in natural resources (e.g., Venezuela, Iran) where resource-based industries provide the springboard for development but must be supplemented by downstream and supporting activities, and by expanding exports;

(b) countries just beginning the process of industrial development (e.g., Burma, Cameroon, Paraguay) where financial and manpower resources are limited and a wide range of issues must be faced;

(c) Sub-Saharan Africa -- similar in many respects to category (b) above, but with certain special features -- high wage levels inhibiting development of labor-intensive exports, dominance of private foreign investment (which may be a causal factor in the dualism of these economies, use of inappropriate technologies and the stunting of small scale enterprise), etc.

(d) the protective, import-substitution model (extensively studied but its complexities still defying definitive conclusions);

(e) The "Latin American pattern" (which also extends to certain non-Latin countries), characterized by semi-industrial economies, relatively rich in natural resources, with problems of unemployment, adjustments to continuing inflation and attempts toward greater export orientation.

K. Conclusions

25. It is very difficult to draw firm, final conclusions about the World Bank's near-term research program in the fields of industry and trade. The various topics discussed earlier in this memorandum interlock at many points; and with a few exceptions, continuation (and usually extension) of the research avenues opened up so far seems justified on the merits. The question is what can be accommodated within the budget and manpower resources prospectively available. As was suggested earlier, very little flexibility exists at present.

26. The Steering Group has not yet addressed, in systematic fashion, the establishment of priorities among the various topics outlined above; substantial differences of view, at least of emphasis, are evident. From the discussions

so far, however, it is generally accepted that work already far advanced -- e.g., that on patterns and sources of industrial development -- should be completed, and possibilities for application of the findings or methodology be further defined and made accessible to key operational staff, before major commitments are made to new blocks of research that would preempt these possibilities. Among potential expanded areas for research it would seem that technology development deserves a special place -- whether through a macro approach (para 18) or through study of specific industries (para 22), or from both perspectives, needs further analysis.

27. The Steering Group would welcome the views of the External Panel on this question of priorities. And before the next meeting with the External Panel the Group will reach specific conclusions on the matter -- assisted somewhat, we would hope, by the fact that the Bank's budgeting process for FY80 will have advanced considerably by that time.

28. The Group has not as yet attempted to quantify staffing and budget requirements for a research program still undefined and dispersed among numerous administrative units. However, it would underline the importance of adequate staff resources to help absorb and apply research results as they are achieved -- which may often require preparation of a clear synthesis of findings of use to country, sector or project missions; direct involvement of research staff in such missions; and time for practitioners to acquaint themselves in general with what research findings are available, and their implications and potential uses.

November 29, 1978

Areas for Research on Industrial Development

1. This memorandum is intended to help focus discussions on areas that merit attention in further research on industrial development by the Bank. The External Panel on Research in Industry and Trade may find it useful as an indicator of some of the topics that are being considered by the Industry and Trade Research Steering Group that has recently been established to review research programs in these two related fields. The Steering Group proposes, following initial discussions with the External Panel, to prepare a more detailed outline of a near-term future research program in these fields.

2. This memorandum should be read in conjunction with review of the status of on-going programs, with which the future programs may have strong linkages. Also suggested herein are some additional topics or lines of investigation that are becoming more important in the Bank's work and that have not been adequately covered.

3. The focus is on topics in industry. Consequently, this memorandum may be regarded as a companion piece -- with a different orientation and coverage -- to the memorandum entitled "Medium Term Work Program in Trade and Commodities."

4. In order to avoid a long, indigestible list of individual topics, among which it may be difficult to reach any consensus on priorities, a smaller number of topical "families" are identified below. Each family may include a number of individual topics that are closely related, and it is not necessary at this stage to attempt exhaustive identification of specific topics. The order of presentation of the topical families does not imply anything about priorities.

