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ISAD Reference Code: WB IBRD/IDA DEC-01-05

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The World Bank
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INDUSTRIAL DEVELOPMENT & TRADE
PANEL REPORTS (3)



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I. Incentive Policies; Economic Integration

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Comments

OFFICE MEMORANDUM

TO: ✓ Mr. Bela Balassa

DATE: March 13, 1979

FROM: Donald B. Keesing DBK

SUBJECT: Draft Report of External Research Panel on Industrialization and Trade

1. I find myself very much in agreement with most of the External Panel's draft, in its semi-final form, and I think it is already excellent.

2. The first chapter is particularly extraordinary in quality in everything it treats. The one omission that I sense is that too little attention is paid to the limitations inherent in existing Bank rules of staffing and organization. The nonavailability of senior level (M level and over) staffing slots for new appointments in DPS, and the rigidities in the existing supply of slots, table of organization, and hiring rules, are likely to make it impossible for the Bank to do what is recommended in the second chapter. I would like to see a few words to the effect that some Bank researchers feel strongly that the Bank research effort is now limited by our recruiting and staffing and nationality rules together with staff slots so that one is dealing with a severely overconstrained system, not well designed for taking up new research tasks or shifting resources toward new priorities.

3. In Chapter II, assuming that slips in wording or proofing will be corrected, notably "low lost effort" on page 22, I find myself in at least rough agreement with the evaluation of past and present research as far as I know it. Thus, in the first 21 pages, my only significant disagreement comes from an intuition on my part that further research in some of the directions recommended on pages 22-26 will require further mining of the data turned up by earlier projects, notably the initial "Sources" project (RPO 671-32) and those that treat capital-labor substitution and scale economies. Particularly in regard to "Sources," I suspect that the potential applications have only begun, because the data contain a potential for defining and measuring efficiency and productivity gains, technical change, etc., in new ways. In this regard I would suggest that the draft ought to express at least some uncertainty as to whether new breakthroughs might come about in using the data being generated by these not-yet-complete projects, once the results become available and fall into a wider set of hands, particularly when these data are looked at as a possible input into further studies.

4. I would also associate myself with the general thrust of the recommendations concerning new research, although not with all the details and omissions in the discussions of these topics. Of the three, I most like what is said under "innovation, entrepreneurship, and technological change." The discussion of "internal policies and institutions" seems to me rather ill-defined as yet to fit the Panel's own rules as to what sort of research is likely to be successful in the Bank. This is all the more true since the general bias of the Bank, from Mr. McNamara down, is to prefer quantifiable questions and issues, where one can hope for a

numerical or numerically-based answer. Unless the argument is convincingly made with some indication of what will be learned, the recommendation is in danger of being taken lightly.

5. In the section on topics related to trade, it seems to me that the research described in the first paragraph on page 23 is not trade research alone but potentially involves study of scale economies, choices of technology, learning effects, interindustry relationships, other externalities, experience in building institutions, and effects of alternative policy regimes in generating efficiency, entrepreneurship, sound investment decisions, and desirable changes in human resources and technology. The study of these less industrialized economies could be grouped about as well under research on internal policies and institutions, if not the other heading. Perhaps it belongs as a fourth focus of research, or perhaps it should be discussed in connection with all three groups of topics. Within this discussion, I am also not clear on the three-way distinction among countries "rich in natural resources, those which are just beginning their industrialization, and those which are extremely poor and have not yet started in developing manufacturing activities." These categories as described overlap the last two and are hardly different from one another. Perhaps countries rich in mineral wealth, those with substantial agricultural resources and light population density, and mineral poor countries that are already densely populated, would be a proper three-way distinction, with some further distinction in each category between those with large and small populations, perhaps even with explicit attention to islands and landlocked countries. Despite all these potential distinctions, all these economies would have in common a very modest level of industrial development as yet. To the extent that trade opportunities and policies are to be the focus in studying these economies, it may be very important to evaluate their opportunities, particularly in manufactures, in the context of broader research on what is happening to trade and industrial development in the more advanced developing countries, and in recent transitional countries such as Japan and Italy, while taking into account the effects and implications of protection in industrialized countries. Surely the changing global trade picture must be analyzed as a whole to classify the opportunities of the less industrialized developing countries--and even then, it may be very difficult to evaluate whether Paraguay or Sierra Leone or Nepal can eventually turn into a successful manufactures-exporting country, in terms of the supply and cost situation.

6. Within trade, I find two major topics not sufficiently mentioned and one in which I disagree with the emphasis. In my judgment, studies of what is happening to trade as a whole, in countries at different levels of development, could be usefully undertaken on the basis of a combination of long-term historical series going back about twenty years, and projections forward for another twenty, with attention to changing market shares and systematic shifts in costs and comparative advantage, searching for the underlying forces and generalizations that might give a sound predictive handle for the future. As a second strand of trade research, I see a need

to study the impact in detail, and down to a country level, of particular protective devices used in the industrialized countries. This would have at least five purposes: to add to our knowledge of the processes involved; to furnish ammunition for getting rid of protection; to provide guidance for improving its design so as to reduce negative effects; to convince countries choosing trade policies not to overestimate the damage to be expected from the obstacles being created; and to provide an objective basis for their development strategies. Research in this area would be in some ways complementary to the Baldwin-Waelbroeck studies. Concerning trade among developing countries, I sense a need to analyze in some depth what is happening independently of regional arrangements. In my observation there is already a large literature on regional arrangements but almost nothing on what is happening anyway, mainly outside these arrangements. Without this, the study of preferential agreements appears poorly grounded and fails to yield satisfactory fruit. Hence, I find one-sided the discussion of inter-LDC trade on the bottom of page 22.

7. Some, if not all, of these points have probably already been considered by the Panel. I mention them in the hope that some may be helpful even though others may not be. Precisely because the draft is already so perceptive and well written, it is a pleasure to be associated with it even as a critic. My congratulations to the authors!

cc: Mr. B. B. King, DEDDR
Mr. L. Westphal, DEDND

OFFICE MEMORANDUM

TO: Mr. B. Balassa, DRCDR

DATE: March 13, 1979

FROM: Dennis Anderson, DEDER *DA*.SUBJECT: Report of Research Panel on Industrialization and Trade - Further Comments

Chapter II, Section II.1 (d), p.7. This paragraph still hasn't taken into account the comments that Mariluz Cortes and I submitted to Mr. Bueno on February 22, 1979. The next draft might note more explicitly that a primary purpose of the first phase of the research is to develop methodologies. I still feel, further, that the Panel's comments on "quality levels" are premature, given that we are still in the midst of the first phase, and were barely one-quarter of the way into it when we met Mr. Bueno. It is also perhaps incorrect to classify our research under the general heading of "Review of Past Research."

cc: Messrs. King, Leiserson, Westphal, Mazumdar, Ms. Cortes (DED)

DAnderson:krt

OFFICE MEMORANDUM

TO: External Review Panel on Research on Industrial
Development and Trade

FROM: (through: Mr. S. Bery, VPD)
Larry E. Westphal, DEDND LW

SUBJECT: Comments on Final Draft: Report on World Bank Research
on Industry and Trade (pre-dated March 31, 1979)

