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Dates: 1/1/1977 – 12/31/1978

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

Digitized: 8/13/2019

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R.A.P. Income Distribution and Employment (RAPIDE)



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WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. Balassa

DATE: May 2, 1978

FROM: Michael Beenstock

SUBJECT: RAPIDE Report: Final Version

Following the comments in response to my memo of March 14, 1978, I attach Prof. Fishlow's final version of the report.

Attachment

cc: Messrs. Ahlu

Ahluwalia Chenery Choksi Duloy

Karaosmanoglu

King Leiserson

Yale University New Haven, Connecticut 06520

DEPARTMENT OF ECONOMICS

37 Hillhouse Avenue Box 1972 Yale Station April 10, 1978

Mr. Michael Beenstock The World Bank 1818 H Street, N.W. Washington, D. C. 20433

Dear Michael:

Enclosed is what I hope is the final version of the report. You undoubtedly appreciate that I am even more pleased by this eventuality than you. I would ask you to take just a few more hours to read through the draft one more time in search of obvious grammatical or other infelicities. I did not use the totality of your contributions because as you can see from the enclosed, they are somewhat replicative. I do not think the same points should quite exactly be made both in the text and the appendix. I trust you will forgive me.

I recall as well that you were going to identify the research projects in Table 1 that originated in the Employment Division. It might also be a good idea to set the Table apart as a separate page in order not to interrupt the flow of the text.

I want to thank you again for your cheerfulness during this long and somewhat tedious process. I hope there will be further occasions when our interaction can be more substantive and interesting.

How did the Yale visit go?

With best regards,

Sincerely yours,

Albert Fishlow

AF:g

Enclosure

Introduction

The written work of the Bank in the fields of income distribution and employment is extensive. In the last several years, as Bank objectives have become redefined, a large part of its activities have focused on these problems. Country reports, policy papers, and project evaluations all contain elements of research in these areas. In interpreting its mandate, the Panel has necessarily concentrated upon the activities of the Income Distribution Division of the Development Research Center and the Employment and Rural Development Division of the Economics Department. Time simply did not permit us to cast a much wider net. The research of these units are quite extensive by themselves and cover a diversity of topics that are representative of Bank priorities. Such a perspective has had the advantage of enabling us to consider in more detail a range of issues centering on the relationship of the formal research program with other Bank activities.

One part of our task has consisted of critical evaluation of the research product. Three specialized appendices provide these detailed reactions. Here, it may be simply said in summary that our general impression of the quality of work is quite favorable. In an activity in which risk is high and disappointments are always to be encountered, the Bank accomplishments — compared to other research organizations and academic institutions — by and large meet rigorous professional standards. Our principal caveat is the failure to afford sufficient priority to the collection and processing of basic data on a consistent and continuous basis.

These evaluations have also helped to provide a basis for suggestions

and recommendations regarding future research priorities and procedures.

We offer these fully aware of our limited perspective and partial knowledge.

We believe they may nonetheless serve to stimulate the internal discussion that is merited. The Bank research program is an important component of its activities, and can make significant contributions to its operations.

This is especially true of research on such central topics as income distribution and employment. The Bank's lending operations require systematic knowledge of how particular kinds of projects can generate jobs and lessen inequality. The Bank's consultations with foreign governments require a comprehension of the underlying economic processes at work and their responsiveness to different kinds of policies. The Bank's leadership role within the development community provides an opportunity to speak on these issues both with authority and influence; it also imposes the need to understand the distribution and employment problems in depth.

The body of this report is organized around a series of recommendations for future research priorities and for procedural changes to facilitate them. There are seven parts. The first considers the emphasis appropriate for Bank research. Our suggestions derive from our perusal of the research results thus far obtained in the study of income distribution and employment, but also have a relevance for other subject areas. The next three sections illustrate specifically how such a reorientation of research objectives helps to define a promising agenda for the collection and analysis of data, and for the study of income distribution and employment. The final three sections relate our recommendations for changes in the research process to improve the articulation of the research program with other parts of the Bank, to make more effective the dissemination of results, and to make more efficient the selection and evaluation of research projects.

I. Research Objectives

Research within the Bank as a formal activity is of relatively recent vintage and still modest proportions. External spending for the last few years has been at an almost constant \$2.5 million (in constant FY '77 dollars), and has declined as a percentage of total current expenditure. Allocating staff time committed to research projects and adding other outlays increases the absolute level to only about 3 percent of expenditure, and does not alter the trend. Income distribution and employment have together commanded about a fourth of that budget between 1974 and 1976. In the last fiscal year, as the initial research program has been progressively completed, its share fell to less than 10 percent.

The objectives of the Bank research program have from the beginning been ambitious. As identified at its initiation, and reiterated subsequently, the goals have been far ranging:

- (1) To support all aspects of Bank operations, including the assessment of development programs in member countries;
- (2) To broaden our understanding of the development process;
- (3) To improve the Bank's capacity to give policy advice to its members;
- (4) To assist in developing indigenous research capacity in member countries.

The intended audience for research results has been correspondingly diverse: the policy and operating staff of the Bank; policy makers in developing countries; and the international research community.

The choice of such a broad perspective for Bank research has been

conscious. "It has sometimes been suggested that we should concentrate more on the Bank staff as the major concerns of our research output.

However, since research that is valuable for internal use in the Bank will almost by definition be of value to others concerned with development problems, we have concluded that most research projects must be addressed to all three groups."

. .

This approach has been characterized by extensive use in the research program of outside consultants and a willingness to undertake longer-term, more basic analyses of the development process. It has meant accumulation of a technically highly qualified staff. It has lead to an increasing volume of publication in externally referred professional journals. It has, moreover, successfully established an academic integrity for the Bank's research program, an accomplishment of some proportions: Bank research does not hew to a single line or insist upon a single point of view.

But such a broad mandate has also produced an independence from operational concerns that is worrying. Table 1 provides a classification of research projects in the fields of income distribution and employment by their inspiration. Category I encompasses projects which have been instigated outside the DPS. This research can be said to service a felt need emanating elsewhere in the Bank.

Category II contains projects originating in the DPS, but characterized by significant discussion and collaboration with regions and relevant operating departments of the Bank. Category III includes research initiated and pursued by the DPS more independently.

It is clear from the Table that the majority of projects — and even more so the expenditure — fall in Category III. Relatively more — but

still a minority — of the employment studies are in Categories I and II than was true for income distribution. This in part reflects the Employment Division's greater operational responsibilities, but also in part the Income Distribution Division's more complete and comprehensive research design that it early on justified and progressively implemented.

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Autonomy in the initial design and execution of Bank research makes more difficult its subsequent articulation with other parts of the Bank. That is a problem to which we advert subsequently, and one to which the researchers are not the only contributors. It also tends to encourage original and theoretical research that may be more responsive to conceptual needs of the profession than policy requirements of the Bank.

The comparative advantage - and for the most part, needs - of Bank research reside in the following:

- (1) Establishment and analysis of a basic Data base

 The Bank needs to know, on a consistent basis,

 what has and is happening with regard to underlying

 development processes in order to conduct its

 affairs. Its frequent missions, country offices,

 and position in the United Nations structure in

 turn afford significant opportunities.
- (2) Project-related inquiry

Bank lending operations pose specific economic questions with regard to effectiveness of alternative design that call for focused and relatively shorter-term research in order to be answered.

(3) Policy-related inquiry

Bank consultation with individual countries
centers around policy packages of one or another
kind. The Bank and country positions are
influenced by what is known about the effectiveness
of such policies in different contexts. Academic
research devotes limited resources to assessing
what has happened in particular countries and why.

(4) Development strategy

The Bank's role in the development community — and its unique comparative, international perspective — requires it to take a position on questions of larger magnitude: the workability of trickle-down vs. Basic Needs, for example.

It is our sense that the research program in income distribution and employment, while of high quality, has not focused narrowly enough upon these requirements and opportunities.

Such is the case with the creation of a data base. The research reveals a deep-seated ambiguity in the Bank attitude toward the importance of data. At one level there is strong commitment to empirical analyses as a means of discriminating among alternative interpretations and views. At another level, however, there is acceptance of information from other, and sometimes non-primary, sources with only minimal attention to reliability, comparability and continuity. The Bank compiles, issues and consumes an impressive volume of statistics; at the same time, it seems to refuse to take serious responsibility — intellectual and financial — for their accuracy. Specific research designed to improve the quantitative record

relating to income distribution and employment illustrates this ambiguity perhaps more than other subject matter.

In similar fashion, project-related inquiry has relatively little weight in the present scheme of formal research. In part its modest role reflects an intentional division of labor within the Bank, reserving to the DPS more fundamental inquiry, and allocating to the CPS more operational studies. In part, its limited importance is also a requirement imposed by the level of resources available to the DPS; the research budget is a modest one. Yet the potential significance of such research within the Bank context requires that more conscious priority be given to it by the professional research staff. Two aspects should be stressed. One is the design of research to exploit the quasi-experimental access to information operating projects afford. The other is direct assistance to operational staff in coping with project design.

Commitment of large, additional resources need not be involved for the latter. The DPS could offer to the operating departments on a regular basis an initial diagnosis better to define the practical problem, and recommend outside consultants qualified to pursue it in direct collaboration with the operating divisions. Budget for such consulting is already available; part might be internally allocated to the DPS to recompense their contribution. Recurrent problems that had a more general character could in turn serve to stimulate a genuine medium-term research effort within the Bank. Its objective would be to devise an appropriate approach and set of tools that Bank staff could regularly apply in their preparation and evaluation of projects.

Policy related inquiry has an equal, and perhaps more important claim upon the research budget. Just as project-related advice and research

provides a direct link to the operating departments, a policy related emphasis responds to felt regional and management needs. The formal separation of the preparation of Bank policy papers from the research process has had the unintended effect of diminishing the incentive for design of research projects whose final contribution might be a policy paper. Rather, research has come to be defined as long-term to the exclusion of more focused and medium length efforts. The process of project submission and review also virtually guarantees that even interim results will be a long time in coming. It does not encourage adaptation and application of existing conceptual research; the researcher's own biases, particularly if there is interest in a subsequent non-Bank career, are thereby reinforced.

Yet such policy-related inquiry is of the essence for the Bank. It can provide a firm basis for its hortatory influence upon domestic policy within developing countries. It can influence perceptions of which individual country lending programs should be supported to a greater or lesser degree. It can translate the specific projects undertaken by the Bank into a larger and coherent influence upon economic policy and resource allocation within particular countries.

A rich experiential basis for such inquiry exists. Particularly in the areas of income distribution and employment, it is difficult to imagine policy instruments that have not already been used in one place or another, at one or another time. Such research also especially lends itself — as does a more serious data collection effort — to greater collaboration and institution building within the developing countries themselves. Advice that has its roots firmly grounded in local inquiry and analysis is more likely to be taken seriously.

This point merits further discussion. It is fair to say that among the diverse goals the research program has set for itself, an impetus to indigenous research capacity in developing countries has been most honored in the breach. This has not happened entirely for lack of trying. Two of the principal, data-oriented income distribution projects have been contracted to regional agencies, for example. Still the focus of research attention upon a few developing countries — Colombia, Korea, Malaysia among others — stands out. They are selected not on a scientific or systematic basis, but largely because such research capacity is already present, and cooperation is easier to obtain.

Institution-building is costly, especially in human resources, and cannot be effectively pursued as a by-product of other goals as at present. Consultancies will have a natural tendency to be concentrated in the industrial countries, and even in the United States, because the necessary supervision and desirable interaction with the Bank staff are easier to assure. Solicited and unsolicited suggestions for projects will come forth from a circle restricted by the asymmetry of information about Bank research interests and procedures. Particular country targets therefore must be identified as such; the specification of the research projects themselves must come subsequently. They will depend upon the indigenous research capacity and professional interests initially present, and that can be mobilized. No less than the projects themselves, the means of developing indigenous capacity requires careful planning and continuing monitoring.

Closer collaboration admittedly presents complicated problems of freedom of academic inquiry and publication. Governments and academic research institutions overlap to a considerable degree in developing countries. Inquiries involving income distribution and employment are

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sensitive and political matters — involving not merely different socioeconomic groups, but frequently also racial and regional divisions. The
Bank has an obligation to press for dissemination of research results as a
condition for participation, just as it has an obligation to insist upon
objective and professional standards of inquiry. Some countries may refuse
to collaborate on such terms; others will be eager for assistance and
recognize the advantage of research rather than advocacy.

Despite the additional costs, financial and other, we believe the goal of supporting research in developing countries deserves greater attention and planning. If one ultimate objective of the research program is to influence developing country policy makers, it is difficult to imagine its realization absent a more sustained set of initiatives to encourage research in the countries themselves. A more explicit policy focus to research—and one less emphatic upon conceptual originality—can readily assist. Those problems are more compatible with the skills and interests of the developing countries.

Beyond the direct institution building benefits of greater research interaction, we detect and emphasize another. Research collaboration with developing countries is a two-way relationship. There are diverse ideologies of development being pursued today in a variety of countries. The implications of these different models for income distribution and employment is at the heart of the matter. Continuing Bank intellectual contact with developing countries, rather than a relationship through the exclusive medium of missions and project teams, could mean a research program that better responded to the non-orthodox, mixed economic systems that are the rule in much of the world. It could also expose a facet of the Bank of which many developing country academics and students are unaware.