5. A point of general concern affecting all the topics listed below is the question of the basic orientation of the Bank's research in industry. The four main objectives stated in the terms of reference of the External Panel include, inter alia, "to support all aspects of Bank operations . . ." and "to broaden our understanding of the development process." In order to meet these objectives the research program will necessarily involve a mix of work on innovative methodologies as well as work of immediate relevance to specific operational problems. The time required to develop new methodological approaches and to perceive operational "payoff" may be lengthy. Ultimately, however, all such work must be related and of value to the stated objectives of the research program, and in areas where the Bank has a comparative advantage over academia. This relationship needs to be continuously reviewed, its definition refined, in order to avoid a widening gap between researchers and their work on the one hand and the practitioners in the Bank's project and industrial policy work on the other, and worsening of the already serious dissemination problem. At the same time, due attention must be given to meeting the research needs that emerge from the practitioners' day-to-day work and problem solving attempts. The staffing and organization of research should be such as to enable research staff to work closely with the operational and sector departments and have a participatory role in the Bank's industrial operations and policy dialogue.

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The formation and composition of the Steering Group was intended to reflect this necessity, and to take account of the range of questions and options it poses in considering research priorities; the External Panel is urged to do likewise.

A. Industrial investment strategy and policies in selected country situations.

6. The variety of country situations calls for different designs of industrial growth paths and policies to support them. The following categories are illustrative of differentiations that can be made; in-depth case studies could be undertaken to illuminate familial differences and to provide operational guidance.

(a) Countries rich in material resources (e.g., Venezuela, Iran, Indonesia). Resource based industries may be used as the springboard for development but must be linked to other downstream and supporting activities, and to expansion of exports.

(b) Non-industrial economies (e.g., Burma, Cameroon, Paraguay), which are just beginning the process of industrial development; attention must be given to the relative importance of policy tools, incentives, management and labor training, the role of private foreign investment, and the sequence of import substitution and export development. Research project could take off from work done on basic mission to Tanzania and current sector work in Cameroon.

(c) Sub-Saharan Africa. While similar in some respects to countries in category (b) above, this group of countries may deserve separate treatment for various reasons. High wage levels may inhibit development of labor-intensive export industries. Dominance of private foreign investment in Africa may have a great deal to do with the dualism of these economies, use of inappropriate technologies and the stunting of small scale enterprise. A comparative study on wage rates and labor productivity would throw more light on this problem, and on reasons for their divergence; this needs further exploratory work and preparation of a research design. A review of the literature on (i) the impact of private foreign investment on African economies, and (ii) the relevant policy frameworks of African governments should be undertaken initially; a great deal has been written but there is little evidence, in sector policy or operational decisions, of its absorption by Bank staff. A systematic review would enable us to take stock of what is known, and to define more correctly the subjects requiring further research.

7. Other patterns might be the export-oriented East Asian countries, the import substitution pattern which has characterized South Asia in the past (although now changing to some extent), and the Latin American propensity and adaptation to inflation. All have been studied, more or less intensively -- more than those in para 6 above -- but there is continuing need to relate the findings of these studies to other regions and situations (as in para 6).

B. Employment enhancement through industrial development.

8. This topic clearly is important in the context of the Bank's concern for alleviating urban and rural poverty and its emphasis on creating not just more jobs but more productive (i.e., higher paying) jobs, since solutions are supposed to be efficient and not simply welfare systems. Papers by J. Stern made a beginning on this problem, and highlighted the significance of both direct and indirect effects, and the wide differences in this regard among various industrial subsectors. The second Sterm paper also explored the implications of employment effects in countries at various stages of development. Specific research topics that result in improved methods of measuring or enhancing employment through industrial investment, or that bear on the choice of industrial subsector priorities or mix, or that affect the design of projects, etc., would be much to the point.

9. One important sub-family of topics is the employment (and other) aspects of small and medium scale enterprises (SME). Their problems are often thought to be so special as to constitute a separate genus. Are they efficient users of factors? In what industries or activities? How can efficiency be enhanced? What special incentives or technical and technological assistance do they need? What mix of small and large scale industries is effective, and how do they interact, in complementary or competitive ways? More broadly, what is a proper balance between capital-intensive and labor-intensive industries in countries at different stages of development and with different size and population characteristics? What are the implications of large scale, capital intensive patterns of industrial investment for indigenous entrepreneurship, finance, business organization and technology? These are some illustrative issues.