DATE: March 14, 1979

1. Since I have not said it formally in writing before, let me first take this occasion to say that I think the report is an excellent one. In particular, Chapter 1 contains a great deal of wisdom regarding nearly all relevant dimensions of research management within an organization like the Bank; whatever mix of priorities is finally decided upon, I am confident that researchers will benefit greatly from the availability within the Bank of this lucid and penetrating discussion. You have done us a great service in taking the time and care to spell out the various considerations with such clarity.
2. Chapter 2 should prove equally valuable in future discussions of research on industry and trade. Especially to be welcomed is the clear articulation of the Panel's recommendations regarding future research directions. I agree with the thrust of much of what is said, particularly insofar as it represents an evolution from the existing base of Bank research. Though it is doubtful that all of the recommendations will be accepted or implemented with the dispatch that the Panel might desire, the recommendations themselves should usefully help to focus future discussions within the Bank.
3. I have two general comments and several specific ones on the present draft. None of them should come as surprises, since I believe all of them have been made to you previously. Moreover, most of them are rather minor, in the sense that I could comfortably live with the draft as it now stands. In this respect, I wish to convey my appreciation for the obvious diligence with which you have worked to accommodate comments on previous drafts.
4. The more general comments follow: Firstly, taken in its entirety, Chapter 1 appears to me to lay blame roughly equally on researchers and on operational staff for the poor communications between them. However, others may conclude differently from their reading. In particular, the discussion starting on page 17 seems to lay the blame almost entirely on the researchers. I would urge that the discussion at this point be softened a little, to reiterate that, if nothing else, operations officers have been severely constrained in their ability to communicate with researchers by the force of their other responsibilities, the point being that the failure is not for our lack of trying.
5. In turn, I still look in vain for recognition that much of the research recommended to be "abandoned" has in fact not yet been completed through final documentation, or publication. Also missing is a strong, parallel recommendation that it should so be completed. I believe that I have several times made

March 14, 1979

this point in previous comments, asking for a strong statement of support by the Panel for completion of on-going research, along with an indication that the resources (and time) required are not minimal. In many cases, you have been able to read and absorb the content of our hastily written drafts, which for a more general audience desperately need to be polished. Moreover, a sizable volume of this research has not been completely written up. Statements along these lines could usefully be put in the summary that starts on page 26 of Chapter 2.

6. Various specific comments follow; all of them pertain to Chapter 2.

7. Page 4; I continue to object to the statement "The research guided by the more ambitious general equilibrium conception (RPOs 670-07 and 671-09) has not yet added much to existing knowledge about the development process." I would again recall to you that there has barely been any research as yet under the latter RPO.

8. I also think legitimate issue can be taken with the next statement, "Moreover, skepticism about its usefulness predominates within the operational staff." The only application of a CGE model in actual operational work in the Bank was the Dervis-Robinson model of Turkey, the perceived usefulness of which is attested to by its use in the preparation of various formal operational documents used in discussions with the Bank's senior management, the Turkish government, and in presentations to the Bank's Executive Directors. The operational staff involved (which includes the regional Chief Economist), while sensitive to special features which may preclude replicability in other country situations, have highly praised the work, particularly the explicit concern of the researchers with relevance to questions of operational concern. Nonetheless, one should note a central intellectual issue which remains unresolved, namely whether a model at a somewhat higher degree of aggregation might not have proved just as useful; surprisingly, the operational staff argue the negative. Finally, in regard to the quoted statement, it is probably more correct to say that generalized skepticism predominates about the usefulness of any model, an attitude reflective of a general problem facing researchers who aim to test or otherwise show the operational usefulness of models.

9. Page 20; with regard to the need for applications under process industry investment programming, you may wish to indicate that in this area, as in some others, extension services to application are required not only within the Bank but equally to member developing countries. While Bank operational staff should be capable of making use of such studies, it seems most likely that the studies themselves would generally be undertaken in the less developed countries. Indeed, the need to offer extension services to potential practitioners outside the Bank is a central concern of Mr. Parikh's detailed review of this research.

10. Page 21; it is very useful that you have now spelled out the grounds for your skepticism regarding CGE economy-wide models. I would make two observations. First, the uninformed reader may misunderstand that these

March 14, 1979

grounds apply as well to the research on programming models for the non-process industries; I doubt this implication is intended -- while the grounds are doubtless similar, they would be stated in different language. In turn, if the "cons" are to be spelled out, we would also appreciate the "pros" being enumerated at equal length, with more specific reference to the project in question. This is of course a matter for your discretion, but I nonetheless have asked Mr. Robinson to draft a paragraph of "pros" that might usefully accompany your statement of the "cons." He is transmitting it directly through Mr. Bery.

11. Finally, I should probably comment on the skepticism regarding programming models for non-process industries; I don't believe I've commented on this before. I share the skepticism, but believe that the question remains very much open. Some day, under the appropriate circumstances and in relation to a carefully selected quasi-operational application, I would myself like to test the merits of this approach. (I do not consider that it has been tested heretofore, since the work to-date has mostly been development work.) I have nonetheless not objected to the expression of skepticism, for the simple reason that in this particular case I do not believe the Panel's statement will have the same kind of potentially damning consequences that may arise in the case of CGE models.

12. In closing, let me make clear how much I have appreciated discussions with all of you over the past nearly nine months. I have learned a great deal, and benefited both personally and professionally from the experience of your review. I have the greatest admiration and respect for your insights and judgement, which are well reflected in the present draft.

LEWestphal:mm

cc: Messrs. H. B. Chenery, VPD
B. B. King, DED
A. Stoutjesdijk, DED
✓ B. Balassa, VPD

OFFICE MEMORANDUM

TO: External Review Panel on Research on Industrial Development and Trade. (Through: Mr. S. Bery, VPD) DATE: March 14, 1979

FROM: Sherman Robinson, DEDND S.R.

SUBJECT: Report of Research Panel on Industrialization and Trade

In general, the report is excellent. However, I still have problems with the discussion of the RPO's based on general equilibrium models. In chapter II, page 4, the following appears: "The research guided by the more ambitious general equilibrium conception (RPO's 670-07) and 671-79) has not yet added much to existing knowledge about the development process." The Sources II project, RPO 671-79, has only just started and it is hardly surprising that it has "not yet added much to existing knowledge ...". One minor quibble on chapter II, page 4. The model to be applied in RPO 671-79 is still called a "multi-sector programming model." The term is misleading since the computable general equilibrium model is not a programming model. There is no overall welfare function being maximized and programming methods are not used, even as a solution algorithm.

The discussion in chapter II, page 21, on general equilibrium models seems to me to emphasize the disadvantages with only a passing reference to the advantages. To give an idea of what bothers me, I would like to offer the following paragraph which would replace paragraph 2 on page 21 (beginning "It may be useful ...")

"General equilibrium models have obvious and important attractions as instruments of economy-wide economic analysis. They have the advantage of making explicit the interconnections among economic activities and the complex and not always immediately obvious relations between prices and quantities. The growing body of work with computable general equilibrium models, both in the Bank and elsewhere, indicates that they provide possibly the best framework available for exploring the economy-wide impact of policies which work through the price system. In particular, the Sources II project (RPO 671-79), which has only just started, will have as one focus the testing of this new methodology to see how well such models can replicate the historical experience of two or three countries. The models will focus on the impact of trade strategies including policy instruments such as tariffs, quotas, subsidies and the exchange rate. While such an exercise in historical analysis has never been done with a planning model before, preliminary work with a model of Turkey indicates that the approach may well be a very rewarding way to explore the relationship between development strategies and resulting growth and structural change. It is, however, still much too early to make any firm judgement about how useful such models might be in this context.

While the panel recognizes the advantages, a review of the past work with large-scale general equilibrium models indicates two disadvantages which, in the view of the panel, weigh heavily against them ..."

cc. Messrs. H. Chenery, VPD; B. King, DEDDR;
A. Stoutjesdijk, DEDDR, B. Balassa, DRCR

OFFICE MEMORANDUM

TO: Mr. Bela Balassa, DRC

FROM: E. Bevan Waide, ASNVP *EBW*

SUBJECT: Report of External Panel on
Industrialization and Trade

DATE: March 15, 1979

Thanks for the draft of the above. In general, I find it thoughtful, down-to-earth and sensible in its recommendations, both on the substantive content of proposed research and on the administrative arrangements. The ideas on improving dissemination and ensuring that research output does not run too far ahead of the Bank's capacity to absorb it are particularly welcome. The various recommendations are not, of course, fully worked out but I think they go far enough to enable us to consider and respond once the report is finalized.

I have one cosmetic suggestion, namely, that both Chapters I and II contain a mixture of description, discussion, observation and recommendations on both research substance and organization. All of these are stimulating but it is a little difficult for the reader to decide which are the main recommendations. It will help the dissemination of this report if it could be accompanied by a short summary of the key recommendations in point form of a length not exceeding, say, two to three pages.