This lengthy digression is a measure of the importance we attach to the desirability of a sustained effort to realize the Bank's potential contribution to developing countries. It is a means of going beyond the research itself.

In similar fashion, adapting the research output to make it relevant for the larger questions of development strategy is less a separate category or style of investigation than an integrating principle. Increasingly, the Bank is called upon to take positions about how development should be pursued, internationally and by its member countries. The Bank has spoken out forcefully to call attention to the widening inequality and inadequate employment opportunities associated with sheer income growth. The Bank will undoubtedly be called upon to do so again, if only to give greater substance and structure to the evolving consensus in favor of development assistance oriented toward satisfying basic human needs. These statements should emanate from a cumulative and technically informed view of both underlying development processes and responsiveness to policy interventions. They therefore require a synthetic view of Bank and outside research. It might well be useful to have the relevant research divisions prepare at regular intervals a user's rather than a producer's review of the literature on the larger issues. While the Bank has indulged and financed many good and useful reviews on employment and income distribution, they have been largely directed to a professional audience. What we have in mind is a review that goes beyond to draw implications for Bank policy and development strategy. Such reviews should anticipate management needs and help set an agenda for more specific policy papers. Such reviews in turn could help to define new Bank research projects to test the validity of the conclusions and to question the conventional wisdom.

Our suggestions do not involve a radical reorientation of the priorities of the present research program. Some have been the ones the Bank itself seems to be moving toward. Taken together, and implemented coherently, the recommendations may provide a useful basis for focusing more precisely where the Bank ought to be going. The next three sections provide more concrete directions.

II. Data Collection, Processing and Analysis

We recommend a greater initiative and expenditure by the Bank to assure collection and dissemination of reliable, consistent and continuous information on income distribution and employment.

The Bank has played a prominent role in publicizing and using extant information on the size distribution of income in a large number of developing countries. It has also sponsored two large regional projects — one in Latin America, the other in Asia — to exploit survey data already available but not uniformly organized. These efforts fall short of what is desirable and necessary because the primary concern has not been upon the data themselves, and because the projects are not part of a continuing effort over time to monitor changes as development proceeds.

We understand the reluctance of the Bank to accept prime responsibility for such an undertaking. It is neither cheap in resource cost nor easy to execute. Yet we emphasize its importance if the Bank is to make well-informed and reasonably based judgments concerning the most fundamental income distribution and employment issues. At the present time, it is frequently difficult to say whether conditions have improved or deteriorated in a given country, let alone precisely how. Accumulation of instantaneous data in a number of countries is not a solution. Cross-section studies are

not an adequate basis for formulating "development laws," or even more modest projections of the positive and negative forces operating on the income distribution in specific countries.

What holds for income distribution holds with equal force for employment statistics. The Bank currently relies extensively upon information obtained directly from member countries through missions, and upon statistics of the ILO. How such data compare with those of previous missions is often not a matter of concern. Neither is the quality and selection of price deflators that have a central importance in the determination of whether real wages have increased or declined. The dominant practice seems to be, when the issues are addressed, to use as much and as diverse information as possible and to hope for modal consistency.

Such a casual view — perhaps understandable in the immediate pressure of producing a country report or a policy paper — is inconsistent with longer term requirements. As an institution, the Bank can afford an investment whose social yield is extremely high but farther off in the future; no individual or academic institution can have that luxury. There is a large pay-off to the Bank in reducing the present duplicative and inaccurate procedures. It would be nice if others, like the statistical office of the United Nations or the ILO undertake the required effort. The intellectual requirements, particularly at the start of such an undertaking, militate against Bank delegation of the responsibility. Country statistical offices cannot be expected — even with technical assistance — to perform the required tasks alone.

We therefore see considerable merit in a decisive Bank contribution to the design and execution of the 1980 Census that all countries will be undertaking in the very near term. Bank participation need not be universal.

The choice of a reasonable panel of countries, based not upon the ease of data collection but their representativeness, is a feasible means for establishing benchmark measures of the distribution of income.

The concepts utilized in such a project should include the size distribution, organized not merely around individuals but also households. But there should also be attention to relevant socio-economic groupings — urban service workers, rural landless landlaborers, etc., cross-classified by other characteristics. To the extent that individual incomes at any point are influenced by a series of random efforts that alter individual positions without affecting their long-term, permanent stream of income, the size distribution may be a misleading indicator. It will also fail to draw attention to the full implications of structural change and mobility as groups change in size and composition.

Such information on socio-economic groups is a more direct link with the production process than are data in the size distribution. This makes them easier to obtain, accurately, and on a continuing basis over time. It also facilitates the construction of economic models that more naturally relate to these groups than the individuals and households that serve as the basis of the size distribution. We therefore encourage and endorse the on-going work at the Bank to estimate such social accounting systems. What is required is a much larger and more systematic approach toward collection, organization, and testing of the data than has been characteristic in the past.

In parallel fashion the Bank must concern itself with developing reliable series on unemployment, wages, skill differentials, price deflators, etc., not for single years, but over time. Whether in cooperation with the ILO or not, the Bank cannot afford to wait for others to take the initiative.

Research at the Bank too frequently is conceived as testing hypotheses and using data. It is time to elevate the production of the data to equivalent status because of the present parlous state of the field. A conscious and continuing effort by the Bank would have large pay-off for the total research done in the subjects of income distribution and employment, in the Bank and out.

III. Income Distribution Priorities

We have identified three priorities for future income distribution research. They are compatible with the evolving interests of the Division, and build upon its previous analyses.

In the first instance, we see an opportunity and a need to utilize the data relating to the levels and changes of income of socio-economic groups. Empirical studies of the behavioral characteristics of these groups should be a fruitful area for investigation. Some of these characteristics are traditional-like consumption demands; others have been less explored — like the substitution among different classes of workers in production; and still others have been largely put to the side — mobility among groups and regions.

These characteristics, among others, are the essential building blocks for models of the distribution process. Understanding the factors that influence behavioral response is a prelude to meaningful abstraction into more formal structures that link these responses. To some degree the process has been reversed in some earlier Bank research. The premature construction of large-scale models that are uncomfortably rooted in behavioral relationships has been a factor in making that effort less productive than it might have been. There is still much to learn about

the demand for labor by small sector industry, services, and peasant agriculture that constrain efforts to ameliorate the income distribution. The appeal of modern goods and their influence upon style of development and distribution patterns in rural areas is likewise not a topic that has been exhausted.

Such empirical investigations are required for a wider range of countries, and not merely as a means for determining the best parameter estimate for one or another model. The hypotheses are of interest for themselves. They tell us how groups are likely to respond to changes in income, prices, wages, etc. They tell us whether income change alone is sufficient to satisfy basic consumption requirements.

Of special importance is closer attention to changing composition of socio-economic groups over time. Equal measures of static inequality give rise to far different implications in societies of high and low mobility. Temporary circumstances of individual poverty can be compensated by transfer, private and governmental. Permanent conditions of deprivation are another matter. Much of the debate about a basic needs strategy implicitly assumes that provision of such goods and services will enhance mobility. This assumption can be converted to testable hypotheses.

Socio-economic mobility is not an easy matter to pursue. Longitudinal data are not abundant in developing countries, particularly for the rural and informal sectors. What we have is limited to a few surveys. There is more that can be done in the formal urban sector, however. Most countries now have social security schemes and hence data on individuals over time. The importance of the mobility issue commends a major effort to define a research project on the subject.

A second thrust for Bank research ought to be empirical analysis of

the impacts of particular policy interventions. What makes income distribution policies more difficult to implement than most is that the initial consequences get diluted by subsequent market interactions: a rise in minimum wages may limit employment opportunities and contribute to price increases, for example; public basic services may be traded or underutilized. There is therefore a need to systematically pursue these and other policies in a comparative framework to trace their ultimate impact.

Such partial analysis frequently runs aground because the second-order effects should ideally be studied in a general equilibrium context. Yet there are many cases where one can stop short of looking at the whole economy to understand what has happened. It is that difficult, but creative intermediate research that we wish to encourage.

The policy interventions that could be studied are diverse. They range from provision of social services, food programs, monetary stabilization programs, minimum wage legislation, diverse Bank lending priorities, to the consequences of the more complete integration into world trade and capital markets of recent years. Such research has obvious and direct ties to Bank policy, both in its own design of projects as well as in its recommendations for member country domestic policies. Such research must be comparative and benefits the unique circumstances of Bank access to information in a variety of countries. Not least, such research is of direct interest to the developing countries themselves.

Such issue focused analysis runs the danger of missing the cumulative impact of a series of interventions that add up to an over-all strategy. Its natural complement, therfore, is a series of comparative case studies organized around country units. That is the third proposal. The countries

should be carefully chosen, for representativeness. Close collaboration with local researchers should be established, and a start on collection of basic data established.

The usefulness of such country studies for policy purposes should perhaps be underscored. The now changed perception of developing country opportunities to benefit international trade has been influenced by exactly such comparative national studies, one pioneer effort undertaken by the Bank itself. That had the advantage of a natural measure, effective production, around which the studies could be grouped. What must be sought are similar organizing principles to prevent mere catalogues of policy measures and unstructured speculation.

These specific research priorities give substance to our earlier recommendations regarding desirable emphases for the Bank program as a whole. They are not projects as they stand; they require more precise formulation. It is our sense they will repay the effort to do so.

IV. Employment Research

Our recommendations for priorities in employment research are, not surprisingly, closely related to those for income distribution. They also generally conform to those of the Division. They may be grouped again under three categories.

First there is a need to know how labor markets work in developing countries: the institutional parameters that constrain market forces, the relationship of enterprise characteristics to labor demand, and the determinants of labor supply. These factors together result in a set of differential wages and levels of employment in different occupations and sector that vary among countries. These differences are what are of interest.

Of greater absorption of labor is an objective of policy, a starting point must be understanding how labor markets have thus far performed on their own.

Such markets exist in urban areas for the formal and informal sector, and in rural areas for agricultural and non-agricultural activities. They are very different. Except for the most rudimentary (and sometimes incorrect) distinctions — like characterizing the informal sector as the market which wages clear — these differences have not been much studied. Instead assumptions have been made to facilitate construction of simple employment models.

There is scope here for studies that disaggregate the so-called informal sector in order to determine which activities expand with higher income, and the sensitivity of employment opportunities to wages paid.

The characteristics of the internal labor market — the recruitment, training and promotion within firms — also call out for study, particularly in the middle income countries.

Our second category is a logical counterpart to the focus on employment opportunities. That is more careful and consistent measurement of unemployment and the systematic study of those who are and remain unemployed. In many countries it is almost a matter of sheer guess-work to assess the degree of unemployment and underemployment. Inconsistent definitions over time and among countries inhibit meaningful comparisons. The composition of these groups are not known with great precision. Nor is there much information about how they are financed when they are not working.

Policies to promote employment are, as a practical matter, policies to create jobs for many who are currently without work or underemployed.

Their effectiveness necessarily depends upon better knowledge about who such persons are. Policies to reduce the high costs of unemployment to those without earnings depend upon similar information. We are not advocating mere compilation of statistics. In most countries policies of both kinds to deal with unemployment have been tried. It is important to find out what has been successful.

The third line of research activity to be encouraged is more specifically project-oriented. High in priority here are studies that trace through the indirect employment effects of different types of projects. For example, does lending that establishes a low capital input per job ultimately mean more jobs? Under what conditions will such a proposition fail to hold in rural, and in urban, settings? This research benefits from the data generated by the projects themselves.

A second component of project-oriented research should be technology focused. Such Bank work already has been undertaken in civil construction in the design of techniques that are more suitable for labor abundant economies. One should go beyond describing outcomes to research designs that are more comprehensive. The small scale industry research being undertaken should be tied to the projects actually implemented.

Project-oriented research has not always fared well because it has sometimes not been regarded as sufficiently conceptual, and rather too much a practical matter. It is a mistaken view. In the first place, understanding practice is not a mere matter of description. An economics, as well as an engineering perspective, is required to be sure that relevant interactions are not missed. In the second instance, the experimental opportunities afforded by project-related research involve considerable conceptual talents.

Research on employment in the Bank is complicated by the designation of the ILO as the primary U.N. institution in this area. This creates overlaps of responsibility, particularly as regards data, and imposes delays. The Employment Division also has had a far-ranging set of responsibilities that have made it difficult to concentrate upon a related series of research projects. The priorities we have suggested may help to focus interactions with the ILO, as well as to bring together the efforts of what is after all a very small number of Bank staff.

V. Articulation

Research at the Bank has among its prime objectives an influence upon Bank operations. Indeed, many would regard it as the principal goal. For that very reason, the relationship of research to operations has not been an easy one within the Bank. Operational expectations of a proximate feedback are inevitably frustrated by the larger perspective of research, the Intermediate rather than final character of much of the output, and the generic inability to meet close deadlines. The small size of the research staff relative to operational capacity means a limited in-house capacity to take on specific problems for solution. It also means an inability to perform the research directly. The role of consultants is therefore large; in the early phases of the research on income distribution, it was unusually so. These consultants are not on the scene to explain and motivate their research, or to mold it better to the interests of others. They interact to a limited degree with Bank staff outside of a restricted research circle.