10. There is need for a more systematic analysis of institutions supporting SME development -- for technology, management, marketing, export promotion, product design, etc., as well as various kinds of finance. What could be done almost immediately is a summary of Bank experience in SME support, including criteria for judging institutional effectiveness, analytical descriptions of those support systems which are regarded as particularly strong, and possible means for closer linking of technological assistance with project appraisal in Bank DFC lending with a view to taking greater account in such lending of capital saving devices and processes. The modalities of rural industrialization may require separate analysis: the nature of industries suitable for rural resource endowments and markets, linkages with agriculture, infrastructure requirements, policies and institutional framework, etc. The case study approach that has often been used (e.g., 671-59) may have to be supplemented by other approaches to this problem.

C. Developing the technological base.

11. Many countries, particularly those classified as semi-industrial, are increasingly interested in developing their own technological capabilities, reducing dependence on licenses and enhancing their adaptive capacity. Technological assistance to small industry, to be effective, may have to be linked to R&D activity of a simpler character, especially in smaller or poorer countries. Some specific projects are in process of discussion and design, and a few have been approved and are being implemented. Moreover, in industrial sector work more attention is being given to this topic. For example, work on the engineering industries is being stressed because they may have many desirable characteristics (e.g., they tend to be efficient at small scale; they tend to be labor-intensive; the demand is highly income elastic; they have strong backward and forward linkages; there are definite export possibilities; and they are an important "carrier" for technological change). These characteristics need to be more fully demonstrated and conditions for successful projects established through research in depth on the policies, institutions, and methodologies for strengthening indigenous capabilities in countries at different stages of development.

12. Possibilities of labor-capital substitution are under study in the mechanical engineering industry, with special reference to African conditions (Phase II of R671-51). Depending on the outcome of these projects, further studies of possibilities of improved project design in selected industries may be indicated.

13. It is recognized that research on technology will require close collaboration between engineers and economists, will have to be product and industry specific, and possibly be conducted in a sequential manner. Hence, we cannot expect to mount a large number of projects in this area. Our main object would be to influence the processes of project preparation and industrial planning by paying more explicit attention to the technical feasibility of capital-saving devices and processes.

D. Financial systems and industrial development.

14. There is considerable interaction between the availability of finance and the pattern and direction of industrialization. Several topics involve issues that affect Bank operations and hence may deserve attention. The Bank has a long history of lending to financial intermediaries. A fresh look is needed at the relationship between the growth of these entities (and also capital market and venture capital institutions) and industrial growth requirements. What kinds of specialization are called for? What strains and problems arise at various stages of expansion and diversification? What are instruments and conditions for success? What alternative sources of finance (foreign and domestic) are available? What incentives and policies will stimulate the right kind of financial sector development? How should future Bank programs aim to foster such development?

E. Programming models in industry.

15. This work got off slowly and has had success in the Egypt fertilizer model, the S.E. Asia and India models; it is in danger of being terminated for lack of wide recognition of its potential practical application. There is a whole "cascade" of modelling work that needs to be pursued: investment planning in a sector for a country; allocation of output within firms in the industry (the locational aspects are important); planning output within the firm; linking sector models to obtain an economy-wide model; inter-country locational models for investment and output planning. A continuing effort is needed to avoid losing momentum. (See 670-24.)

F. Structure and patterns of industrial growth among countries.

16. This topical area has received a great deal of attention in the Bank, most notably in project 671-05. Another "round" of activities recently approved, involving special case studies on Turkey and Korea, will use a newer and modified data system and will pursue prior country results to investigate comparisons in several dimentsions. Questions of a similar kind also arise in the context of Bank operational work.