One specific point -- on page 19 of Chapter I, para. (e), it is suggested that "innovative research within the Bank will require a strong commitment to protecting the time of research staff for the production of research". This gives the impression that researchers should be isolated at all times; clearly while undertaking research staff need that protection but the thrust of the rest of the Chapter is that researchers, and research users, should integrate their work on research design and research application as much as possible, and indeed researchers should themselves, on occasion, work with the operating departments in applying their findings. This particular paragraph could be misinterpreted as endorsing an ivory tower approach which I am sure is not the authors' intention.

OFFICE MEMORANDUM

TO: Mr. Larry E. Westphal, Chief-DEDND

DATE: February 9, 1979

FROM: Mariluz Cortes, DEDND *MC*SUBJECT: Review of the External Panel Draft Assessment
of the SSEs Research Project

Mr. Bueno points out that the preliminary analysis of the first Colombian survey shows that there are three areas where further thought is strongly required. These are:

- (a) factor productivity and specially in regards to the different combinations of capital and labor.
- (b) the overall question of technology selection, utilization and adaptation, as well as the learning process.
- (c) the overall effect of economic policies on the development of SSEs.

With respect to points (a) and (b), I think that it is correct that more thought is required. However, the preliminary analysis of the first SSEs survey in Colombia may not reflect the relative importance given to these issues in the Colombian surveys. This shortcoming is due to the fact that the paper was written before the first survey was completed. We did not have time to analyze the technology module in which data about factor productivity was collected at the shop level. We hope that the analysis of this data will illuminate further point(a). The issues of differences in factor productivity among firms of different sizes will be studied further in the survey of firms in the food processing sectors. The decision to choose food processing for the second survey was partly based on the expectation that in this sector it will be methodologically easier than in the mechanical engineering sector to calculate factors productivity.

With respect to technology selection and adaptation, the first survey in the mechanical engineering sector gave emphasis to the design capability of different firms (design of products and machines) and the type and sources of equipment. The latter resulted so important that we decided to carry out a parallel survey of importers of secondhand equipment. This survey is now finished. Technological efforts at the plant level in the area of production process will receive special emphasis in the survey of food processors. The difference in emphasis in both surveys is related to the different nature of the technological efforts carried out by firms in the mechanical engineering and food processing sectors. In the Colombian study we are also giving importance to point (c). Once we have the results of the surveys, we intend to relate what we know at the micro level to some aspects at the macro level, including overall effect of economic policies on the development of SSEs. This part of the study will be done in collaboration with Prof. Al Berry who has a very deep knowledge of the Colombian economy.

Mr. Bueno points out that the SSEs project lacks an "overall encompassing methodology." He thinks that this situation could heavily affect further work. Stated in this form the lack of an "overall methodology" seems to be a basic shortcoming. However, there were important advantages in the decision to give each researcher freedom to pursue their own interests. It has to be remembered that this is an interdepartmental research project and people in different divisions have different areas of interest. It was agreed that all the surveys should analyze, to some extent, the working of the different markets in which SSEs participate (i.e., factor markets, credit markets, product markets, and technology markets). However, the emphasis given to the study of specific markets, and the methodology used to collect and analyze the information, had to reflect the interest of the researchers in charge of each country survey. Thus, the Colombian study (carried out within the Economics of Industry Division) gives more emphasis to technology issues, whereas, the studies of India and Philippines (carried out by the Employment and Rural Development Division) give emphasis to finance and labor markets.

Once the advantage of the concept of "research umbrella" is accepted, the relevant questions are: a) how adequate are the methodologies used in the different studies, and b) how comparable will be the results of the different studies. It may be too early to answer these questions. However, with respect to the question about comparability, I think that the three surveys will have enough in common to permit us to draw a fairly complete picture of the conditions faced by SSEs in each of the markets in which they participate. This should be a good basis for structuring the second phase of the project. In this second phase we can take Mr. Bueno's recommendation of including a wider variety of countries in the study.

Finally, Mr. Bueno's summary of the objectives of the study are not correct. The trial surveys are not a mere attempt to define ways in which the information can be improved.

MCortes:mm

OFFICE MEMORANDUM

TO: World Bank Panel on Industrialization and Trade DATE: February 8, 1979

FROM: Larry E. Westphal, Chief-DEDND ^{WW}

SUBJECT: Comments on Drafts as of February 8, 1979

1. This memorandum serves mainly to transmit various items to you. First, though, I would note that you should have received my memorandum to Mr. Suman Bery dated January 30, 1979, with comments on the draft as of that date. By and large, the comments made in that memorandum still hold; however, those labeled as pertaining to Chapter I now pertain Chapter II.
2. Attached, in order, are the following:
 - a) Xerox copies of Chapter I and II, giving comments in the form of marginal notations;
 - b) Comments on various elements of Chapter III, in the form of memoranda from my staff who were asked to comment as they felt appropriate (note that not all elements of Chapter III are covered);
 - c) An edited version of Mr. Parikh's section of Chapter III (see the paragraph below).
3. As may be clear from my memorandum to Mr. Bery, we are quite unhappy with Mr. Parikh's section of Chapter III, for the reasons stated in that memorandum. What we have tried to do is edit his section only to correct factual errors. However, a comparison of the unedited with the edited version will undoubtedly show the latter to be somewhat more supportive of the research. This is because the edited version takes account of past work not mentioned by Mr. Parikh but which is in line with his recommendations for future work. I should note that even so edited we remain quite dissatisfied with this draft.
4. Finally, having now had time to reflect on the draft report, I would offer the following additional general comments. As regards Chapter I, with minor exceptions, I feel this is an impressive and well reasoned statement of the factors that should be taken into account when assessing priorities for research within the Bank. It constitutes an excellent foundation for laying down principles of research management. However, the Chapter lacks a concluding section summarizing its main points. Without it, the reader is left wondering at the end of the Chapter precisely what it is that he has learned; in fact he should have learned quite a lot, but it needs to be recalled to him.
5. Chapter II is not as strong as Chapter I; it does not consistently carry through the themes enunciated in Chapter I. A general point: readers are in various places "invited" to read between the lines; I am not sure it is a good idea to permit readers to draw their own conclusions from vaguely phrased nuances. I continue to be quite upset with the way in which skepticism

February 8, 1979

is expressed regarding general equilibrium approaches; in particular, the draft makes it appear that that this research is well underway, while this is simply not true, the project having barely began. I am also very concerned about all of section 3 of this Chapter, where I feel a number of important supplementary points are not clearly made or are totally omitted. For example, where application of past research is called for, much could be said regarding how such application should proceed, which could lend further support to the recommendation. I do not think this section is as supporting to the researchers now in the Bank as you might wish it to be. I would be happy to discuss this aspect point by point.

6. Finally, Chapter III is exceedingly weak. I suspect that it is largely the writing that is at fault, and expect that it will take considerable effort to achieve an exposition which would properly reflect the Panel's work and understanding, with which I have been very impressed in oral discussion.

LEWestphal:mmm

cc: Messrs. B. Balassa, DRC
S. Bery, VPD

Attachments

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.

Is type a
problem? In
past research?

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

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

I:1 Bank research - why and for whom?

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

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

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

good way of putting it.

It would seem that this approach to the issue of research priorities would follow from a research philosophy according to which the Bank, in the most efficient way possible, tries to contribute to the accumulation of research knowledge in the world as a whole.

Thus, the Bank would be regarded as a producer of "public goods" in the form of scholarly knowledge in the field of development economics. The choice of research priorities of the Bank would then be determined solely by the Bank's ability to produce research, and not by its internal demand for (use of) research knowledge.

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

- (a) The research of the Bank should concentrate on fields where particularly competent researchers are available within the Bank, or (in a longer perspective) can be hired by the Bank.
- (b) Bank research should exploit the skills and information that are acquired within the Bank in its operational activities as a lender and adviser.
- (c) The Bank should 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 research in circumstances where there has not been strong leadership exerted from researchers within the Bank. It would seem that projects dominated by "outsiders" have usually not succeeded as well as projects dominated by leading researchers permanently employed by the Bank. Perhaps open competition when choosing outside researchers could, in some cases, improve the quality of such research. In this connection we would recommend the Bank to ²void 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.