There has been a conscious decision, moreover, to protect research against the all consuming demands of immediate problems. The Income Distribution Division, for example, was sheltered from operations by its

creation in the DRC to facilitate its getting on with the rather considerable task of initiating research. This has been tantamount to the creation of a small research center within a larger institute for development economics.

One of the consequences has been a lively and congenial atmosphere in which to work — involving more personal interaction than seems typical of the Bank as a whole, and an independence that is an important element in creative scholarship.

Elsewhere in the Bank, however, such an arrangement is looked upon with a somewhat jaundiced eye. It is interpreted as freedom to pursue more esoteric interests. This lack of shared values reflects itself in the style and extent of intellectual interaction. Despite an institutional structure governing research that has sought to be supportive of contact, and a variety of requirements regulating consultation, there is not much effective collaboration — as the earlier discussion revealed. We have found expressions of disappointment regarding the usefulness and relevance of the research, and little enthusiasm: laissez-fair seems to be the modal view.

Conversely, among the researchers there is also some dissatisfaction with inadequate attention to, and appreciation of, their own efforts.

Users in the rest of the Bank are not entirely innocent victims. As far as we can tell, it is not standard practice for those with regional and operating responsibilities to examine research reports with care, still less to return comments, even when the focus is one's country, or functional specialization. The typical reason is lack of time. It is not wholly acceptable or convincing; it is just another way of saying that such interaction has low priority. On the user side, such neglect is symptomatic of the larger pressure to get on with projects, or the required country reports, memoranda, etc. Relevant research in this sense is viewed not

merely as nonproductive, but potentially anti-productive. If particular views must be re-thought and old patterns altered, the same pace of project formulation cannot be maintained. In some instances, it has even been suggested that irrelevance is therefore preferred to more applicable analysis.

That undoubtedly is a caricature. Still, the reward and incentive system does not seem to encourage individual initiative and novelty. Indeed, in the case of one country desk of above average performance, it was standard practice not to retain any of the earlier research reports in the files. Each country economist was presumed to start from scratch, without knowledge of on-going work relating to the country.

Nor has there been an impressive impact at the management level.

Research reports have not regularly and systematically fed into the policy papers that regularly form their basic agenda. Rather it seems to be more typical for other, and sometimes parallel, investigations to inform the management on such income distribution and employment questions like urban poverty, basic needs, etc. To be sure, the research staff has sometimes made significant contributions to such considerations by attaching memorandum growing out of their own perceptions. But these interventions would be more effective if they were regular and an outgrowth of the formal research.

The lack of articulation between research and operations is therefore not a simple problem. It can be somewhat ameliorated by creating larger and more varied working groups from different parts of the Bank, and by improving dissemination. Resolution of the problem goes deeper. It goes to the heart of how operations proceed and policy decisions are taken. It is influenced by the perceptions of potential users that the knowledge generated is not a useful adjunct to their performance.

One can perhaps make a useful start by focusing on the preparation of policy papers as the culmination of the research process, rather than as its initiation. There will be obvious exceptions — as when the research is explicitly methodological. Designing such an end point as the product of a defined bloc of projects might make a considerable difference.

VI. Dissemination of Results

Communication of findings is a central part of the research process. As the research program has proposed, the Bank has broadened its audience in recent years. Publication of articles in professional journals by Bank staff has been encouraged; books regularly appear with Bank sponsorship; working papers in their preliminary form are sent to a series of recipients (outside the United States), in developed and developing countries both. The increased external dissemination of Bank research results is both a significant accomplishment of the research program, as well as a measure of its high quality.

Despite this success, and indeed partly because of it, we recommend a new departure. That is the publication of a new Bank Staff Papers analagous to those of the Fund. We believe such an outlet is a necessary and powerful incentive to guide research along more policy applied lines as we have earlier recommended. So long as research staff must aim for academic journals as a primary outlet for their published efforts, their research will almost inevitably be academically slanted. Publication is essential for research staff to retain their professional identification and mobility. It is important to have a vehicle that remains of high caliber but whose principal focus is development practioners concerned with problems more than concepts.

We perceive a second advantage. Such Bank Staff Papers would be a medium that is not exclusive to research personnel alone. Other Bank staff could and should contribute as well. Such an opportunity will have a subtle but powerful influence upon the articulation problem discussed above. Novelty and originality will be given explicit reward and encouraged. More

abstract research results adapted to particular applications will have an intellectually respected outlet. Differentiation between economists working elsewhere in the Bank and those specifically engaged in research would narrow.

The arguments against such an innovation seem to be two-fold. One is cost. This does not seem binding. The I.M.F. Staff papers, published three times a year, have a net cost of \$100,000 annually, after full allowance for personnel time allocated to editorial effort. That would represent only 5% of the present Bank external research budget, and less than 2% of total expenditures for research. This must be regarded a modest expense to reach on a regular basis some 3000 subscribers, and many more readers through access in libraries.

The second argument is that such a Bank publication is premature at best. Some feel that the quality of Bank research, especially as broadly defined, can not guarantee the regular flow of results required. Reliance on outside refereeing, as at present, assures automatic enforcement of standards; internal decisions, coupled with the pressure to produce continuing issues. Such an assessment cannot be taken lightly, particularly when journal publication has been subject to exponential proliferation.

We do not concur, however. It is precisely such a narrow criterion of research, and the corresponding academic measure of quality, that the Staff papers are designed to remedy. Something intermediate between Finance and Development and decentralized publication of Bank research results seems an appropriate and powerful medium for encouraging a distinctive style and focus to the research program.

The issue can presumably be settled at little cost by establishing

a proto-editorial committee within the Bank to select from among the current annual flow of working papers, country reports, policy papers, etc., representative issues. These can then be evaluated. Such a test excludes the beneficial incentive effects that the new publication should have. This modest effort seems well worth undertaking. The potential impact upon the research program and upon the professional activities of the Bank as a whole goes beyond an enlarged audience.

With or without a new journal, external dissemination of Bank working papers, some which will perhaps not be published at all, can be extended. At present University centers in the United States are excluded. There seems no good reason why. The number of institutions abroad receiving the papers on a regular basis might also perhaps be increased, or at least greater publicity as to their availability. The intended effects on developing country research and research institutes do not seem to have occurred; there does not seem to be great and regular reference to parallel Bank research in the work done abroad.

One additional limitation of present communication with developing countries should also be pointed out. Infrequently is there any post-research contact with countries that have been studied. As a rule, research results relating to a specific country, or group of them, ought to be formally presented there. This, too, even without direct collaboration, is a means for influencing developing country policy as well as research priorities and methods. Country reports presently are the occasion for careful consultation; the same should apply to research. The Bank research effort will remain small relative to the capacity for local investigation in all its member countries. Its activities therefore must have a catalytic

component, one that is not being fully exploited.

A ready extension of this principle further suggests the usefulness of sponsoring Bank seminars abroad more widely. This has already been done on occasion. New techniques and research results could be presented in an effort to stimulate local projects. In some cases regional meetings might be held; in others, individual countries might be more appropriate. Such a broader program could counteract to some extent the concentration of research upon a limited number of countries. Thereby it contributes both to the institution building objectives of the research program, as well as exposes Bank researchers to a greater diversity of developing country views and experience.

The second aspect of dissemination is internal to the Bank. The present system relies primarily on written rather than verbal transmission. Seminars in regional departments or operating divisions are infrequent. There is evidence that current practice is not entirely adequate. Two mechanisms suggest themselves.

The first is preparation of a new series of reports in a special format for non-DPS staff, in addition to the working papers themselves.

These could take the form of a relatively short -- but more than an abstract -- statement of the research results within a broader context. It would largely spare technique in favor of drawing out the implications for country application and policy explicitly. These statements would be widely distributed, reserving for a narrower audience the original research publications themselves that would be available on request.

A record initiative would be an expanded system of Bank seminars. It could be standard procedure that every project -- as it is being elaborated

and even prior to submission for research committee approval -- be the occasion of a special seminar in the relevant region or operating department. Seminars that are presently scheduled during the research project cycle are largely directed to Bank researchers themselves. What is proposed is an out-reach to potential users. At the conclusion of every project, a similar presentation geared to consumers could discuss the results and motivate their possible application and extension.

Care and attention to internal communication can ameliorate, if not resolve, the problems of articulation. It can expose end-users more satisfactorily to what researchers are doing. It can also increase the gratifications to researchers by providing them a Bank audience. That is one of the reasons the researchers themselves ought to redraft their papers and present the seminars. No new layers of specialist translators is called for.

VII Research Administration

The administrative structure governing the Bank research program has evolved considerably in the last several years. It has impressively responded to felt inadequacies of project preparation, supervision, and evaluation. That makes it unnecessary to dwell at length upon the subject. More attention has increasingly been paid to pre-Research Committee panels drawing together diverse interests within the Bank, to the phasing of projects involving large expenditures, to the need for adequate internal supervision of consultants, and to the standardization of the final evaluation process. We dare say that many of our earlier suggestions have already been anticipated.

To some degree, our present recommendations changes in the process

react to what now seems perhaps an excessive formalization. Research projects are considered in an NSF-like fashion, to the possible erosion of a more active management role in stimulating the research that the Bank wants especially to see done. Submitted projects are netted well, but they may not be the most important ones. The Research Committee has not performed with distinction in the establishment of clear research policy guidelines; the attempt to do so by commissioning a series of research overviews in particular subject matters was not wholly successful.

At this point additional flexibility and wider latitudes for Bank research management may be helpful. The higher professional standards of recent years can stand more on their own with less need for continuous vigilance. As a matter, of course, management should be able to finance the initial phases of projects — at higher limits than currently imposed — without going through a cumbersome review process. The burden of proof ought to come later after initial results are in. This requires an even more tough—minded approach, because it will mean the need to cut off unpromising lines of inquiry. But, if implemented, it should mean a higher proportion of large project successes.

Similarly, at the other end of the research cycle, the present system of project-by-project evaluation is too mechanical. It can hardly uncover research styles and approaches that have been more successful than others. Evaluation of single pieces of research in isolation by committees of different composition seems destined to produce not merely partial, but also rather bland, observations. No project fails to make some kind of contribution; none, taken alone, is a spectacular success. A more useful evaluation framework should be more subject-oriented and recurrent, taking

into account projects both completed and underway. Such an internal exercise, performed annually or at longer intervals, could more probably feed back to the determination of research priorities on an informed basis. That review, unlike our own, should be dominated by Bank personnel, with perhaps minority outside representation. Whatever the advantage of occasional outside perception, self-criticism has the virtue of more likely leading on to corrective change. It is only when Bank management and staff come to feel strongly that inadequacies should be remedied that they will; internal discussion is more likely to produce that consensus than outside reports.

Conclusion

The Bank research program has performed to high professional standards in its investigations into income distribution and employment. Both the quality and quantity of its output are a tribute to the competence and technical proficiency of the staff and consultants. Our principal concern has been the ambiguous Bank view toward primary data that has resulted in inadequate attention to preparation of basic statistical series.

The research program has now convincingly established its academic integrity and professional capability. That evolution now makes it possible, and desirable, to turn it more directly to the variety of policy issues of Bank and member country interest, as well as to greater responsibility for data collection and dissemination. The reorientation in priorities recommended involves no drastic changes, but perhaps enough to permit greater Bank and developing country use of the product. It will also imply an additional commitment of resources for the expansion in statistical

activity proposed.

There is no need here to rehearse the specifics of our suggestions about Bank priorities in the gathering of data or what seem fruitful lines of research on income distribution and employment to pursue. They come down to greater emphasis upon the importance of knowing the distribution of income among socio-economic groups rather than persons or households exclusively; to understanding how the incomes of these groups respond to policy interventions; and to studying their changing size and composition over time. They also emphasize the importance of understanding how labor markets in developing countries, urban and rural, actually work in order better to design projects and policies that can create permanent employment opportunities.

Many of our suggestions relate to creating incentives and channels of communication to facilitate focus on these priorities. They include the possibility of a more direct relationship between research and policy papers, with the causality flowing from the first to the second, rather than the other way round. We have proposed serious consideration of a Bank Staff Papers to publish the best of the wide range of problem oriented analysis performed by the Bank. We have also recommended a new internal series of Bank papers for non-research staff, as well as a much expanded seminar program.

Among our principal concerns has been limited attainment of the objective of institution building in developing countries, as well as the limited impact of Bank research in stimulating local research. The goal is one to which we subscribe and to which we attach importance. It cannot be met as a by-product and without willingness to commit larger resources,

monetary and human, to its pursuit. Presently a small handful of countries are relatively favored objects of Bank Research to the exclusion of many others. And even they do not all regularly have interchange that permits Bank research results to be presented locally. Much can and should be done to improve the relationship.

The changes we suggest require a high-level impulse in some instances. But many can also be implemented within a research management framework that is more flexible, and has greater decision powers. The present procedures, designed to assure high professional quality, have accomplished their intended objectives. They may now inhibit rather than promote the research projects that ought to be pursued. The managers of the research program merit confidence and greater responsibility for defining what might be done, and how, subject to more over-all evaluation, and not on a project by project basis.