17. Decisions to support specific projects by the Bank are made primarily on the basis of calculations internal to the project and the country (e.g., economic and financial rates of return). But increasingly there is awareness that in counseling several countries to pursue (or not) projects in textiles, pulp and paper, fertilizers, shoes, steel, etc., a global (or at least a regional) framework and consideration of dynamic changes are really required. Also the locational aspects of new investment have been brought up, specifically by the UNIDO Lima targets; the FAO has been conducting studies of the phased development of world pulp and paper resources, and the subject is receiving increasing attention in various other contexts. In order to have a solid basis for recommendations on industrial strategy, the Bank should undertake research on the relative development and locations of investment in selected intermediate goods (as indeed it does now on a limited basis); but, more importantly, the research should investigate questions such as: What are the gains and losses in trade and market control? Will the locational shifts lead to greater or lesser efficiency? What are the implications for investment criteria and finance? What are appropriate Bank responses and approaches in supporting specific projects in these products?

G. Export potential and promotion of industrial goods.

18. This topical area provides the best example of overlapping interests in industry and trade. Extensive work has been done in the past, notably on trade incentive systems, and further work is outlined in the memo on the work program for trade and commodities mentioned previously. This research has now reached a stage where it can be applied to particular country situations, as a basis for assessing or devising specific operational policies/programs for export promotion. The scope

of the work will have to be tailored accordingly. We have the West African studies (DRC) at one end, and the Turkey and Portugal examples at the other. Several other countries will require fresh attention. We should also assess DFC lending as a tool of export development, with a view to making it more effective.

H. Policies and programs affecting public sector enterprises.

19. Increasingly Bank sector missions undertake to review the performance of public enterprises and suggest policy and institutional changes to improve their efficiency. Public enterprises cannot be judged on the same basis as private firms, and we need to develop an analytical framework adequately to tackle this problem. Our capability to provide policy advice in such areas as pricing and product mix, incentive and control mechanisms, financial policies and socio-economic environments needs to be strengthened. Another key area is the institutional framework for managing and controlling public sector enterprises -- what forms are appropriate under different political and administrative contexts, e.g., holding companies as in many Western European countries or a Public Enterprise bureau as in India.

J. Investment appraisal methodology.

20. The acquisition and processing of data for shadow pricing and effective protection or domestic resource cost calculations can be time consuming and costly. Exploration of short-cuts and "second best" methods to determine how close they come to full-system results, how sensitive they are, how much time they actually save, etc., would potentially be of significant benefit to the project analyst.

K. Studies undertaken in context of DFC and industry lending.

21. There are relatively few such studies (certainly less than, for example, in Education or Rural Development) but in a number of cases, in different countries, they clearly relate to our operational concerns, e.g., textile sub-sector, characteristics of small industry sub-borrowers, trade incentives and functioning of financial markets. There is need for a more systematic review of what exists and of the potential for focussing the attention of borrowers on key issues in their country context. More Bank staff support in starting the studies, and in reviewing their implications and usefulness, would doubtless be required to do this effectively, and this may be a bottleneck; but effective use of these external resources should certainly be economic in the longer run.

July 13, 1978
IDFD

OFFICE MEMORANDUM

TO: See distribution below DATE: December 5, 1978

FROM: Ravi Gulhati, EANVP

SUBJECT: Research on Industry and Trade: An African Perspective

1. This memorandum should be read as a supplement to the general report of the Steering Group. The aim is to review past Bank research from the standpoint of usability in African settings and to offer some comments on future priorities. Let me hasten to add that the memorandum is being written in a great hurry and there has been very little opportunity for reflection or consultation with colleagues.

Pay-off from Completed Research

2. Although past research has not treated the key questions for African industrialization, it has a lot of relevance for these countries. For example, Bela Balassa's work on effective protection and biases in incentive systems is very germane in countries such as Kenya, Tanzania, Sudan, Zambia and Madagascar. An analysis of the exchange, tariff, tax and interest rate regimes from this standpoint would illuminate policy options. And yet very few studies of this kind have been made./a Another example is the work on capacity utilization which defined alternative approaches for diagnosing the phenomenon of idle capacity. This too is very relevant in East Africa where the problem is widespread and where studies of the causal factors may trigger policy reforms. Few analyses along these lines have been made so far. Yet another example is the Chenery-Syrquin analysis of industrial patterns which provided a framework within which sector studies on individual countries could be conducted. Unfortunately, the results are not very robust for very low income countries (such as those in Sub-Sahara Africa).