*wrong name
I would
prefer names
be omitted.*

*wrongly suggests
that researchers
don't learn
from being in
the Bank.*

One example is research on investment project evaluation. Studies of investment projects that are relevant for several countries at the same time may be particularly suitable for the Bank, as compared to other (usually national) organizations. Another example of a field where knowledge that is useful for research activities is acquired by Bank operations is probably 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 intermediaries, for instance within the Industrial and Finance Division (IFD). Ongoing analytical work within the Bank on trends and problems on international markets and on national economies could also be a foundation for more systematic research efforts within the Bank.

*I don't find
this as
objectionable
as does Falk.*

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.

yes

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

It should be noted that these areas all also have in which Bank has CA under (b)

Define more clearly

Please see the ITRSG report

"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.

This may too much omit questions of technology? Depends on how interpreted.

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?

OK

I am not sure that it covers information, in schools of thought, etc.

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

I would like some evidence across other organizations - eg. large consulting firms.

This depends on the organizations strategy to access to world-wide research! is needs to be tried to raise in section 1.2.

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

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

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

- (a) a comparative advantage approach;
- (b) an ambition to function as a residual supplier of research;
- (c) an attempt to create externalities within the Bank in the form of "sophistication" among Bank staff; and

non sequiter - seems to assume necessarily implies using LDC researchers.

Here's what I might get a 2nd research sponsor - this open competition in the Bank.

(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-

excellent way of making several points.

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.)

*I seriously
question
that this
is so*

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).

*Role of CPS
versus research
not clearly
spelt out.*

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 ^racts" papers, an issue to be discussed in connection with the problem of "dissemination of research" (p. 19).

*This principle
appeals 1st for
relying heavily
on staff* ←

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.

*Strongly
agree*

It is possible that research in these "new" fields sometimes would gain on applying rather inter-disciplinary knowledge and methodology. Examples of such fields are research about entrepreneurship, innovation and technological development.

For an important question in these fields is under what general conditions (policies, institutions, systems of incentives and perhaps also cultural characteristics) entrepreneurship, technological advance and innovations are likely to flourish. Studies of the determinants of "work ethic" is another topic that may require rather broad inter-disciplinary knowledge and methods of analysis.

Agree

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.

*Another reason
to that given
previously to*

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.

AMEN!

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

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

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

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

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

Thus, for good research to be made, researchers should not, in our views, only do the things that are immediately relevant to operating officers, or which these think is immediately relevant.

On the other hand, it is also obvious that it would be fruitful if researchers were better informed about the usefulness of research knowledge among operating people, and if researchers could communicate more effectively with operating officers.

Unfortunately, there is no easy way out of the conflicting ideas about "enclave research" (free from disturbances) on the one hand and strong interaction between researchers and operating people on the other hand. We have to be satisfied with "uneasy" compromises between these conflicting objectives. Keeping this in mind, several reforms suggest themselves.

- (1) That the researchers write, and circulate within the Bank, popularly written reports on research - concerning research produced both inside and outside the Bank. 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.

What is the
 level of interest that
 the state of things
 is so bad. Perhaps
 it is merely a
 reflection of
 normal tension
 PLUS terribly
 overworked
 staff and
 human capital
 run down
 by Sr. Mgmt.

What we already
 do in this
 regard is
 not read
 or used

(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

AMEN

Here CPS ✓
DPS

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

Very
true

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 ^(a) 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

I am not very much persuaded that this follows.

*ok -
tell I
see it
does.*

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 ^a thought, if at all, in conventional static microeconomic investment calculations. With this approach, studies of investment planning in some sectors would be of rather high priority. It has in fact also been so for the Bank in recent years (see section III:6), in the sense of "investment programming" - mainly in cases where the optimum size of a firm is of about the same magnitude (or larger) than the entire national market. Another reason why sector planning models, or at least investment programming models, may be of interest is that in many countries, perhaps in particular in less developed countries, a number of infrastructure and process industries, for which investment programming models may be particularly useful, are in fact under rather detailed central government control.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.

*I would
add:
policy
analysis*

*very
good*

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 ^{? word choice} 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

The Bank has done much in this area outside research confined to general research. Eg. Alter Report.

Work of
Finance
Division,
et. al.

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.

*What about:
initial
conditions;
resource
base;
etc.*

Σ - This is an excellent discussion reflecting what an intelligent approach to ~~the~~ formulation of research priorities should consider. It needs, however, a summary to recall for the reader what he has just read.

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.

A 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, ^{which} ~~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.

B
Another cluster of projects has been concerned with comparative advantage, trade patterns, and economic growth. These projects include RPOs 670-07 (International Model),

670-19 (Expansion in Manufacturing for Exports in Developing Countries), 670-79 (Economic Development of East and South-east Asia), 671-05 (Patterns of Industrial Development), 671-32 (A Comparative Study of the Sources of Industrial Growth and Structural Change), and 671-79 (Sources of Growth and Productivity Change), and two non-RPO studies as well. The unifying theme is the objective of explaining the pattern of resource allocation within and between countries, economic growth, and changes in industrial and trade structures as a function of various country characteristics, including policies employed. While two projects (670-19 and 670-79) were clear failures, the other ones met, by and large, this objective. The failures are regrettably indeed, since the projects addressed the questions of how to shift manufacturing activities from developed to developing countries and of how to plan competitive export industries in the developing countries; information on these matters would have been an extremely important ingredient of any effort to shape rational industrialization strategies. The projects differ greatly in their methodologies. Some of the projects involve modeling that is very simple or indeed primitive; others involve attempts to empirically implement a very complex general equilibrium methodology. In the view of the panel the most comprehensive of the projects were those that employed and elaborated an accounting framework based on sector-specific supply-demand identities for analyzing the nature of modern industrial growth. The projects that worked within a less formalized framework did not yield interesting conclusions. The research guided by the more ambitious general equilibrium conception has not yet added much to existing knowledge, nor has it produced sensible empirical results to an extent which could not have been obtained at lower cost from simple macro-economic formulations. [The operational staff does not attach a high utility to this type of research.] And it is not clear

unless I could not pick out which projects are being referred to.

Hell - has barely started

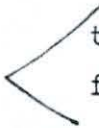
Moreover, it is wrong in particular cases - eg Turkey mission work

→ This is a separate matter - of Chap 1.

if this is good.

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

Evidence for this please



C

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 ^{reviewed} 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

more than 5 must have been done

*not so -
eg. Key Institutions
A premise
on this
hypothesis -
does not
test it.*

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

17
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.

very good point.

E

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.

CAVEAT:
 Work to complete this research (i.e. writing) and should be supported.

The staff wholeheartedly agree.

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

F

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

What does this mean? What negative aspects are being referred to?

Yes

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-

*This invites
one to
read
II:1 between
the lines -
perhaps more
than you
would
initially*

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.

*might be well
to be more
specific re
what fruit
should be
found.*

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 ^{which kind?} this kind of research may in fact have been more influential regarding decisions on particular loans than the ^{i.e.?} more general analyses done by Bank researchers. However, if the focus is on the influence of Bank research on the way Bank officials view appropriate economic development policymaking and set their positions in bargaining with LDC officials, as stated above we believe that it is the more sweeping ideas and documentations for these that has had the greatest influence.

We feel ourselves in an even weaker position regarding the ability to judge the impact of Bank research on policymaking in the developing countries. A real impact could be recognized with regard to the studies on incentives and domestic resource costs in industrial and agricultural activities. For the other projects, we would conjecture that all of our remarks above obtain. Where (and it is certainly not everywhere) the research done at the Bank has had influence, we suspect this has been largely through affecting the general climate of thinking, and through its effect on dialogue between the Bank and government officials of developing countries. But we are able to acquire very little direct

I don't understand what is being said here.

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.

*relevance?
of this
at this
point*

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.