We conclude both positively and optimistically. The Bank research program, after a relatively few years, has impressive stature. We hope our recommendations serve to retain and reinforce it, while pushing in the direction of more policy relevant analysis.

Appendix A: Income Distribution: The Empirical Foundation

Our focus here is upon the adequacy of the data base, the coverage and quality of the date relative to the diverse concepts of recipient unit and income that should be employed. One may state at the outset that the data requirements, for a proper coverage of the distribution of income, free from transient disturbances and of the effects of different phases of the life cycle of income, among recipient units that are comparable basic family household units with allowance for their differing size, and with needed adjustments for purchasing power differentials among various distinct groups of such units within the economy, are highly demanding. The result is that adequate measures of such distributions are difficult to secure even for developed countries. One can expect a variety of non-comparable estimate, deficient in many respects, requiring critical scrutiny and diverse experimentation to reduce non-comparability, and extreme caution in deriving differentials claimed to be significant.

In view of the wide range of the difficulties with the supply and quality of size-distribution data and estimates, and of the dominant use of the latter to determine internal income inequalities, it may help to indicate the several problems that will be touched upon in the discussion that follows. These are problems of: (a) inadequate international coverage of the data and estimates;

- (b) errors in those estimates, relative to what they claim to cover;
- (c) disparities between the recipient units in the distributions and the ones required for analysis of income inequalities associated with economic growth;
- (d) disparities between the concepts of income and its variants used in the distributions and those required in proper analysis. The discussion will necessarily be far from complete for any of the problems touched upon, but it is important to cover the full range of difficulties. We conclude by (e) reflecting on the effects of limitations of the data on some of the findings.

(a) Redistribution and Growth (1974, designated Source A) contains in Table 1.1 perhaps the most comprehensive cross-section of ordinal income shares from size-distributions of income for 66 countries, 5 of them Communist and almost two-thirds of the remaining developing market economies. The compilation by Shail Jain, Size Distribution of Income (1975, designated Source-B) provides income shares, measures of aggregate inequality, and a few other measures, for 71 countries, 6 of them Communist. A more recent cross-section, in M.S. Ahluwalia, Inequality, Poverty, and Development (World Bank Reprint Series, No. 36, 1976, designated Source-C) covers 60 countries, selected almost wholly from the Jain compilation, the choice having been "dictated by particular judgments about the reliability of data in some cases". (p.339). This list contains 41 developing market economies, 13 developed market economies, and 6 Communist countries. We shall not discuss the Communist countries, since both the data base and the institutional arrangements for them involve major incomparabilities with the freer market economies -- concealing costs (and returns) so different from purely economic as to shift any attempt at proper comparison to a different level of discourse.

The larger number of market economies for which size-distributions could be assembled and compared suggests a wealth of data. But this impression is dissipated when the lists are examined, even if we disregard for the moment the errors and conceptual deficiencies that remain even in the data of the selected 41 developing market economies in Source C (see Table 8, pp. 340-41). The first observation is that some major developing countries in important regions are missing. Thus, for Subsaharan Africa, Source-C fails to cover the more popular countries: Nigeria, Ethiopia, Zaire, Sudan, let alone South Africa (which, by its over-all per capita income would belong to the developing group). And were we to possess a proper typology of developing countries, we

might find other important omissions for some type-classes within them. Second, the size-distributions refer each to a single year, with few relating to a time span (e.g., Lebanon for 1955-60). The dates vary widely within the period from the mid-1950's to 1971. Thus of the 41 developing countries, in Source-C, in which we are most interested, 10 countries are covered by estimates relating to years from 1955 through 1960; another by estimates relating the years from 1961 through 1965; and 22 countries by estimates within the time range from 1966 to 1971. Since the estimate of size-distributions can be affected not only by transient elements peculiar to a particular single year, but also by changes over time -- even in relation to the changing per capita product in constant prices -- there is an element of non-comparability in a cross-section comparison in which the estimates are spread over a time range as long as one and one-half decades.

But the most serious limitation in the supply of size-distributions is revealed not in Source C, which fails to contain intertemporal comparisons for one and the same country (but discusses some temporal implications of the major cross-section comparisons). It is to be seen in Source-A, in which Figure I.1 (p. 14) presents a graphic comparison of the growth rate in the income share of the lowest 40 per cent with that of GNP -- for 18 countries, of which 13 are developing Market economies; and Table II.1 (p. 42) which shows growth rates in the income of and market groups for 13 countries, or 12 developing (excluding Yugoslavia). This is a rather limited sample; and even were we to assume full statistical comparability, the short stretch of the interval (six years and not more than ten)

combined with the possible differences in transient elements in the terminal years may mean that temporal trends in income inequality, if any, could not be easily discerned. Such scarcity of time series relating to size-distributions of income in developing countries, is a major gap in the supply of data -- significant even if we were able to adjust fully for any incomparabilities in such intertemporal comparisons of the estimates as are available.

(b) Size-distributions are usually estimated from sample studies of household income (and/or expenditures) or census income questions. Under certain but limited conditions, they can be derived from the national accounts data using industry-factor income cells or other components that lend themselves effectively to a size of income array. But given the usual source, the commonly observed result is that the totals of income and components so derived tend to fall appreciably short of comparable totals in the national economic accounts; and the shortfalls are both substantial and significently different in relative magnitude among different income components associated with different levels within the income distribution.

Since the results of the intensive study by Dr. Oscar Altimir and his colleagues at ECLA, Income Distribution Estimates from Household Surveys and Population Censuses in Latin America: An Assessment of Reliability (Bank Staff Working Paper, November 1976) are available, there is little need to labor the point further here (see especially Chapters VII and VIII, Summary of Findings, and Main Consequences for Income Distribution Analysis). The wide incidence of major shortfalls in the sample and Census income data, with differences in relative shortages for various income types, are not limited to Latin America.

In another World Bank study, by Sudhir Anand, Size Distribution of Income in Malaysia (manuscript, Fall 1977), the author states (Chapter III, p. 22) that the "mean household income estimated from the Post-enumeration Survey (for 1970) is \$264 per month . . . and the degree of understatement in PRS income relative to the National Accounts is on the order of 25%." Then the author adds: "Although this might seem quite large, it is in fact not particularly great by the standards of household surveys conducted in LDCs."

Both of these Bank studies, and other of the work on developing particular social accounts includes, negate many of the assumptions of the aggregate compilations. They show the incomparability of the surveys over time, and warn against their use. Yet even they do not undertake much experiment with correction in order to produce a more reliable set of statistics that could be so used. It is only when there is such an alternative that the task is complete: warnings and cautions do not inhibit analysis and policy conclusions.

Even close agreement between the two sets of totals, of the surveycensus base underlying the size-distribution estimates and of the
national accounts, is no ground for assuming that there was no understatement (pr overstatement) within the size-distribution: it may
means that the shortfalls and excesses at different ranges within
the total roughly offset each other. This applies to any pair of
totals, whether they be for comprehensive income or consumption
aggregates, or for subcomponents such as wages and salaries, entrepreneurial income, and the like.

(c) The present subsection deals with scope of coverage and the recipient unit of the size-distributions used in the World Bank cross-section comparisons, in Sources A-C already referred to.

We begin with findings of a comparison of scope and recipient unit in the size-distributions used in Table I.1 of Source-A with the information on these distributions provided in Source-B. We again exclude the Communist countries; and find that of the 61 market economies in Table I.1, one (Madagascar) is not reported in the Jain compilation in Source B. Of the 60 market economies, the size distributions for which are identifiable in both sources, the check reveals that the coverage is short of national for 8 countries, with some question about the 9th. For Argentina, Burma, Dominican Republic, Greece, and Iran, the coverage is either of the capital city alone, or of urban only; for Guyana, Sierra Leone, and Uganda, there are major geographical or group exclusions. For Thailand (1970), Source B shows distributions for rural and urban households separately, but not together; and the ordinal shares shown in Table I.1 are close to those for the urban distribution in the Jain compilation. For these 9 countries there is an unknown element of non-comparability, associated with limitation of coverage, relative to the distributions for other countries with full national coverage.

For the remaining 51 countries, the size-distributions are for the following types of recipient units, using the terminology of Source-B: households -- 25 countries; income recipients -- 12 countries; economically active population -- 5 countries; total population, a rather vague category -- 6 countries; and per capita -- 1 country (this being the United States, the reference indicating a combination of the

distribution among unattached individuals with that among families reduced to a per person basis). The dominant groups are then of distributions among households, or among recipients -- whether all or among the economically active population.

A check on the data base of a more recent summary of cross-section evidence on size distributions of income, in M. S. Ahluwalia's paper on "Inequality, Poverty and Development," in Journal of Development Economics 3 (1976), pp. 307-342 (Source-C), indicates that of the 41 developing countries only 2, Uganda and Guyana, relate to an area or group short of the national total; that of the remaining 39 developing countries, the distributions in 16 countries are among households; the distributions among income recipients, or economically active population, or total population cover 7 countries each; and for 2 countries the distributions are for per capita income. With the distributions for the 13 developed countries being among households for 8 and among income recipients for 5, the distribution for all 52 market economies, with 24 distributions among households, and most of the other among different variants of personal income recipient, is not unlike that for the 51 countries in Table I.1 (Source-A) described in the preceding paragraph.

With respect to the possible results of a mixture, in one comparison, of size-distributions of income employing different, and
conventional, recipient units, two comments can be made. First, the
difference in recipient units may result in different ordinal shares
for the same country and year -- with a marked tendency for distributions
among income recipients to show wider inequality than for those among
households. Thus, if we distinguish within Table I.1 of Source A,
the 25 countries for which the distributions use household units (Group
I) from the 26 countries for which the recipients units are individuals

(Group II), we find that the 9 countries with low income (below \$300 GNP per capita in 1971 prices) in Group I show an average share of the lowest 40 per cent of 14.2 per cent in the total income; whereas the 12 countries of Group II in the low income category average, for the lowest 40 per cent, 11.2 per cent of XMX total income. A similar comparison of the 10 countries in Group I with the 5 countries in Group II that are in the middle income bracket (\$300 to \$750 per capita GNP) yield average shares of the 40% lowest of 13.4 and 8.0 per cent of total income respectively. For the 6 countries in Group I and the 9 countries in Group II that are in the highest per capita income class (\$750 and over), the average shares of the lowest ordinal group are 17.1 and 14.2 per cent respectively. Disparities in ordinal shares of the lowest group of units within similar ranges of per capita income of this magnitude -- a fifth to four-tenths of the larger of the two shares -- are too wide and too consistently in one direction to be neglected.

Second, and perhaps more important, neither of the widely used types of unit, whether household or individual income recipient or worker, stands for equivalent groups of dependent consumers. Households differ in number of members, and the distribution that classifies households by income per household would naturally show a significant positive correlation between size of household and its income -- so that lower household income is associated with a small household, a smaller number of persons dependent upon that income. But the same is likely to be true of the size-distribution among individual income recipients. The earners or recipients of lower incomes, dominated by part-time workers, secondard labor supply, and the like, tend to be associated with fewer dependents on that income than the high individual income recipients, more representative of heads of families

and of ages in the life cycle where both income and number of dependents are likely to be large. It follows that ordinal shares, say the lowest 40 per cent of households or of income recipients classified by income per household or per recipient, may represent shares of population (whether persons or consuming units) that are distinctly below 40 per cent; while the shares of the top 20 per cent of households and income recipients represent those of more than 20 per cent of population or of consuming units. The essential point is that these adjustments varydifferentially among countries and over time, introducing significant incomparabilities in the welfare implications of the measures.

This comment affects not only Table I.1 in Source-A and the size distributions in Source-C, but also the uses of the original shares (and inequality measures) in other applications. Thus it relates to attempts to use the conventional size-distributions to identify people below poverty lines, or deficient in associated consumption levels, or, constituting the rich at the other extreme. Even disregarding the advisability of employing equivalent consuming units rather than persons, and of stressing, at least as an alternative, the distribution and levels of consumption rather than of income, the need of adequate adjustment to shift from households and income recipients, is indispensable.

All work on poverty and basic needs must face, in addition to other problems, the conversion of conventional size distributions to shares of properly defined units (presumably equivalent consumer units) in the appropriate income totals.

One should note at this juncture that in the case of intertemporal comparisions, there are additional major difficulties. These may be difficulties of attaining adequate statistical comparability among two or more samples over the span of time; of adjusting for the difference in transient characteristics of the two or more years being compared, assuming that the data on recipient units and income relate to single years rather than to averages over several; and, analytically most XX important, the difficulty of establishing the extent of mobility over time of recipient or dependent units in and out of the lower or upper ordinal groups. This is partly associated with the effects of transient. short-term elements in the income distribution, one encountered in particular with income levels but applicable also to the structure of the household; but it raises the bigger question as to how many of the poor and rich of today were among the poor and rich of, say, a decade ago. Clearly, wide mobility among properly defined ordinal classes by properly defined income per properly defined recipient unit would lend an entirely different meaning to comparisons across time of the shares of the poor and the rich than would be ascribable under conditions of complete or relatively complete lack of such mobility.

A final problem is the very definition of the household or family in diverse cultural settings. Some recent trends, such as the rapid morcellization of family households in the developed countries, and institutional aspects of family structure strongly suggest the need to be aware of the consequent limitations of the conventional recipient units -- over and above the lesser problems with which we are more familiar and experimentation with which it is now feasible for a number of countries.