3. Three factors have interfered with the utilization of past research in East African countries. First, regional staff have not had the time to absorb these findings and to apply them in the field. Support from the DPS has been available only on a highly selective basis. Secondly, our member countries are not well-endowed with well-trained, sophisticated professional staffs with appetite for elaborate methodologies. Finally, statistical data are very scarce; industrial surveys are infrequent and there are scarcely any reliable input-output tables or capacity utilization figures. Trade data are less scarce than industrial data.

/a The exception is RPO 670-87 which produced case studies in West Africa.

Distribution:
Members of External Panel on Industry and Trade Research
" " Steering Group " " " "
Regional Chief Economists
Messrs. Chenery/Karaosmanoglu/Balassa/Bery, DPS
Haq/Streeten/Wright, DPS
Eastern Africa Region:
Messrs. Wapenhans/Adler/Gue/Wiehen
Bronfman/Hendry/Maubouche/Maane
O'Brien/Greene/Nouvel

Observations on Future Priorities

4. It makes little sense to make new research starts while the existing body of knowledge remains considerably under-utilized on account of the shortage of staff required to apply it in specific country situations. Presumably, senior management will not allow this anomalous situation to continue. It can be corrected by (i) expanding substantially the resources for country economic and sector work in the regional offices, (ii) increasing sharply the share of DPS/CPS time devoted to support of operational missions with a corresponding reduction in time for new research, and (iii) increasing DPS/CPS staff so that both operational support and research activity can expand.

5. There is recognition now of the vast diversity that exists among LDCs. The African countries are very much behind all the rest of the Third World in terms of industrialization and economic development generally. They have low incomes, savings, small populations scattered over vast areas, thin layers of industrial entrepreneurship and an extreme scarcity of managerial/administrative talent. It follows that the key issues for industrial research in Africa will be very different from those in other continents. Furthermore, to be relevant and usable, industrial research on Africa will have to be 'data-saving' in character. It will be a long time before countries in Sub-Sahara Africa acquire the statistical base which exists now in East Asia, Latin America and the mediterranean area. To be usable in the 1980s and 1990s, methodologies will have to be developed for Africa which rely much more on field visits, structured interviews and limited case studies than on econometric techniques and models requiring well-organized censuses, sample surveys and cost-accounting records.

6. The implications of the special needs of Africa for future Bank research priorities must be recognized explicitly. Of course, this applies not only to industrial and trade research but to all research. If measures are not taken to secure an appropriate balance in the Bank's research portfolio, there is the danger that we will have too little research output which is germane to key African issues and which is usable in data-scarce African settings. Our natural propensity will be to focus on issues which are "researchable", i.e. where available staff can deploy familiar tools in situations which are reasonably well-endowed with statistics. The real challenge for research managers is to avoid falling into this trap. This will not be easy. At least in the short-run, choices will be constrained by (i) the need to complete existing research projects or follow through on a line of enquiry involving a sequence of research projects, (ii) the training, experience and preferences of available staff, and (iii) the haziness of alternative methodologies usable in data-scarce situations. These constraints, however, need not be binding over the medium and long run and research strategy should be responsive to the special needs of Africa both in terms of issues and usable methodologies.

7. Let me conclude by listing what I regard as key issues in Africa which research on industry and trade should tackle:

a) While some countries have chosen the etatist route to industrialization (i.e. use of parastatals), others have allowed transnational corporations to enter freely. Both institutional solutions are fraught with problems. There is need to define optimal structures appropriate to the history, ideology and other relevant characteristics of each country. Before this can be done effectively at the country level, however, it is necessary to undertake a number of comparative analyses of (i) the efficiency of parastatals, (ii) regulation of parastatals by core ministries, (iii) the efficiency of transnational corporations, (iv) policy towards private foreign investment, and (v) origin and development of indigenous, private industrial entrepreneurship. To define the methodology for these studies will be difficult and the Bank may not have the relevant expertise. The neo-classical economists will look with horror at this research agenda. And yet the substantive issue is a crucial one.