I agree with both points

?
is the spectrum notion is there, as where on the spectrum

II:3 Future research priorities

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

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

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

- (a) To contribute to the research knowledge in the world about the development process; a "comparative advantage approach" is then adequate.
- (b) To improve upon the research knowledge that is needed for Bank operations and policies; a "residual supply of research approach" is then adequate.
- (c) To create externalities within the Bank in the form of "sophistication" among Bank staff.
- (d) To help generate research knowledge and research capacity in the LDCs.
- (c) To create externalities within the Bank for its operational and policy formulating staff in generating a more analytical view of the problems and an increased level of "sophistication".
- (d) To help generate research knowledge and research capacity in the LDCs.

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

different kinds of research, Bank needs and LDC needs for certain kinds of studies to enhance their decisionmaking ability, the kind of research that is likely to attract and hold excellent scholars at the Bank, and the kind of research most amenable to cooperative endeavors between the Bank and LDC institutions.

well put

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.

not very well put.

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.

Then Chap III has to be much better written.

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.

do you have in mind the specific RPO or a general area including several RPO's

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

*unless what
otherwise
refers to.*

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.

*+ object -
see my
memo of
1/30/79*

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.

*Agree - as
long
as it is
recognized
that some
involvement
by researchers
will be
needed
2) researchers
finish writing
up their
existing
work.*

Vital

~~It is essential that~~
Agree

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.

What about assistance to LDC's using these

Agree

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.

Again - I object to this strong statement

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.

Eg. Wetzel

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:

what remains here that is being rec'd that is not already in progress? need to be more specific

- (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 - *cf. Leiserson*
- (4) Comparative studies of government policies (that influence industrialization and trade)
- (5) Industrial strategies in non-industrial LDCs

as research, or applications

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

how differs from work on labor capital substitution

I would be much stronger than perhaps excellent reason.

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.

Re adjustment at post I agree - but what about research on means to promote restructuring?

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.

My point is that adjustment assistance may be too narrow a focus.

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

I am missing the rest.

Now I have the rest!

- 20 -

Agree

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

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

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

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

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

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

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

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

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

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

The clearly legitimate research on factor market distortions has not been intelligible with that on industry

Here it is very unfortunate you have not seen the Bank's research - of which there is "quite a bit" in employment and factor markets. R.A.P. & report - maybe others.

but perhaps more relevant to understand how policies affect innovation.

what does this mean more specifically?

*I would
use more care
in stating this -
ex. 1st. I would
completion of
research of existing
of existing research
etc. a task*

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.

*I am
already forced
to this is disruption
of all work as
our completion
RMCN as
too rapid forces
implementation
of this rec.*

*would be
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more
strongly.*

*See the
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report
with
which
I strongly
agree.*

Evaluation of Bank Research on
Programming in the Manufacturing Sector

1. The Nature of the Research

A number of research reports and monographs are expected from the research program carried out under the heading "Programming in the Manufacturing Sector" (RPO. No. 670-24). The list of reports/draft reports which were studied for this review are given in annex. 1. The program has dealt with the problems of investment planning in industries characterised by increasing returns to scale and in industries where interdependence in the production of different products is important. Interdependence may be important when different products share capital equipment or when they use the same intermediate inputs, the manufacture of which may exhibit economies of scale. It has focused on development of improved methods for selecting investment projects from among the many alternatives in size, timing, location, technology and output mix. In addition, it has investigated the extent to which such interdependence affect project selection and planning for the development of a sector and offer scope for co-operation among the countries of a region.

These issues are explored within the context of specific investment planning problems in two sets of empirical studies, one set dealing with what is termed as "process industries" and the other with "non-process industries". Those industries characterised

by a manufacturing process stream which is more or less continuous, has a limited number of processes and where the cost of carrying mid-stream intermediate products is large are termed process industries. Examples of such industries are gas transmission, fertilizer, cement, etc. These industries also have a limited number of products which are more or less uniform.

The "mechanical engineering sector on the other hand has a variety of products and processes and the same processing equipment can be used for manufacturing many different products. Such industries are termed non-process industries".

Both these sets of studies have used mixed integer programming models in a fixed charge formulation to account for economies of scale.

The major problem that has been faced in previous studies,

~~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.

The research program under review has developed a

~~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 ^{sometimes} not significant at the sector level. For the part of the mechanical engineering sector of Korea that was studied (120 carefully selected products), this would have led to an increase sector-wise total supply cost of no more than 3 percent of the total value added for the products involved in the study.

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

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

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

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

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

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

2. Review of Studies

2.1 Process Industries: The studies carried out for the planning of the fertilizer sector in Egypt and in East Africa have explored the choices of technology, size, location, transport, product and trade. The East African study has in addition explored in quantitative terms the gains from co-operation in fertilizer sector development for the three countries of the region (Uganda, Kenya and Tanzania).

The formulation of the models for the fertilizer sector is ~~not conventional and~~ straightforward ~~in mixed-integer programming~~ terms, though the inclusion of substitution among products marks an important innovation. 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 and recognized limitation of the models as developed is the neglect of uncertainties. Explicit accounting for it ^{would} ~~might~~ make the computation problem even more formidable. ~~Yet it would be~~ Therefore, the researchers stress the use of ~~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 Asian Region and FAO's World program; Clinker production for Brazil, ~~Petrochemicals for Portugal and Mexico~~ and Energy for ~~Nigeria~~ and Chemicals for Turkey. Nigeria; other studies are underway.

An attempt to use this approach to develop a model to quantify the benefits of regional integration based on a simultaneous analysis of a number of industrial sectors, the pros and cons of which were succinctly presented in a small paper, was abandoned as being too ambitious.

The project has obviously been successfully carried out and as judged from the number and variety of applications it has been found useful too. Sometime ago, ^{the} ~~the~~ project has reached a stage where research ends and applications begin. But further effort is required to ~~the technology developed has to~~ *successfully disseminate and transfer the methodology for use in practical applications.* ~~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. *The recently initiated GAMS project (No. 671-58) should go far toward providing the necessary software.*

~~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 *analyzed*. ~~andogeneous~~ 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

The mechanical engineering sector is not easy to ~~model~~ ~~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 should not be substituted for the more thorough analysis of the type undertaken in the study.*

~~advantage, though they do not discriminate unambiguously between make-buy choices, are adequate and entail a small loss compared to analysis which account for interdependence.~~

It is ~~also claimed~~ ^{noted} that the ^{estimated} 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 "Appropriate Industrial Technology" (RPO 671-51) are such projects.

3. Evaluation

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

3.2 Usefulness for LDCs: Large programming models ~~and particularly economy wide programming models~~ create an impression that the vision behind the process of development that motivates such studies is one in which an elite all knowing planning authority attain economic growth through effectively allocating resources to various sectors. Yet one need not share this vision before one considers such models to be useful. Process industries such as fertilizers, cement, etc., characterised by economies of scale and relatively a small number of plants, are the industries which are usually the ones whose development are guided and promoted by most

governments of developing countries. Starting a few large industrial projects is one of the easiest thing that governments do to promote developments. The planning models developed by Bank's research has the potential to improve the rationality of government decisions in developing these sectors. Some of this potential is already realized in the numerous applications already made for different sectors and different countries.

However, the full potential usefulness can be realized only if adequate "extension work" follows this Bank Research. We shall return to this later.

3.3. Usefulness for Bank

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

Part of the work on the fertilizer sector plan for East Africa might have been made irrelevant by the subsequent political development in East Africa and the break up of the economic union. Even then the non-cooperation solutions could still have been useful to the policy makers in the three countries.

The Bank may have a unique comparative advantage in carrying out studies such as the fertilizer study for East Africa that identify areas for regional cooperation and which facilitate the process of realizing such cooperation. As an authority which lends money to the various countries of a region, it may have access to data and policy

makers in the various countries. Moreover, as a third party its analysis may be less suspect. On the other hand Bank should also be interested in promoting such cooperation that reduces need for credit in the region.

3.4 Development of Research Capacity in LDCs

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

~~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~~ make such participation difficult. understandable. ~~And yet the outcome is regrettable.~~

The ~~Thaich~~ country

specific sector studies undertaken to date have involved

~~should involve~~ participation of local ~~persons,~~ persons.