(d) This sub-section deals with the problems involved in the definition of the income total that is distributed among recipient units in the conventional and available size-distributions.

Two observations relating to the income total aspects of the size-distributions used in the several World Bank sources already considered can be made. The first is that there must have been some differences in the scope of income totals used among the size distributions for different countries or for different years. We know that for some countries (e.g., the United States) sample studies of family and household incomes are limited to cash income and exclude income in kind; that for other countries households are grouped by total income receipts including gifts and transfers from other households (e.g., Taiwan); and so on. Hence, the multi-country cross-sections in Sources A and C must include elements of non-comparability in the definitions of the income totals, in addition to those involved in the use of different types of recipient units; but how large such elements of non-comparability are, we cannot tell at present.

The second observation suggests that the size distributions for few countries, whether developed or developing, would be based on the use of income totals that would satisfy the analytically desirable criteria. These criteria relate first to the completeness of coverage of the income, in its inclusion of both cash and income in kind; of factor receipts as well as flows from government and other institutional sources, as well as the compulsory drafts that may be imposed by them; and also of receipts and transfers among households insofar as they reflect the ties of common interest among separate households of the type noted above.

The criteria relate next to what might be called the time level of the income reported and used, as distinct from the time level desired for many analytical purposes. This refers to the need to eliminate or damp transient, short-term components in annual income, and to adjust for the effect of shorter phases of the life cycle income path of the recipient unit.

In the third place, the possibility of substantial differences in purchasing power between the rural and urban recipient units, and within these large groups, between the lower and higher income groups, has to be considered.

And, finally, one should note again, for intertemporal comparisons, the possibility of mobility of recipient units among the distinctive size-classes, even when distinguished by comprehensive estimates of long-term secular income levels adjusted for inter-group differences in purchasing power. Admittedly, these criteria are a counsel of perfection but there is value in formulating the analytically desired income totals, if only to induce experimentation designed to provide a better notion of the magnitudes involved and a better understanding of the kind of basic data needed if questions implicit in the disparities between the conventional data and those alleged (?) are ever to be answered.

Numerous illustrations of this second observation could be provided, both from the World Bank documents and from scholarly publications elsewhere. But one may hope that the points made are sufficiently clear; and we can turn to considering the implications of the difficulties with the supply and quality of data for some of the aspects of the work on size-distribution at the World Bank.

(e) The discussion above relates solely to the weakness of the empirical foundation provided by the conventional data on size distributions of income among households or among income recipients. The comments should not be misinterpreted as denying the value of emphasis on the distributive aspects of economic growth, particularly in developing countries; of the ingenuity with which the work in the field by the World Bank attempted to distill findings from disparate data; with some caution that increased progressively as the limitations of the data became more apparent; of analysis of distributive implications of different structural aspects of growth illustrated by relatively simple models employing notional but still plausible parameters; and of trying to introduce into project appraisal and other service operations of the World Bank sensitivity to possible impacts on internal income inequality. But one is left with the question whether much more experimentation and selective treatment should have preceded (rather than followed) the kind of stocktaking and generalization that were exemplified in the empirical summary of the dize distributions of income of the type provided in Sources A and C, or in publications relating to poverty, or in the compilations exemplified by Source B.

Many of the limitations of the data used, relating particularly to the nature of the recipient unit and definition of them. were recognized by the authors. Yet the natural inference from such limitations, in the direction of experimentation with different recipient units, different income totals, scrutiny of the disparities between the sample totals and the comparable totals in the national accounts, critical rejection of some country data as resting on too

weak a basis, and retesting the findings in terms of the results of such experimentation, was apparently not followed. Instead the presumption has been that the errors are random and do not lead to spurious conclusions. In a subject so wrought with political implications, the attempt to obtain the right numbers for individual countries cannot be so easily dismissed in favor of the aggregates; nor can randomness be assumed when the various effects of recipient unit, size of family, etc., earlier discussed, are considered.

One may wonder whether a closer scrutiny and rejection of a number of shaky estimates (which could be listed, but with the data omitted), and more of useful information on the data included, would have met the needs of furthering quantitative research far more than the present compilation; and whether the failure to provide such selection and information may not result in uses of the readily available ordinal shares or inequality measures that would be more misleading than enlightening.

The argument for greater selectivity and experimentation with the conventional size-distribution of income data is not made on the ground that the results are likely to modify substantially the few findings already derived on differences in internal income inequality among broad groups of countries, or on the time pattern of such income inequality associated with phases of economic growth. Given a variety of biases in different directions, one cannot tell.

Appendix B: Research on Income Distribution

Introduction

The Bank research program in income distribution has emerged in a self-conscious, planned fashion. In recognition of the limited investigation conducted elsewhere, the Bank has been prominent in stimulating and pursuing research on income distribution in developing countries. The initial Bellagio conference and subsequent publication of Redistribution with Growth has meant a leadership role that has not been characteristic of all fields of research.

Bank prospects and priorities were established after careful review of what was and was not being done elsewhere, the likelihood of potential progress, and the relevance to Bank interests. The research strategy, elaborated for and endorsed by the Research Committee in 1975, has emphasized three subject matters:

·empirical, data-oriented, analysis

·construction of economy-wide models for policy experiments

·examination of consequences of policy interventions in a partial equilibrium framework

The first two components of this program have absorbed the lion's share of external resources. More internal staff time has been allocated to the third.

These priorities were chosen to respond to the issues posed to the Bank as it seriously began to grapple with the income distribution question.

The data base was limited and uncertain, and called out for improvement:

as Appendix A has indicated, there are profound difficulties in assessing how the income distribution in even the most important countries has fared over time for a consistent set of recipients. Beyond that, there was much talk

of iron laws of development, inevitable impoverishment, and the like. The role of public services in offsetting the inequalities of private receipts was a matter of debate and dispute. The characteristics of the poorest groups and the means of reaching them were still largely limited to a handful of countries.

What could be done about these problems — while retaining the market and growth orientation emphasized by the Bank — was also not part of the conventional wisdom. There was considerable attraction to viewing income distribution amelioration in the context of a larger productive structure, precisely to capture the second-round effects that direct policy analysis fails sufficiently to take into account. Hence the relevance of large country models, as well as research focused on policy instruments.

The strategy thus made, and for the most part, continues to make sense. The professional quality of the analytical results, as well, compares favorably with non-Bank sponsored research in the field. Many have been or are in the process of publication. Bank consultants have been eminently qualified and highly regarded. More question has rightly been raised in Appendix A about the weakness of the compilations of data, and the uncertain yield of the substantial investment in extending the data base. There is no need to rehearse those issues here, save to say that the analysis of the data has been much less plagued by such difficulty.

In the following sections, we now evaluate a sample of 16 individual contributions under the three headings noted above. The concern is less with the research for its own sake, than in seeking to draw out its attributes relative to what the Bank ought to be doing.

Empirical and Analytical Analyses

The research classified under this rubric is largely of two sorts,

cross-section analysis of aggregate data, and country studies. Seven works have been examined, three of the first kind [1, 2, 6] and three of the second [3,4,5,7]. Three are book length, four take the form of articles.

The cross-section focus of an aggregate kind [1, 6] responds to the important issue of how the size distribution of income changes with sectoral, educational, and per-capita income change in a large number of countries. It is clearly the most sophisticated of its genre, both in its cautions and in the carefully qualified conclusions. The limitations of cross-section analysis are clearly understood and presented. Publication is amply warranted.

Still, it might have been better to start the other way around: with the substantive issue of the trade-off posed by rapid growth for inequality, in which the cross-section regressions might have figured as one piece of the evidence, rather than being the central analytical focus. A small selected set of countries whose changes in inequality over time were carefully measured might have been set off against the cross-section to measure its predictive content. The famed U-shaped curve of the size distribution as per capita income varies might have been assessed more critically as the artificial consequences — at the lower end — of minimum subsistence requirements, causing us to examine in detail the processes that operate in the critical, but limited income range of \$200-\$300. Thereafter, for all practical purposes, a linear relationship seems to prevail. Finally, other Bank research on real income comparisons might have played an integral part in making the statistical analysis much more meaningful.

These criticisms do not mean that the research conducted is not useful. Quite on the contrary, the Bank results - properly understood -

mean that great care is needed before affirming the inevitable propensity for the poor to get poorer. The authority of the result, and its relevance for Bank policy, could have been much enhanced, however, had that objective, rather than the method, dominated. That is why, in our recommendations, we have tried to stress a policy rather than an academically oriented research view.

Decomposition analysis of the Latin American survey results [2] is cross-section in a different way. Not only is a single region involved, but the issue at hand is also another: what is the influence of personal characteristics versus market characteristics in the explanation of inequality. Again, both in the theoretical discussion of decomposition techniques and in their application, the research goes beyond previous efforts. Its technical proficiency is not at issue. What is, once more, is having the mechanics of the techniques dominate the larger set of substantive questions that are at the heart of the matter. The variables of sector and occupation do not adequately capture the distinction that is needed, leading to a statistical decomposition whose interpretation is in doubt - the more so because of interactions with education, age and sex. Nor is any comparison of the results among the different Latin American countries undertaken in order to see whether they conform to what other evidence might suggest. Finally, although considerable effort was expended in order to test the validity of the raw data - which are high variable in reliability - the uncorrected information is processed in all cases as if it were of identical quality.

The basic limitation is research that is directed and defined by an internal body of data, rather than a broader set of hypotheses and concerns that require statistical testing, but of a more varied kind. Research that attains only the first stage fails to have the impact on policy it should

and might have.

Country-oriented research within the Bank has managed more frequently to cross the barrier, perhaps because it lends itself to more broad gauged concerns. The Malaysia study [3] is a case in point. While utilizing information from surveys, it is critical in its evaluation of their comparability, and careful in the inferences drawn. It transcends them to take on a series of policy questions and issues that impinge upon the size distribution and their changes. The study is consequently one that both Malaysian economists and those in the Bank will find helpful in understanding what is going on; the irony is that the country economists were largely unaware of it.

The construction of a social accounting matrix for Malaysia [5] is of similar value, although its principle objective is measurement rather than analysis. The focus on a single country and the necessary reconciliation of the national accounts with partial information on distribution processes produces an attitude toward data that is more productive than at the more aggregative level. While more experimentation with different imputation techniques might have been tried, and deviations from the frequently held assumption that the national accounts were invariably correct permitted, the monograph is evidence that data can be manipulated effectively within the Bank when a serious effort is made. It is relevant that these socioeconomic accounts have become a matter of more theoretical concern, and that a Malaysia model requires their input. That helps to explain the motivation.

Yet country research can also bog down in too narrow a context. The

Taiwan study [3] becomes a virtual catalogue of Gini coefficient decompositions

of all shapes and sizes over time, neglecting some of the basic forces at work

that reflect themselves in these coefficients. There is little discussion, for example, of the factors favoring the spread of rural industry in the case of Taiwan, little explanation of the processes by which dualism diminished within sectors, the reasons why technological change favored wage income, etc. Taiwan is an essential component in the puzzle of how rapid growth can be consistent with a distribution that not only did not deteriorate, but displayed some improvement. The study, while a technical advance in its demonstration that Gini coefficients, too, can be decomposed and interpreted, fails to draw out those lessons and make them available to Bank and developing country decision-makers.

The work on earnings functions growing out of an extensive study of income distribution in Thailand is of a similar bend. It is the technique that is cared about more than the specific results. The article is well done and makes its point that indirect estimation of the contribution of

That is the natural bias of the professional advancing the field. It has a role. But for Bank needs, the substance and the policy too must count; that is what differentiates its research agenda from that of an academic institution.

Large Scale Models

The research upon economy-wide, general equilibrium models shares some of the same attraction to the mechanics at the expense of some basic questions. Two large but different models, for Brazil [8] and Korea [9], have been estimated and put through simulation paces. These efforts have been major projects. Their intent was to examine the effects of government policy upon the size distribution of income. Both have made technical advances in theoretical and programming terms. Yet neither lives up to the

original intent.

There seems to be three reasons why. One is inattention to the inherent limitations of what remain "standard" economic models for the analysis of the size distribution of income. The basic rules for distributing income to persons, as opposed to economic agents defined by the productive process, are not an integral part of such models. The personal distribution is attached only subsequently by converting wages and profits via static and non-behavioral constants that have been computed from earlier survey and Census results. This is equivalent to saying the rank-ordering of persons does not itself feed back upon the economic (and political) process. Its expression is the assumption, in the Korea model, of an unchanging contribution to inequality within particular socio-economic groups, for every group but one. In the Brazil model this source of variance is simply assumed to be null. Yet the level of such variance can equal half or even more of the measured inequality among persons, and its changes can, and have contributed to the observed economy-wide changes.

A theory of distribution to individuals is not a simple matter. But until it is explicitly focused upon, it will not be possible to examine how the distribution of permanent, or life-time, income varies in response to economic change and economic opportunity. Nothing less than a complete specification of individual mobility, and the variation in individual incomes, is needed. This class of models does not attack such questions.