b) Most countries have rather small national markets for manufactures implying limited scope for efficient import substitution. This may turn out to be a major hurdle unless (i) regional or sub-regional markets can be created, (ii) exports to world markets can be generated, despite the detrimental impact of relatively high wages on competitiveness of labor-intensive exports, and (iii) technologies can be identified which are scale-neutral. A study of the future prospects for industrialization of Africa should deal with these questions and provide a framework within which sector studies in individual countries can take place.

c) A number of Governments in Africa with a socialist and egalitarian philosophy are searching for patterns of industrial development different from those characteristic of the now rich industrialized countries or Eastern Europe or the semi-industrial economies of Latin America and East Asia. As "late starters", they wish to avoid the mistakes of earlier industrial drives. Bank research can make a contribution by deriving the lessons of the history of industrialization relevant for 'late starters'. To be useful, such a synthesis will have to draw on scholars familiar with the Soviet and Chinese models as well as Singapore, Brazil, India and Korea/Taiwan models.

d) There is resistance within the Bank to explore the implications of "appropriate products" (see F. Stewart's writings) although work is under way on "appropriate techniques". I have never understood the reasons for this resistance. If African countries wish to strive

for egalitarian development they will want to question the appropriateness of rich country products for satisfying the basic needs of the population. The concept of "appropriate products" is, of course, applicable to poor countries everywhere but the ideological climate necessary for implementing policies to alter the product-mix exists especially in some African countries, e.g. Ethiopia, Tanzania, Angola and Mozambique. The implications of this strand of thought for industrial policy need to be drawn out after an empirical assessment of the adverse consequences of manufacturing or importing 'inappropriate products'.

RG/mo

WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Distribution List

DATE: December 13, 1978

FROM: Luis de Azcarate, Chief Economist, WAN

SUBJECT: Research on Industry and Trade

I support Mr. Gulhati's memo of December 5 on the subject as practically everything said there applies to West Africa as well. I would like to add three additional areas where research would be useful for us:

a) Labor costs in industry: Africa is often seen as an area where labor is "cheap." However, although nominal wage rates are, of course, low relative to those in developed countries, the actual cost to the firm, taking account of low physical productivity, rates of turnover, and other personnel problems may, in fact, be higher than assumed. Furthermore, the implication of the cheap labor paradigm is that labor is cheap relatively to capital. This also requires further investigation at the level of the industrial firm.

b) Our experience with DFC sub-loans to medium and small-scale enterprises is that the capital cost per job appears to be higher in West African countries, especially in francophone countries, than in other parts of the world: we would like to know much more about this, especially whether the cif cost of a number of capital goods is generally higher than elsewhere or whether the distribution costs are the main factor, or what. Since exchange rates are usually overvalued rather than undervalued, this cannot explain the higher domestic cost of capital equipment. It would appear also that locally-manufactured implements are not particularly cheap.

c) The question of the level and structure of interest rates in Africa is a general one. The prevailing view in Africa seems to be that interest does not weight much in industrial investment decisions and, therefore, it would not help much if, as we often propose, they were raised to more "natural" levels. At the same time, governments always strongly resist any idea to increase interest rates. An investigation on the response of industrialists to actual or hypothetical changes in interest rates (as well as better documentation on the effective cost of bank loans to firms) could help clarify the issue and assist us in our dialogue with the countries.