But, an

~~to~~ effective

programme has ^{yet} to be designed to facilitate participation of researchers from the LDCs, to ensure that

~~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~~

~~to which case studies have been carried out,~~ there would be ~~any~~ established the

capability to either update and/or improve the particular sector study

or to carry out a similar study for another sector. ^{1/}

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 low cost and to what extent this reflects on the difficulties of skill transfers is anybody's guess.

[Not relevant since no RPO 670-24 case studies undertaken as yet for Korea. The request is for such case studies.]

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.

The manuals currently under preparation are vital to facilitate extension, but their availability constitutes only one element in the process.

~~Writing manuals accessible to technical persons, even when the manuals are written to be accessible to non-specialists, is only a beginning.~~ Short training courses would also be inadequate. What would be required is a case study for a sector, which is carried out with an active participation of a local team or better still, a local institution. It may even be desirable that the studies be carried out by a local team. Such studies may be co-ordinated by Bank staff who are familiar with such research work, and should certainly be financed by the Bank. Even when the success rate of such research is not high, it may be considered a necessary investment in building up research capability in LDCs. Such support should also include provision of computer hardware in the case of many LDCs. A sector study based on MIP models needs convenient and substantial access to fairly large computers if the study is to be completed in reasonable time.

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.

The foregoing is not to deny the usefulness of attempts at extensions through previous case studies, but to recommend that more resources be devoted to extension, and that the case studies have as their primary objective effective extension.

(b) Programming models to Evaluate Appropriate Technology
for Rural Industrial Development:

The models dealt with in this research can be used to ~~and yet, with some additional efforts, one could have used~~ ^{to} ~~these models~~ throw some light on issues of considerable interest in ^{should} LDCs. To what extent ~~could~~ one develop a decentralized industrial structure? What are the costs of a rural based industrial development? What is the implication for employment and income generation of development based on small scale industries? These issues need to be explored in a systematic, technical and dispassionate way. ~~The models~~

The case study of pulp and paper in Malaysia examines some of these issues, while the follow-on work under RPO 670-23 has a great deal of relevance to understanding questions concerned with the organization of production. ~~developed to measure the benefits of largeness should be of use to~~
~~measure the costs and benefits of smallness.~~

Nonetheless, with the foregoing exceptions, the ~~The~~ studies in industrial programming under RPO 670-24 have ~~viewed~~ tended to view the problems of development in a purely technical way. The objective has been to find least cost solutions. Institutional issues in the organization of sectors, in the difficulties of implementation, or in the realm of selection of policy instruments ~~have been beyond the scope of these studies.~~ should be brought to the fore in subsequent research.

In evaluating the appropriateness or otherwise of technology not only relative factor scarcities should be taken care of but also the limitations of public policy in using certain instruments. Thus if income redistribution policies are politically hard to pursue, one might lay an emphasis on income generation in selecting "appropriate" techniques.

(c) Institutional issues in Implementation and Capacity Utilization:

Though the Bank research in this area of industrial programming has been useful and has indicated benefit in excess of costs, one may still ask if these are the most important issues that need to be researched for promoting industrial development.

The problem of implementation of projects and the efficiency with which even large industrial projects are operated in many LDCs are perhaps of much greater consequence than the selection of an optimum set of projects. *MIP models can be used to examine the efficient operation of existing plants, as was done in the Egypt fertilizer case study. Nonetheless, the issues here go much deeper.*

To what extent delays in installation of capacity and inefficient use of installed capacity in large industrial projects are due to improper organisation or due to inappropriate objectives of the management or due to inexperience and inadequate skills? Are delays and inefficiencies inherent in the organisation of the public sector, which plays a large part in the development of industrial sectors in some countries? Does the private sector really perform better? These issues need to be examined. The potential gains of being able to design effective organisational and institutional frameworks are enormous. Moreover the Bank would have a comparative advantage in carrying out research in this area as it would be able to pull together experience from a number of countries with a wide range of characteristics.

(d) Consequences of Interdependence for Planning and Promoting Industrial Development: If economies of scale and interdependence are important then the development of that industry would benefit from some form of central planning or coordination. On the other hand, effective implementation and central planning of a sector such as the mechanical engineering industry characterised by a large number of products and processes is difficult to say the least.

Thus the findings of the Korean Mechanical Engineering Industry study that interdependence appear not to be ~~is not~~ of much quantitative consequence ~~is~~ are very comforting as one can rely on implementable, decentralized procedures for developing the industry. Yet, as emphasized by the authors of the research, the findings need to be confirmed with further ~~research~~ research; in particular, theirs is the only

study that has investigated the issue empirically. Moreover, as they are at some pains to point out, there is reason to suspect that the aggregation approach used may bias their findings. Would a different set of international prices such as may prevail at other times lead to a different result? Would different domestic prices, such as may be found in another country, give a different result? Would a different product-mix produce contrary results? Is it possible that under such different circumstances the benefits of accounting for interdependence would be highly significant say 50% or more of the value added in the sector?

The quantitative dimension of effects of interdependence is sufficiently important for policy purposes that further explorations to test the generalizability of the Korean Mechanical Engineering study are called for.

OFFICE MEMORANDUM

TO: Mr. Larry Westphal, DEDND

FROM: Mariluz Cortes, DEDND *MC*

SUBJECT: Nelson's Review of World Bank Projects on Capital Utilization, Capital-Labor Substitution, and Technological Change

DATE: February 7, 1979

I have only two comments with respect to Nelson's report. In his recommendations Nelson points out that the Bank has been reluctant to do research on issues of innovation, entrepreneurship and institutional structure, because they are viewed as not amenable to rigorous research (because precise models and econometric techniques cannot serve as the primary tool.) If this is true, the Small Scale Enterprises Study is a major departure from this orientation, and he should be aware of this. Nelson also points out that the part of the project on appropriate technology that explores the design capability of domestic capital goods producers has considerable promise. His argument could be stronger if he mentions that as part of their mission work, researchers in this department have gathered evidences of a highly developed design capability among Argentinian producers of capital goods.

OFFICE MEMORANDUM

TO: Mr. Suman K. Bery, VPD

DATE: February 8, 1979

FROM: Kemal Dervis and S. Robinson, DEDND

SUBJECT: Report by World Bank Panel on Industrialization and Trade

This memorandum gives our comments on the draft of chapter 3 of the report, especially the discussion of RPO 671-79 (the Sources II project). We agree with Westphal's comments on the chapter and only wish to expand on them with respect to this project.

First, there are three minor points of clarification in the description of the project. The draft refers to Yugoslavia as the third country study. As the project was finally approved, a third country study will not be started in the first year and it is not clear what country will be chosen. Second, Sherman Robinson and Kemal Dervis are jointly directing the project. Third, it might be better to refer to the models which are to be used in the historical analysis as Computable General Equilibrium (CGE) models rather than as "programming" models since our approach is based on direct solution of the system of non-linear equations and does not involve the use of programming methods.

Page 7, paragraph 3

Price endogenous general equilibrium models have never so far been used as part of an historical analysis of growth and industrialization (with the exception of the very aggregative work of Kelly, Williamson and Cheetham). There is therefore not really any accumulated experience in this area. There is, on the other hand, by now a fair amount of experience with forward-looking models. A common criticism has been that they have "too many degrees of freedom" and have not been "tested" against real historical development. An attempt at historical analysis would therefore be a step in assessing the explanatory power of general equilibrium models. We do not want to overemphasize this point. The data problems as well as problems relating to behavioural specification remain very substantial and in the light of the difficulties faced and the energy that went into developing the purely demand and quantity oriented input-output studies it would be a bad mistake to underestimate the effort needed for a simultaneous and policy-oriented analysis of quantity and price structures.