The difficulty of the task, indeed, is one reason why attention might perhaps be more profitably focused on socio-economic groups, thereby, as in the Brazil model, ignoring the within-group variation. But even then, more than mechanical allocation of labor income and profits are required, and mobility between groups cannot be ignored.

A second limitation of such models is that while nominally they are large, by virtue of including many sectors and groups of income recipients, they are in fact very sensitive to their macro-economic specifications. The interactions among most sectors makes little difference to the outcome. In this sense, the models are too simple. Both models, in fact, seem to equilibrate by taking some magnitudes as fixed in nominal terms, and allowing the overall price level or the terms of trade to adjust to make real demands and supply equal. Such effects then become the source of the distributional changes. Whether these processes conform to the structure of the actual economies is another matter. In neither case was the issue one of significant empirical investigation prior to incorporating such processes in the model. If they are inaccurate or exaggerated representations, then policy conclusions are, of course, much affected, and much weakened. It is difficult to believe that increased agricultural productivity in Korea goes to naught because demand for output is fixed, provoking deteriorating terms of trade; or that in Brazil inflation improves the distribution, but only because some service incomes do not change while other wages do.

In the third instance, too little attention has been paid to capturing the differences among clusters of sectors, or groups, rather than multiplying their number. Sheer size is a complication, not merely for calculations, but also comprehension. The crucial parameters like the elasticity of substitution, the characteristics of technological change, and savings propensities get lost sight of in the exercise. The simulations take these for granted in assessing policy impacts, when their changes — partially in response to policy — may be more important.

The upshot of the matter is that the models have had little impact on the analysis of Korea and Brazil, within the Bank or within the countries themselves. It was perhaps exuberantly optimistic to think that such would be the result. Lowered expectations might have been in order. In that context, they can be evaluated more favorably and positively. They have advanced Bank thinking about model construction. They have also forced Bank researchers to ponder their common result that little change can be induced in the size distribution even by far reaching policy measures — although that is exaggerated by focusing on aggregate indices. Small changes in Gini coefficients can yield rather large distributional shifts, and these have been observed in many countries.

It is important to note that these criticisms and others have emerged from critical discussion within the DRC itself. Indeed, one of the papers under review [10] is a thoughtful appreciation of the characteristics of such models written by one set of their authors after the fact. It in turn inspired other such analytic work within the Bank, as well as was a subject of discussion at the Bank sponsored second Bellagio meeting. This capacity to learn, and to adjust future priorities is a strong point of the present research management; what is important is to shorten the perception time, and also to target less ambitiously technically, but perhaps more so substantially.

A contract in model building is the effort underway for a few years now to construct one for the Muda Regional Economy. It takes its start from a serious effort at construction of regional accounts for relevant socio-economic groups [11]. Its ultimate purpose is to measure more accurately the second-round consequences of project investment in the region, whose spill over potential was considerable. Both in rooting the exercise in region-specific information, and in developing techniques for ascertaining what the additional project effects were, the exercise is better suited to

Bank expertise and need. The model in question is less sophisticated than those for Brazil and Korea in that it is smaller and has fewer indigenous variables. The results it is able to produce, however, can and should influence Bank policy on design of irrigation projects. Such research has been carried on in close contact with the Malaysian planning office, and is a multiple effort within the DRC itself. The lesson is that there are useful models that seek to introduce integrally the issue of distribution. What is at issue is their appropriate design and their objectives. The more rooted in actual policy issues, the more country oriented in empirical design and collaboration, and the more directed to particular substantive questions, the higher the potential yield.

Specific Policy Instruments

Four papers have been sampled among the diverse output that falls in this category. They each represent a significant aspect of research.

The most general is a recently published occasional paper on malnutrition and poverty that has aroused considerable interest [13]. It seeks to establish that increased income is inadequate to solve the malnutrition problem, requiring intervention instead. The problem then turns on defining efficient mechanisms of intervention. This research is the precursor for a whole class of research regarding basic needs. The two basic criticisms directed at it are applicable to this larger set as a well

First is the problem of measurement. As Appendix A notes, casual allocation of population based upon size distribution even of households is not a direct process. In this instance, the problem is further compounded by limited distribution data for many of the largest units. Although only

orders of magnitude are perhaps all that is required at this point, at what point do errors of 10 and 20 percent begin to matter? When basic needs come down to implementation, surely the inaccuracies must affect the effectiveness of policies.

Beyond the measurement problem associated with allocating individuals to particular classes, there are the two additional difficulties of determining minimum nutritional standards, and the elasticity of intake of nutrients with respect to income. The former are still a matter of controversy among nutritionists. The latter are not constants to be readily applied: they must vary widely with custom, the intrusion of processed foodstuffs, the rural-urban mix, let alone traditional economic variables.

The monograph thus makes the definition of the problem too easy, when the persistence of malnutrition in spite of income growth cries out for more subtle and variegated investigation. At the level of solution, there is the same simplification. Efficient intervention is required, yet that efficiency is measured by reference to consumer preferences that are negated by the very necessity of intervention itself. If consumer preferences were a meaningful standard, then the market should allocate expenditures accordingly in order to maximize satisfactions. When we say the market doesn't work, we cannot use it as the measure of efficiency by which to judge direct provision of services. This point is of course the general one for both basic needs research and basic needs provision. The essence of the strategy is its delivery system that ignores individual wants. Deciding how much is sufficient, and devising substitute means for allocating the relevant goods and services is central.

While attractive in holding out means both for measuring the extent

of the deficiency and criteria for correcting it most efficiently, the monograph does not entirely hold up under closer scrutiny. That said, the line of research is patently worth pursuing further and improving upon. Despite its limitations, the monograph is an example of how Bank research can attack problems ignored elsewhere. Measurement of basic needs and schemes for intervention are matters that are likely to be of continuing priority in the next several years. The attractiveness of this research style is its capacity to eschew larger and general equilibrium approaches in favor of a partial focus. So long as each basic need is considered individually, that may be true; when taken as a composite such independence may well no longer prove feasible.

Whereas the criticisms of the large economy-wide models go to the genre, and hence the wisdom of continuing Bank effort in that area, the criticisms here go to the particular. They suggest instead the need for a larger and continuing effort along the same lines.

The same is true of the research on the distribution of public services across income groups [14]. The size of the public sector in most developing countries makes its contribution to the distribution of real goods and services an essential one to take into account. It is a problem both well defined and relevant. Yet it fails to take into account the differences in the quality of services to persons in different income strata, when that seems an essential part of the problem. It fails to explain why the survey on which it is based differs so substantially from an earlier one in which the distribution of services is much more that there been a real change in Colombia, and why? How do demands for public services that appear to be satisfied as income rises, differ from basic needs? As earlier noted for other empirical research, the study

becomes involuted in its own statistical basis, to the exclusion of other data, and to the exclusion of extension to other groups of countries.

The merit of such research, after all, consists in measurement not for a single country, but for a variety of representative countries. Yet no continuation elsewhere is proposed, nor is any serious effort made to interest planning offices to undertake similar efforts. That is where the absence of an explicit policy priority in the research effort makes itself felt. What should be concerned as a pilot study to be replicated elsewhere becomes another final product. The work is solid enough to build upon, and improve. As an institution, the Bank has the capacity to initiate and cumulate this kind of comparative research.

The sharecropping research is of a different style. The basic theoretical contribution that is reviewed here [15] makes significant amendments to a literature that has greatly expanded in the last several years. Its publication in the professional journals, as with much of the other writings relating to sharecropping, is a measure of its quality and novelty. What is perhaps more important from the Bank standpoint is the productive relationship between such theorizing and empirical application. Both are proceeding simultaneously. What is less fortunate is that while the structure of agriculture employment in many developing countries is critical to the distribution of income, the theory, and micro-testing, have not lead on to a more synthetic piece that relates the results to larger issues of agrarian reform. Excellent as the work is, and promising in its continuing extensions, there is a hiatus that does not appear likely to be bridged of its own momentum.

The final paper, yet unpublished and only recently completed, relates to the agricultural sector as well [16]. It is motivated from a direct

policy concern with the implications of price intervention for the distribution of income. It casts the problem in partial and simple general equilibrium form of a kind previously used to study tax incidence. The paper makes clear the crucial dependence of policy impacts upon the mobility of factor inputs and their ownership by different income classes. It thereby makes evident the need for empirical research directed to establishing there, among other, behavioral relationships if the framework is to be useful in describing actual changes. It also exposes the need to relate short-term policy impacts to subsequent, dynamic adjustment processes. An immediate increase in real wages will not necessarily be eroded through competitive forces as the model implies. The paper is useful in defining a theoretical framework for subsequent application; its contribution will depend upon whether the latter is pursued.

Conclusion

This review makes apparent both the high quality and varied character of Bank income distribution research. It is technically proficient and directed at a wide range of problems. Its quantity should also be remarked upon. The productivity of Bank staff compared with academic environments in which research is a large and regular component of responsibility is quite high. Specific criticisms relate as much to the product of outside consultants as to Bank staff.

The deficiencies of the research have related less to its execution than to the loss of policy orientation that has sometimes occurred. When it has, there has been a tendency for narrow technique to dominate at the expense of the larger substance that ought to characterize Bank research. It is such a reading of the situation that has lead us to stress so heavily the importance of such a research emphasis.

A second limitation has been the inability to sustain more policy oriented research beyond a single, isolated effort. The Bank budget is small and its research agenda cannot be greatly expanded. For that very reason, however, it should be following a much more active catalytic role in encouraging countries to follow its lead. This has not much occurred, and has lead us to stress the potential inherent in closer links with developing country research institutions.

A third characteristic has been a greater faith in large models than was probably merited by their earlier performance. There are limits to what can be extracted from them. Systematic study — call it a model, if you will — of the second round effects of projects and policies has been slighted in favor of trying to solve everything at once. That illusion now seems well implanted in Bank experience. There should be an occasional reminder to prevent its resurgence. We take the lesson to be the need for more comparative, country-study analyses over time to try to identify the key processes at work, before mechanically trying to establish models that have no story to tell.

Annex

Sample of Papers

- (1) M.S. Ahluwalia, "Inequality, Poverty and Development", <u>Journal of Development Economics</u>, 3, 1976, World Bank Reprint Series 36.
- (2) O. Altimir and S. Pinera, "Decomposition Analysis of the Inequality of Earnings in Latin American Countries", August 1977 (mimeo).
- (3) J.C.H. Fei, G. Ranis, S.W.Y. Kuo, Equity with Growth: The Taiwan Case, 1977 (mimeo).
- (4) S. Anand, The Size Distribution of Income in Malaysia, Nov. 1977, (mimeo).
- (5) Graham Pyatt and Jeffery Round, <u>Distribution of Income and Social Accounts:</u> A Study of Malaysia in 1970, December 1977(mimeo).
- (6) M.S. Ahluwalia and J. Duloy, "Poverty Alleviation and Growth Pessimism: A Re-Examination of Cross Country Evidence", Bellagio Workshop on Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).
- (7) C.U. Chiswick, "On Estimating Earnings Fuctions for LDCs", Journal of Development Economics 3 (1976), World Bank Repring No. 44.
- (8) I. Adelman and S. Robinson, A Wage and Price Endogenous General
 Equilibrium Model of a Developing Country: Factors Affecting the Distribution of Income in the Short Rum, Stanford University Press,
 November 1977 (joint publication Stanford University and Oxford
 University, U.K.).
- (9) E. Bacha, L. Taylor, F. Lysy, Models of Growth and Distribution in Brazil, 1977, (mimeo).
- (10) L. Taylor and F. J. Lysy, "Vanishing Short-Rum Income Redistributions: Keynesian Clues about Model Surprises", Bellagio Workshop on Analysis of Distrubtional Issues in Development Planning, April 1977, (mimeo).
- (11) C. Bell, S. Devarajan, P. Hazell, R. Slade, "A Social Accounts Analysis of the Structure of the Muda Regional Economy", November 1976, (mimeo).
- (12) C. Bell, P. Hazell and R. Slade, "Autonomous Growth and Project Impact in the Muda Regional Economy: 1967-1972", September 1977 (mimeo).
- (13) S. Reutlinger and M. Selowsky, Malnutrition and Poverty: Magnitude and Policy Options, World Bank Staff Occasional Paper No. 23, 1976.
- (14) M. Selowsky, The Distribution of Public Services Across Income Groups:

 A Case Study of Colombia, May 1977 (mimeo).
- (15) C. Bell and P. Zusman, "A Bargaining Theoretic Approach to Cropsharing Contracts", World Bank Reprint Series No. 45, (American Economic Review 66 (September 1976)).
- (16) T. Bertrand, "Market Interferences and Income Distribution: A Methodology for Studying the Agricultural Sector in Less Developed Economies", August 1977, (mimeo).

Appendix C: Research on Employment

Introduction

This Review is based on a sample of over twenty papers produced during the last few years and selected representative of the Bank's research efforts into employment. A striking feature is the generally high quality of the research output. Indeed a number of pieces have already appeared or are about to appear in learned publications. Many of the authors have been recognized as acknowledged experts in the field. On the other hand, the relationship between the subject matter of the research and the operational needs of the Bank has not always been evident. One measure of this lack of impact is the rudimentary attention to employment issues in country reports as documented in a comparative study of them (4).