Attachment

Distribution

Members of External Panel on Industry
and Trade Research
Members of Steering Group on Industry
and Trade Research
Regional Chief Economists
Messrs. Chenery, Karaosmanoglu, Balassa, Bery, DPS
Haq, Streeten, Wright, DPS

cc. Messrs. Chaufournier, Pouliquen, Guetta, Senior Economists

LdeAzcarate/js

OFFICE MEMORANDUM

TO Mr. David Gordon, DFC

DATE December 22, 1978

FROM E. Bevan Waide, ASNVP

SUBJECT Interim Report of Industry and Trade Research Steering Group

1. I would like to comment from a South Asian perspective on some aspects of the work described in your draft Interim Report. In general terms, I think our industrial economists need a supply of "alternative generating tools" for their work in the South Asia Region. By this I mean tools and techniques which enable them to demonstrate to governments the policy implications and alternatives implied by the mix of current problems that the governments face. Examples of this kind of technique or tool are the DRC ratio, which by now is quite an ancient technique but is still very useful in establishing a dialogue with a government which has highly protected and inefficient industries. It enables the economist to give the government some feel for the potential costs and benefits of maintaining the present protective structure or of liberalization. Other examples are, of course, the effective protective rate and effective exchange rate in the areas of tariffs and export promotion respectively. In general then, these are the kinds of tools that would enhance the work of our industrial economists.

2. A supplementary device which would be of great assistance would be comparative information on the experience of developing countries, and sometimes the developed countries, in the use of various tools in various ways; that is, in the use of various policy alternatives. Here again, it would give our economists more strength and consistency in terms of clarifying for governments the probable outcomes and effects--the cost and benefits--of taking one of the choices that face them. As a case in point, your paper (Section D, paragraphs 10 through 12) describes several studies dealing with the problem of small-scale industry and employment creation. In a number of South Asian countries (e.g., Bangladesh, India, Pakistan) the problem of poverty alleviation consists in creating off-farm (presumably industrial, construction, service) jobs for the masses of rural poor. Although we have initiated some work of our own, we are starved for ideas on how, practically, countries can progress in this area. As an institution we take the view today that the path to rural uplift is not to be found through "trickle down" as GNP slowly grows. Does the answer then lie, e.g., in more focussed efforts such as integrated rural development programs (in a regional planning framework?)? Should public investment be redirected to generate rural employment (small-scale industry? investment in rural infrastructure? creating local industrial production and distribution capacity for meeting basic needs?). There are both broad conceptual questions as well as practical problems of devising strategies, programs and projects. Is there now a large enough body of LDC case study experience (success and failure stories) for us to be able to draw lessons of practical use?

3. A third useful characteristic of any research should be that it lends itself to the preparation of backup support. For example, the

extensive work done by the Development Research Center on domestic resource costs in West Africa has led to a computerized model for analysis of DFC data. It will probably be possible to collect data from South Asia in such a manner as to use this program.

4. Another characteristic of Bank research which could be very useful to us would be if it could compile and distill research work being done outside the Bank, and again turn it into useful techniques and tools. For example, almost every country in our Region has a network of public enterprises which are encountering serious problems of varying kinds. From what we can see, it would appear that the current Bank research in this area is considerably behind that being undertaken by other institutions. Indeed, some of the economists in our own area have in fact been involved in research into public enterprises and we do have some ideas about approaching the problem. But this entails pulling together some very useful work which has been done in recent years by such institutions as the Harvard Workshop on Public Enterprises and others. To undertake this kind of work is an excessive load for operational program or project economists, and is the kind of work that should be done by research departments.

5. Our plea, then, is for research which is essentially "research and development" of technology for the economists involved in Bank operations. We need tools which enable us to analyze adequately and demonstrate with clarity and some precision the alternatives available to governments in various areas of industrial policy ranging from industrial organization to management, from licensing procedures to the financial structure. We need to be sure that we are making sense and being consistent across the Bank when we elucidate the implications of alternative policies to the various governments with whom we deal. We need to know that the analysis we are providing and the recommendations we are making are consistent with the most recent research done in these areas. We feel, therefore, that the Bank's research should be primarily applied and not "pure". Careful review of available, applicable research may, however, show some gaps, and these gaps--insofar as they are preventing useful application by the Bank of techniques and principles that could further our work--should be filled by the Bank. To this extent, then, the Bank should leven its work on applied research with some "gap-filling" pure research.

DWilliams/HPilvin:hh

Distribution: Messrs B. Balassa, H. Fuchs, R. Gulhati, F. Moore,
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