We believe however that a start can be made and that some questions, particularly those relating to trade policy (tariffs, quantity rationing, exchange rate policy, terms-of-trade) can benefit from being analyzed in a general equilibrium modelling framework. The experience we have had in building a model of Turkey for the EMENA region has strengthened our belief in the usefulness of applied general equilibrium models in analyzing trade and exchange policies. This is not surprising since trade theory has long

constituted the most fertile area of application for general equilibrium tools. More than in other areas, quantitative general equilibrium work on trade problems has a rigorous and rich body of theory to draw on.

Page 7, paragraph 4.

The last paragraph on page 7 raises the problem of interactions between social, economic and political phenomenon. These interactions are extremely important. A type of quantitative modelling that attempts to endogenize this interaction, as suggested by Bacha, is a most interesting challenge. But we think that it should be presented not as a substitute for but as a complement to models of the economic mechanism as such.

In this context, note that the "conflict" between value theory à la Hicks-Malinvaud and the Sraffa-Pasinetti type models developed in Cambridge (England) may not be as great once capital stocks are treated as combinations of produced means of production (as is done in most applied general equilibrium models); the wage in organized industry is treated as exogenous; and the fiction of futures markets is abandoned.

Page 9, last paragraph.

In the section on future research, Bacha states that some work on CGE models should go on at the Bank in order "to keep the WB researchers in contact with the state of the art in academia." One should note that almost all of the initial work on CGE models for developing countries was financed by the Bank and much of it was based there. The Bank has been a leader in this field and may well have a comparative advantage in future work in this general area.

Regarding the amount of resources required for research using general equilibrium models, it should be emphasized that there is a real and discrete choice to be made.

Demonstration models that further develop our understanding of the structure of particular general equilibrium interactions or that allow systematic exploration of explicit optimization procedures are not costly research tools and methodological work with demonstration models could proceed on a very moderate budget.


The situation is however quite different when one is talking of serious applied country work. The whole point of such efforts disappears if careful empirical work cannot be done. This usually involves greater need for research assistance, travel costs, in-country consultants and/or collaboration with local research institutes and, in general (particularly when the aim is comparative work) greatly increased coordination costs. A further difficulty arises because the uncertainty regarding timetables and completion dates increases substantially when reliance is placed on local consultants due, in many cases, to the inherently unstable working conditions in many developing countries.

We believe that for the World Bank and local researchers the benefits may exceed the costs. Be that as it may, we hope that the nature of the choice and the implications for resource allocation in the Bank receive enough attention. The distinction between demonstration models and applied country work should be brought out in the report.

OFFICE MEMORANDUM

TO: Larry E. Westphal, DEDND

DATE: February 8, 1979

FROM: Demetrios Papageorgiou, DEDND SUBJECT: Review of External Panel: The Donges' Report

I agree with your comment of this report in your 2/2 memorandum. I think the report on Incentives and Economic Integration needs extensive redrafting. I found the same mix-up about economic and financial measures, for example, in para. 10, page 7 where Donges has confused measures of incentives with measures of comparative advantage.

Furthermore, Donges' comments on the usefulness of the "strategies" project seems to me to be a little misplaced. The importance of this project has always been thought to be its contribution to our understanding of the effects of incentive policies on the structure of industrial activity, rather than in providing timely policy recommendations.

Finally, I do not understand why the "Export Promotion" (RPO 67-135), the Morawitz, and the Baldwin-Waelbrook project have not been part of Donges report. Had he discussed them, very few interesting research ideas on trade would be left in his report. His first recommendation, if I understand it at all, is nothing else than what has been done so far with the incentive studies and domestic resource cost calculations. His second recommendation for research is at best vague. The third recommendation is in many ways addressed by RPO 67-135 and the Morawitz project. His last recommendation for research in trade is partly covered I believe, in the Baldwin-Waelbrook project.

OFFICE MEMORANDUM

TO: Messrs. Kim Jae-ik and Assar Lindbeck,
 Research Advisory Panel on Industry & Trade

FROM: Don Keesing *DBK*

SUBJECT: Draft Review of Research on "Export Promotion Policies in the LDCs and Access to Markets in DCs"

DATE: February 8, 1979

1. I have only been given a few minutes to read and comment on this draft before leaving on a two-day trip, and I appreciate the positive things said on it.
2. The first two "projects" discussed on pages 2-4 are not research studies and were never meant to be more than a "useful jumping off point for serious research" (in the words used on p.2). The third ~~is not a separate study~~ at all, but only a reporting study written in less than a week as an update on what has been happening; it was the third in a continuing series. Mrs. Plesch did a recent one less than a month ago. An example of a somewhat more intensive reporting study (forthcoming as a Bank Staff Working Paper) is attached, along with a similar paper done for a conference in Sweden. What needs commenting on here is the (non-research) function of keeping track of what is happening and reporting on it as a guide to Bank policy and operations, and how this may relate to research. I believe there is a potential for a positive feedback on research from our policy and reporting studies since they help to show what issues are important, and give us a general quantitative framework; but they should not be judged as research, for they serve very different purposes.
3. The overall evaluation on p.10 is confusing, at least to me, in evaluating a category called "surveys, data compilations and analyses." Contrary to what is said in the first sentence, we have no research project in this category. What little has been done, by only one or two people, has been an attempt to digest what is going on, based on easily available statistical sources. I am also confused by the overall message here: supplementing existing information sources in any major way is far beyond the realm of feasibility.
4. The basic problem in our trade work is that, unlike most Bank country missions where we have several people, here we are trying to do a big job with practically no staff. We cannot possibly keep up with other people's work, let alone do proper research (apart from hiring consultants) with the one or two people we have. This should somehow be reflected in the draft. There is also a need to balance research against operational and policy needs. We are almost unbelievably shorthanded on all these fronts.

Best wishes and have a good stay in Washington! I'll be back on Monday.

Attachment - given to Prof. Lindbeck.

cc: Mr. Westphal

DBK/jf

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

File: 10+1
Panel.

OFFICE MEMORANDUM

TO: See Distribution

FROM: Larry E. Westphal, Chief-DEDND ^{lw}

SUBJECT: Review of External Panel Drafts

DATE: February 2, 1979

URGENT -- Comments must be returned by c.o.b Wednesday,
February 7 or they will not be taken into account!

1. The External Panel reviewing Bank research on Industry and Trade has made available a draft report, consisting of two general chapters and a third "chapter" containing more detailed reviews of research in particular areas. As the Panel returns on Feb. 9 to complete its final draft, now is the time to review drafts for factual errors so that these can be corrected in the final draft.
2. I have read and will be rereading more carefully the entire report. Attached is a set of preliminary comments, where I tried to flag the most serious problems, but not to comment on more general points. I am asking that selected staff separately read and comment on the detailed reviews of particular areas. The attached table shows assignments, and the review you are to comment upon has been attached.
3. Be particularly careful to check for factual errors or possible errors of interpretation. For example, is forthcoming work explicitly mentioned or is it neglected: are recommendations made that fail to recognize work already undertaken; etc.? With respect to such errors, it would be best were you to underline the relevant parts of the draft and provide substitute text (with accompanying commentary if desired).
4. Where you have difficulty with judgements of merit or recommendations regarding further work, do not provide substitute text, but merely comment. All comments, substitute text, etc. are to be placed on a separate memorandum, keyed in some way to the draft which should also be returned.
5. I must have your comments by c.o.b. Wednesday to permit me to go over them prior to discussion with the Panel on Friday. I will not look kindly upon tardiness -- your interests are at stake!
6. Please contact me if you have any questions re your terms of reference or the format for comments. Please do feel free to check with other staff to insure factual correctness.

LEWestphal:mm

Attachments

Distribution:

Ms. Cortes, Mr. Keesing, Mr. Papageorgiou, Mr. Robinson, Mr. Stoutjedijk, DED

cc (without attachments): Mr. B. Balassa, DRC
 ✓ Mr. S. Bery, DPV
 Mr. B. B. King, DED

Review of
Chapter Three Items

<u>Author</u>	<u>Area</u>	<u>Responsible</u>
Bacha	Comparative Advantage, Trade Patterns, Economic Growth	Robinson (consult with Dervis)
Bueno*	Small Scale Enterprises, etc.	Cortes (consult with Anderson)
Donges	Incentive Policies and Economic Integration	Papageorgiou
Kim	Export Promotion Policies in the LDCs and Access to Markets in DCs	Keesing
Nelson	World Bank Projects on Capital Utilization, Capital Labor Substitution, and Technological Change	Cortes
Parikh	Programming in the Manufacturing Sector	Stoutjesdijk (process industries) Westphal (non-process)

* This item not yet available. Will be forwarded immediately upon receipt.