The impression created, therefore, is a series of isolated papers that do not relate to any clear research strategy. Some of the research is almost purely academic. Three of the papers in question, (10), (17), and (20), were prepared for a Research Workshop on Rural-Urban Labor Market Interactions in 1976. It remains unclear how the results of this Workshop are being processed for consumption within the Bank, as opposed to publication for an outside audience. The papers provide useful summaries of where the field is, but without the critical link of guiding further Bank research or converting conclusions into policy implications. Even where the papers are more focused to Bank concerns, they did not seem to relate well with one another even within narrowly defined subject areas. Authors did not seem to be working together. Both for reason of substance

¹Enumerated in the annex.

and execution, the research output has therefore seemed unplanned and without major consequence for Bank performance in this important area.

The main subject categories of the research, and the associated papers, cover the following topics:

1. Rural Employment

2. Urban Employment

3. Rural-Urban Migration

4. Education

5. Shadow Wage

The basic conceptual framework governing these various categories of research relies on the distinction between labor markets in the urban sector and the rural sector, with a theory of rural-urban migration to link the two. In addition, the urban sector is differentiated into a formal and informal market. Education is singled out for special attention influencing employment opportunities. What is prominently missing is attention to the direct and secondary employment effects of projects, or lead on to better project design. Even the work on shadow wages, as in (14) and (20) complicate the formulae by introducing potential distortions that might arise. How to identify when such adjustments should be made, and how they could be made, has on the other hand received virtually no attention.

Rural Sector

After a period in which it was believed that rural unemployment was a

potential source of "cheap" urban growth, and then a series of studies suggesting that rural labor markets were in fact working, a more balanced and realistic concern with creating more employment opportunities in the rural sector has come to prevail.

The Bank's work in drawing attention to rural, non-farm activities is therefore much to be welcomed. A review of the issues posed by such a strategy (1), a case study of Taiwan (7), and advocacy of small enterprise as a means of providing employment (8) fall in this category. In many respects they are atypical research products: two are highly policy oriented, (1) and (8), and one is very highly descriptive (7) rather than analytic. They are useful illustrations of why the Bank itself ought to engage in analytic research in order to go beyond assembly of information to get at the basic processes at work.

Thus for all the usefulness of the facts organized in the Taiwan study, one is still left unsure why in Taiwan the increased demand for non-farm goods in the rural sector — characteristic of almost all developing countries — was met by supply in the rural areas. One answer seems to be the relatively well developed infra-structure and favorable conditions of labor supply because of agrarian reform. Such hypotheses are barely specified, yet given adequate test. The intellectual challenge is an appropriate model of rural-urban industrial location that explicitly recognizes the possibility of capital mobility to the countryside. More studies of successful, and also unsuccessful, historical experiences are indicated. But they require a firmer analytic foundation.

Both of the policy focused papers suffer by reason of such prior research. They are efforts to reach conclusions without a fully adequate basis. They are themselves research in putting together what is known,

and are largely sensible documents. Yet they come before the research cycle, rather than logically, as they should, at its conclusion. As a result they suffer from inadequate reference to broad historical experience, and earlier efforts to introduce small enterprise in rural areas. The data analysis in (8) is too limited and partial to sustain strong recommendations for a large Bank effort in this area; the labor intensity of small scale enterprise is not adequately explained. The review of policy issues in (1) similarly is not rooted on firm intellectual foundations.

One can be encouraged by the impulse provided by such efforts to undertake a more comprehensive research project that addresses some of these basic issues. It is important that such investigation go beyond particular surveys it may collect to the actual policy experience of many individual countries on a comparative basis.

The Bank has also concerned itself with the agricultural sector, and has made a determined effort to design projects that improve the productivity of the small land-holder. One large project, the Analytics of Rural Change, bears upon such policies. The paper considered here on the rural household (2) is merely one of a related series that has come forth from the project. Its explicit theoretical and econometric research style contrasts with those considered above. It also illustrates how such technique can be compatible with interest in response of individual economic units to policy interventions.

Inevitably, however, the technique also influences the range of permissable conclusions. It is not clear that less restrictive, alternative specifications might have yielded similar results. Production functions that are constant in returns to scale and neutral in technological progress

are of a special character. They are the more questionable in view of the lack of statistical significance of the capital parameter. Finally, the short term format of the model imposes a lack of realism on the consequences of price changes.

The larger issue, however, is that resort to sophisticated econometric technique is imposed by limited, not extensive information. Research such as this, correctly seeking to integrate consumption, production and labor force participation decisions, should be pursued at the Bank using the extensive data available from Bank projects themselves. These afford an opportunity, almost unique, for virtually controlled experimentation. At the moment, only the capital-labor substitution project (6), seems to have exploited this access. It, cut off from a large analytic context, is only a beginning.

Urban Sector

Much research on employment in the urban sector in recent years has departed from the distinction between formal and informal labor markets.

Bank research is no exception. The logic of the differentiation is that the former has barriers to entry while the latter is subject to competitive market clearing forces.

While there is much to the point, and a whole series of papers exploit it (9), (13), (11), (14) and (15) (the latter three are discussed in the section dealing with rural-urban migration), the distinction can be exaggerated. The informal sector is itself quite heterogeneous, affording increasing employment opportunities in some of its sub-sectors, and contracting in others. Nor are barriers entirely lacking. Information, capital, and quite likely a series of personal contacts are necessary to

set up even as a sheet trader. It is quite important and relevant that empirical studies have confirmed, quite contrary to initial expectations of many, that the informal sector is not peopled dominantly by migrants.

The main point is that Bank research in particular, instead of perfecting and adapting what is a dominant mode of academic analysis, should encourage a fresh look at the heterogenesty of urban employment opportunities. This, and the need to introduce a more dynamic view of the mobility among such opportunities, is what motivates proposals for further research in urban labor markets (22). It would be a pity if such a study degenerated into empirical description, like others earlier commented upon. Here is a fruitful, but still apparently unconsummated, opportunity to collaboration between those who have been engaged in more theoretical efforts, and those with good empirical instincts.

It is certainly a strong point of the informal sector article (13) that it brings to bear the mounting Bank empirical evidence relating to such markets. It does less well in presenting a cogent dynamic theory — especially as the parameters in the model remain fixed. That effort stands in contrast with the review of alternative theories in (9) that has limited concern with application, and does not direct itself to developing countries in a convincing fashion. There is no doubt the extent of wage differentials economies can efficiently tolerate is central to income distribution in the urban sector in developing countries. Unfortunately (9) does not shed much light on the relevant determinants.

Bank research along theoretical lines has neither provided new tools, nor classifications of labor markets that subsequent empirical research might utilize. Its empirical work has been perhaps more productive in identifying the urban poor. Among the studies of that type, (21) is useful

in bringing together what has been learned, as well as specifying the data requirements for accurate statistical mapping of urban employment and poverty. It is also one of the few papers prepared with the country economist explicitly in mind. For all its merits, its failure to articulate a policy rationale, and to relate the data requirements more directly to it, dilutes some of its impact. It is too comprehensive to serve as a manual, and too information oriented to raise the basic questions of how

-down works through employment creation.

Rural-urban Migration

The papers relating to this issue overlap those that relate explicitly either to the rural or the urban sector. Most have been theoretical extensions of the imperfection cum search models of Harin-Todaro vintage.

They thus contain specifications both of rural and urban labor markets, and focus on unemployment as the equilibrating variable setting the market determined wage rates.

The model, as set out for example in (11), is subject to question. It gives a role to migrant workers as the marginal urban labor force that is inconsistent with what we know about the selective character of migration and the jobs that they actually take. It makes the informal sector a caricature of what it is, where service employees frequently earn higher incomes than those in traditional manufactures. It is couched in terms of simple, one-period probabilities that ignore the dynamics of the search process. The latter, conditioned as it is by the availability of income transfers, attitudes about mobility, and the institutional characteristics of labor markets is presented in terms that are much too simple.

The model thus fails to be fully descriptive of rural-urban

interactions. At one level, it is entirely neo-classical in its assumptions of maximization and wage, price and quantity adjustments; at another, in the formal sector, it precludes terms of trade or wage changes. Little attention has been paid to the characteristics of decision-making under uncertainty, despite the central role of the probability calculus in the model. There is also uncertainty in agriculture where the vagaries of weather, prices, crop disease, etc. are at least as significant as the uncertainties of employment in the urban sector. Subsequent Bank work as in (15), (3) and (20) enter into some aspects of these limitations, but still within the restrictive ground rules of such search models that rationalize unemployment, rather than focus on employment creation.

The policy instruments in such models are few. Usually they are limited to altering the fixed urban wages, or modifying the shadow wage rate — of which more shortly. Again a special Bank perception of the problem, or the means of coping with migration through investment in regional poles, rural public works, special wage subsidies in urban areas, etc. is generally lacking. It would be difficult to identify uniquely Bank research among the total research performed on the subject. That speaks to its professionalism, but also the failure of the distinct focus that ought to characterize a Bank research effort, given its limited resources.

Education

Bank research on this subject has been concerned with a variety of dimensions of the influence of education upon employability and rates of return. Two of the papers examined are country specific, (12) and (15); two focus on the appropriate amount of resources to be directed to training.

The study of Malaysia (12) is useful in exploiting the income and school-leavers surveys for that country. Its limitations derive from its extensive statistical search for significant associations, and its limited attention to social returns that are the basis for public allocation decisions. Regression results that include a variety of associated variables (e.g. race and education) are not likely to be robust, the more so when education is entered as years of schooling independently of its quality. Older persons with equivalent schooling may have had different training that influences the estimate of the age-earnings profiles. The parallel study of Zaire (15) is subject to these same limitations.

More important, however, is the insufficient attention to the social returns in both countries. Central to the question are the extent and character of unemployment experienced by the educated, and their social productivity. Of their ultimate employment, after a period of waiting financed internally by families, leads them to redundant white collar and civil service work, the social returns obviously fall below the private return.

Calculating the latter, in the absence of specifying whether education has a productivity effect per the human capital approach, or merely serves a credentialling function, merely confirms an association between education and market wages. This issue was not broached in the two country studies, thus limiting the policy guidance the Bank might extend in these particular instances.

Social returns were the focus in (16) and (18), but within a restrictive framework that fully accepted years of schooling as productivity enhancing. The key addition is the assumption that education must be directed to those with greatest ability. The latter is presumed to be

measured by IQ and independent of wealth. That ignores the association that exists and the limitations of a social policy that ignored the poor of low innate ability that is the consequence of centuries of neglect. Implementation of such a policy presumes a prior set of equalizing opportunities that educational investment itself is intended to provide. The emphasis upon the limitations of education in incrementing IQ (18) is useful in indicating that schooling alone — in the absence of complementary investment in health, nutrition, etc. — cannot perform that task. Again the emphasis upon IQ — which other studies have suggested is not a dominant determinant of economic performance — weakens the argument.

Both of these papers, while aiming at central policy issues, fall short of the mark owing to their limited empirical content. If the questions are worth pursuing, they should be pushed in depth to the point where a convincing policy paper might be written that would mean developing a substantial project around the ideas. In such a topic area as education, in which the Bank does finance projects, it is disappointing to find only single efforts, not much related to a coherent strategy of employment creation.

Wage Determination and Shadow Wage Rates

All of the prior discussion bears on the significance of the market wage as a measure of social productivity. It is the latter that is wanted for analysis of individual projects. Two papers extend explicitly to this question, (14) and (20); most of the others do so implicitly in their characterization of the forces that determine the market wage.

Two observations are relevant. The first is that the emphasis upon critical reviews of the literature, (5), (9), (10), (17), (19), (20), as

well done as they have been, seems excessive relative to the limited use that has actually been made of them. Frequently conducted by consultants, the reviews languish for want of translation into more suitable Bank documents. Such useful distinctions as those between the hypothesis that higher wages can increase efficiency on the one hand, or serve to reduce labor turnover, on the other, should inform empirical research and country reporting. The emphases upon labor market rigidities should translate into studies to find out how pervasive fixed minimum wages in fact are. Casual observation would suggest that the dimensions of the wage bargain are numerous enough to allow for more flexibility than the models credit. The role of trade unions as a monopoly element, and their effects upon employment is an assumption that can be investigated empirically.

The fault is not with the quality of the reviews themselves: on the whole they bring together what is known on the subjects in an authoritative, critical and enlightening manner. Nor is is that they are excessively theoretical: on the whole, again, they approach their subjects from a practical perspective. The review in (5), for example, is focused on the efficiency of labor markets — a matter of direct Bank interest. (It is ironic, but telling, that it was written for external publication rather than internal dissemination.)

It rather lies with the limited resources for follow-up research, and the lack of a direct audience for the product in the operational division of the Bank. Unless the former are concentrated upon a small number of directed and inter-linked projects, and until better channels of communication are established, much of the effort will have a limited impact.

The second is the need to convert the complex taxonomies of shadow

wage rates under different labor market conditions into workable rules of thumb. There must be translation from the ideal to the practical. The merit of simply using the wage in the informal sector as the shadow wage is that it is readily attainable. Various of the papers reviewed, and still other Bank research, correctly show the limitations of such a measure when market wages depend upon migration, can influence efficiency, reflect a private return to education, etc. The challenge is to facilitate empirical identifications of such circumstances so a better, but still simple, formulation can be applied. In the absence of carrying the research one further step, much of its potential utility is lost.