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

January 30, 1979

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

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.

1.4 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).

Steering Group
Documents

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1015 file

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APPROPRIATE DISPOSITION		NOTE AND RETURN	
APPROVAL		NOTE AND SEND ON	
CLEARANCE		PER OUR CONVERSATION	
COMMENT		PER YOUR REQUEST	
FOR ACTION		PREPARE REPLY	
INFORMATION		RECOMMENDATION	
INITIAL		SIGNATURE	
NOTE AND FILE		URGENT	
REMARKS:			
<p><u>For meeting February 9, 1979 - Rm. K3700</u></p> <p>Mr. B. Balassa Mr. H. Fuchs Mr. R. Gulhati Mr. B. Waide Mr. L. Westphal Mr. R. Richardson Mr. F. Moore Mr. D. Weigel Mr. D. Gordon <u>Mr. S. Bery</u></p>			
FROM: Michel Devaux		ROOM NO.: A-1148	EXTENSION: 7-5081

EXTERNAL PANEL ON ECONOMIC INDUSTRY AND TRADE

1. In the absence of Ravi Gulhati I have been asked to review the material distributed for discussion at the meeting to take place tomorrow afternoon. I have read the draft of Chapter I and Chapter II with interest, but I have not had a chance to see a draft of Chapter III, and I do not know to what extent my comments should be taken into consideration.
2. I wonder if Chapter I and Chapter II should not actually be two different documents. It is a little bit disconcerting to go through 28 pages discussing various aspects of the research in the Bank before dealing with the subject itself.
3. The discussion of the role for research in the Bank is a very important issue. Defining it more clearly will resolve a number of ambiguities and would eliminate controversies which, as indicated on page 13, result generally from the different weights various staff members put on various principles. The effort to clarify the purpose and the motives for doing research in the World Bank and the four principles identified for the choice of research are commendable, but the report should have made the distinction between fundamental and applied research.
4. One of the recommendations of the report to improve the dissemination of outside research is to hire more people in operating positions with research background. It is correct that the most effective way to import research is by importing it "in the head" of people rather than by reading documents. However it is less obvious that research oriented people should be given management or operational responsibility. There may be differences in the characteristics and qualities attached to a researcher than the one attached to an operations man. The recommendations may improve the quality of research in the Bank or stimulate the interest for research but it is not evident that this will improve the quality of Bank operations. I concur that joint ventures are appropriate to get researchers and operations people to better understand each other.
5. I have some doubts about the recommendation that more state of the arts papers should be prepared and discussed. Outsiders may not have enough knowledge of which data are available and which are not for a particular country. A correct analysis presupposed adequate data and all countries are not justifiable of the same type of analysis. Insiders on the other hand might lack objectivity and may be concerned more about superficial remarks than by an in depth review on how one specific subject should be dealt with. It is most likely the state of the arts paper will result into a set of normative recommendations or a statement of the inadequacy of the current analysis by academic standards.
6. The report recommends that the Bank should make a special effort at attracting researchers of various backgrounds. This is highly desirable. Diversification in philosophy and background is certainly an avenue to improve the quality of the advice and the services the Bank can give to its member countries. Even if very few member countries are centrally planned (none among the developed countries) it does not mean that all economists in those countries share their government's official views.

7. The rate of skill depreciation is probably very high in the field of methodology and technical research but this should not be sufficient to rule out sabbatical leave. It is supposed to give the operational staff a chance to recycle and to refresh their technical knowledge in light of their experience and their new field of interest.
8. Another way to stimulate interest in research in the Bank will be to associate more operational staff into the discussion of the research topics to be supported by the research committee. It might be useful to ask some of the country economists to sit on a rotation basis as a member of the committee. The committee will have a more concrete perception of their needs and they may be able to make more positive recommendations. At the present the work of the research committee is perceived by many in the operation as a luxury for a few DPS staff.
9. Chapter II is more specific and more directly related to the purpose of the panel, and I understand summarize Chapter III. The two together could constitute this panel report.
10. Six major streams of research undertaken in the Bank on trade and industry have been identified. It could be useful if direct reference could be made when discussing the overall quality of Bank research in the different fields. It should also be illuminating to know why in certain areas the research has been more successful than in others.
11. The panel acknowledges the difficulty it met in assessing the influence of Bank research on Bank decision making or on policy making in developing countries. Was this topic not exceeding the competence of the panel members and the scope of the panel.
12. Since basic needs were mentioned in Chapter I, I am surprised that this is not included in the recommendations for future research. It should be quite desirable to review industrial strategies compatible with a basic needs development approach and to analyze the role of industry in such orientation.

Michel Devaux

February 8, 1979

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 ^{more} ~~some~~ 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 application 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

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

Table 2

Professional Staff Needs in Central Units^{1/}
(professional man-years)

	<u>Existing Staff</u>			<u>Proposed Staff</u>		
	<u>Research</u>	<u>Other</u>	<u>Together</u>	<u>Research</u>	<u>Other</u>	<u>Together</u>
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
	<u> </u>	<u> </u>	<u> </u>	<u> </u>	<u> </u>	<u> </u>
TOTAL	<u>10.5</u>	<u>9.5</u>	<u>20.0</u>	<u>12.0</u>	<u>18.0</u>	<u>30.0</u>

^{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)

	<u>Pre-FY78</u>	<u>FY78 on</u>	<u>Total</u>
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	-	-	-
Entrepreneurial 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
<u>Total</u>	<u>38.7</u>	<u>61.3</u>	<u>100.0</u>

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 allotted 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

	<u>RPO Research^{1/}</u>	<u>Other Research Activities^{2/}</u>	<u>Total</u>
A. Industrial Organization/Management	<u>12.3</u>	<u>5.2</u>	<u>17.5</u>
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	<u>4.8</u>	<u>1.7</u>	<u>6.5</u>
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	<u>1.6</u>	<u>3.3</u>	<u>4.9</u>
1. Industrial Financing	1.6	1.5	3.1
2. Extension Services	-	1.8	1.8
3. Entrepreneurial Development	-	-	-
D. Trade in Manufactures	<u>1.8</u>	<u>15.9</u>	<u>17.7</u>
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	<u>27.7</u>	<u>5.6</u>	<u>33.3</u>
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	<u>2.9</u>	<u>.8</u>	<u>3.7</u>
1. Project Programming	2.9	.2	3.1
2. Project Appraisal Criteria	-	.6	.6
G. Industry Studies	<u>-</u>	<u>16.4</u>	<u>16.4</u>
H. Country Studies	<u>-</u>	<u>-</u>	<u>-</u>
Total	<u>51.1</u>	<u>48.9</u>	<u>100.0</u>

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 3Staffing of Research-Type Activity
Average for FY78/9: In PercentDevelopment 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
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<u>Total</u>	<u>100.0</u>
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* 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, footwear, 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 SIEs (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 entities 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 be 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., GAMS); 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.

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 Stern 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 dimensions. 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 *RG*

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/Strecten/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 statist 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

[Faint, mostly illegible text, likely bleed-through from the reverse side of the page. Some words like "Members of Executive", "and Trade", "National", "Members", "and" are faintly visible.]

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.

AttachmentDistribution

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
FROM: E. Bevan Waide, ASNVP
SUBJECT: Interim Report of Industry and Trade Research Steering Group

DATE December 22, 1978

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 leaven its work on applied research with some "gap-filling" pure research.

DWilliams/HPilvin:hh

Distribution: Messrs B. Balassa, H. Fuchs, R. Gulhati, F. Moore,
R. Richardson, E. B. Waide, Ben King,
L. E. Westphal, E. J. Stcutjesdijk, M. Selowsky