Conclusion

This brief review has not been intended to single out particular papers for praise, or others for criticism. Virtually all in fact are of high professional attainment, as their increasing publication rate attests. Rather the intent has been to expose a style of Bank research upon employment questions that seems not fully to have satisfied Bank needs, or to have exploited its comparative advantage. It is this short-fall that has contributed to our particular recommendations about future priorities and institutional arrangements.

It is clear we are imposing different, and higher, standards than research is generally required to satisfy. That is a measure of how far the Bank's own program has come in the relatively few years of its existence, and the opportunities we see ahead for utilizing more effectively the talents and expertise of its staff.

Sample of Papers

- Anderson, D and M.W. Leiserson, 'Development Issues in Rural Non-Farm Employment,' Report No. 1577, April 15, 1977.
- 2. Barnum, H.N. and L. Squire, 'An Econometric Model of Agricultural Household," undated.
- 3. Beenstock, M., "Risk, Unemployment and Rural Urban Migration," May 31, 1977.
- Beenstock, M. and M.W. Leiserson, "Functional Review of the Treatment of Employment in the Bank's Country Economic Work," July 1977.
- Berry, A. and R.H. Sabot, "Labor Market Performance in Developing Countries," November 1977.
- Bose, S., "Some Aspects of Unskilled Labor Markets for Civil Construction in India: Observations Based on Field Investigation," WBSWP No. 223, November 1975.
- 7. Ho, S.P.S., "The Rural Non-Farm Sector in Taiwan," September 1976.
 - Industrial Development and Finance Department, "Employment Creation and Small Scale Enterprise Development," March 22, 1977.
 - Lal, D. "Theories of Urban Wage Structures in Developing Countries," September 30, 1976.
- Lipton, M., "Migration from Rural Areas of Poor Countries: The Impact on Rural Productivity and Income Distribution," August 1, 1977.
- 11. Mazumdar, D., "The Theory of Urban Underemployment in Less Developed Countries," WBSWP No. 198, February 1975.
- 12. Mazumdar, D., "Education and Employment in Urban Malaysia," August 1975.
- 13. Mazumdar, D., "The Urban Informal Sector," World Development, August 1976.
- 14. Mazumdar, D., "The Rural-Urban Wage Gap, Migration and the Shadow Wage," Oxford Economic Papers, November 1976.
- McCabe, J.L., "Education, Administered Wage Rates and Size Distribution of Income in Urban Zaire," November 1976.
- 16. Pinera S., and M. Selowsky, "The Economic Cost of the 'Internal' Brain Drain: Its Magnitude in Developing Countries," WBSWP No. 243, September 1976.
- Schuh, G.E., "Out-Migration, Rural Productivity and the Distribution of Income," August 1, 1977.

- 18. Selowsky, M., "A Note on Preschool Age Investment in Human Capital in Developing Countries - Economic Development and Cultural Change," July 1976.
- 19. Stern, N., "On Labor Markets in Less Developed Countries," March 1977.
- Stiglitz, J., "The Structure of Labor Markets and Shadow Prices in LDC's,"
 August 1, 1977.
- 21. Webb, R., "On the Statistical Mapping of Urban Poverty and Employment," WBSWP No. 227, January 1976.
- 22. Webb, R., "Proposal for Research on Urban Labor Markets in Latin America," December 1976.
- 23. Webb, R., "Framework for Research on Urban Labor Markets," December 1976.



HARVARD UNIVERSITY

DEPARTMENT OF ECONOMICS

CAMBRIDGE, MASSACHUSETTS 02138
March 22, 1978

Mr. Michael Beenstockx
Pàlicy Planning Division, room D441
The World Bank
1818 H Street, N. W.
Washington, D.C. 20433

Dear Michael:

A copy of the March 10th draft of the RPIDE report reached me yesterday morning, and I re-read it. It reads quite well, and I have no disagreement with the content. At points, a sharper position would seem to me warrammed. For example, I do not consider acceptances of the articles generated in the Bank research divisions for publication in refereed journals an adequate test of quality: the quality of papers in a number of such journals is not reassuring. And one could question the extent of emphasis on income distribution, internal to the countries, relatives to the possible emphasis on international income and growth rate differences; and implay the need for considering the work on income distribution and employment within the broader context of WB research. While our group was not empowered to take such a broader view, the need for it could still be noted. But my judgement on these matters is not sufficiently firm to argue for having it reflected in the final report.

There are still a numbers of bothersome errors in that affect one's ease and pleasure of reading the report. E.g., "modifications" in line 6 from top of p. v presumably means suggestions or recommendations; the title on top of p. vii is presumably Employment, not Education; where should be a cross-reference from Table 1, p.6, to ke list of the research projects in the annex to the main body of the report. In short, close editing is still needed, but I assume that it will be done after the responses from internal circulation of the current version are received.

We are leaving for Jerusalem on the 29th of this month, a week from today; expect to stay there from March 31st to May 3rd, and get back home by May 9th or 10th. While in Israel, I can be reached via Institute for Advanced Studies, Hebrew University of Jarusalem, Jerusalem, Israel

With all good wishes

sincerely

Simon Kuznets

67 Francis Avenue Cambridge, Mass.02138

OFFICE MEMORANDUM

TO: Mr. Michael Beenstock, PPR

DATE: March 22, 1978

FROM: Mark W. Leiserson, DED

SUBJECT: Preliminary Comments on Draft RAPIDE Report

- 1. In accordance with your memo of March 14, I have confined my comments, as you requested, to points which appear to be linked to "the Panel's ignorance about the Bank." By and large, I found myself in agreement with general thrust of the Report but there are certain aspects of the Report and its recommendations about which we have some questions. Presumably there will be ample opportunity for discussion of these more substantial issues after the submission of the final Panel Report.
- Employment Research: One particular aspect of the report which I found bothersome was the lack of clarity in the frame of reference underlying the discussion of employment research in the Bank. Although it may be true as stated on pp. 1-2 that the Panel concentrated its attention on the work of Income Distribution Division and the Employment and Rural Development Division the statement is nevertheless somewhat misleading. The concentration is much greater in the case of income distribution than employment. Virtually all of the research projects listed under the income distribution label were the direct responsibility of or directly involved the Income Distribution Division. This is true of only half of the research projects listed under the employment label. There are good and sufficient reasons for the Panel casting a "wider net" in the case of employment research but then care should be taken to reflect this in a discussion of DEDER's place in this more broadly defined area. It is difficult, for example, to see that any valid inferences can be drawn from the classification in Table 1 (p. 6) about differences in the "origination" of DRCID and DEDER research when 6 out of the 12 employment projects did not originate in DEDER.
- 3. A similar problem affects the discussion in Appendix C of the "representative sample" of research papers on employment. In contrast to the discussion of income distribution, the different origins and different contexts in which the individual papers were produced are not taken into account or made clear. As a consequence, some comments appear inappropriate (even when, coincidentally, they may be true!). The impression recorded (p.C 1 "of a series of papers that do not relate to any clear research strategy" would seem to be an inevitable result of treating all the papers as a sample of final research "output" without regard to the sometimes very different contexts in which they were produced. Five papers are characterized as "bibliographic essays" (Appendix C, p. 1) which have not, "guided continued bank research." However, only one Berry and Sabot (5) had a general survey of the literature as its primary purpose. Two Lipton (10) and Schuh (17) are among the six papers (only three of which appear in the "sample") contributed to the research workshop on rural-urban labor markets

Mr. Beenstock - 2 - March 22, 1978

interactions. The workshop has certainly guided our research if only, up till now, to steer us away from certain types of migration research until further progress is made in developing better analytic formulations for empirical testing. Both of the other two papers -- Lal (9) and Stern (19) -- were very preliminary explorations which were intended to be and are being used in developing future research efforts in particular labor market areas. In that sense they are much more research "inputs" than "outputs."

- 4. Furthermore, while there has been an effort in this Division to cluster its research efforts around rural and urban employment and labor markets and their interaction, this has not been the case for education and the application of shadow wage rates in Bank project analysis. The bulk of research in these areas is done elsewhere in the Bank and the few papers considered under these categories provide little or no basis for the general judgments drawn. In the absence of a much more thorough and clear discussion of the problems of relating research on employment to these broader areas it would seem to me better to drop these as separate sections, retaining whatever comments desired on the papers as individual, rather than "representative," pieces of work.
- Policy Related Work: The failure to discriminate sufficiently between the different contexts in which the various sampled items were produced also blurs some of the discussion of the relation between research and policy in Appendix C. The non-farm rural employment (1) paper and the small enterprise paper (8) are characterized (Appendix C, p. 3) as "atypical research products." In fact, both were prepared as very different sorts of inputs into the policy discussion processes of the Bank -- the former as simply an issues paper and the latter as a basis for new initiatives in the Bank's lending program. (Furthermore, no recognition is given in the discussion of the paper by Ho (7) that it was prepared as a background paper for the non-farm employment paper and should, perhaps, be judged in that context.) Since the Panel emphasizes (correctly in my judgment) the desirability of policy papers being a "culmination of the research process" (p. 29) it is disappointing that the one example of this -- the public works research project and the rural public works issues papers -- were not included in the sample selected by the Panel.
- 6. Project-oriented Research: Without denying the general desirability of increasing the operational relevance of Bank research at the project level, I believe the Panel report (pp. 25-26) reflects a much too sanguine view of the problem. On the one hand, it seems to me that the Panel underestimates the staff resources which would be needed to implement their suggestion (p. 9) for greater involvement of DPS at the project level that the DPS offer on a regular basis a "diagnosis and definition of the economic issues arising in the course of project design" which would then be pursued by consultants. On the other hand, the Panel report does not reflect the growing experience in the Bank with the difficulties and complications of designing and carrying out effective monitoring and evaluation

Departments to warrant specific conclusions about the forms an expansion

of DPS project related activities, if any, should take.

Institution Building: I wholeheartedly agree with the Panel 7. Report's emphasis on fostering research in developing countries and that such institution building can not be pursued "on the cheap" as a byproduct of the Bank's own research. But despite the recognition that a "sustained effort to fulfill the Bank's potential institutional contribution to developing countries" would represent a "major departure" for the Bank, the Report does not, it seems to me, make clear how major a departure it would be. In particular, no mention is made of the fact that such an effort would entail explicit abandonment of long standing policies ruling out direct "institutional support" of research organizations and requiring that collaborative research arrangements be made on a short-term project by project basis. The latter usually involved identification of specific research results of direct interest to the Bank and the specification of a detailed methodology acceptable to the Bank. By failing to emphasize that such conscious and deliberate Bank policies are a major constraint on any institution building efforts, the Report may leave the impression that the institution building objective can be pursued with only minor modifications in Bank practice. I just don't believe that is the case.

cc: Messrs. B. King, DED

A. Stoutjesdijk, DED

I. Little, DED

A. Choksi, VPD

Messrs. Chenery, Ahluwalia, Balassa, de Vries, Dubey, Duloy, Haq, Holsen, Jaycox, Karaosmanoglu, Lerdau, Picciotto, van der Tak Vergin, Waide

OFFICE MEMORANDUM

TO: Distribution below DATE: March 14, 1978

FROM:

Michael Beenstock

SUBJECT: RAPIDE Report

I attach a draft of the Report for your comments. At this stage we are more interested in possible errors of omission and commission based on the Panel's ignorance about the Bank rather than critical comments on the substance of the Report itself and its judgement. Once I have received these reactions a final version of the Report will be prepared. I would appreciate such replies by Wednesday, March 29.

Attachment

Messrs. Ahluwalia, Balassa, Chenery, Choksi, de Vries, Dubey, Duloy, Haq, Holsen, Jaycox, Karaosmanoglu, King, Leiserson, Lerdau, Little, Picciotto, van der Tak, Vergin, Waide

The World Bank

March 15, 1978

To: Mr. Michael Beenstock

Subject: RAPIDE Report

I have quickly read through the

RAPIDE report confining myself, as requested,

to detecting any "errors of omission

and commission based on the Panel's ignorance about

the Bank". I have not found any such errors.

Vinod Dubey

T 25 MEXICO DF FEB 20 1978 RHB HRS 17.75

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ALBERT FISHLOW

C/O MICHAEL BEENSTOCK

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Mr. Beenstock for addressee

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REF. RAPIDE. MY GENERAL IMPRESSION ABOUT THE DRAFT OF THE FINAL REPORT IS THAT IT ADEQUATELY COVERS IMPORTANT ISSUES SUCH AS:

THE NECESSITY THAT THE BANKS'S RESEARCH PROGRAM BE MORE CLOSELY RELATED TO POLICY QUESTIONS THAN TO STRICTLY ACADEMIC MOTIVATIONS, SO THAT IT WOULD BE POSSIBLE TO CORRECT THE EXISTENT LACK OF COHESICN

BETWEEN THE RESEARCH TASKS AND THE OPERATIONAL CONCERNS OF THE BANK.

IT IS ALSO WORTH MENTIONING THE RECOMMENDATION THAT THE BANK SHOULD

PAY MORE ATTENTION TO DATA COLLECTION, ELABORATION AND DISSEMINATION.

ALTHOUGH WE KNOW IT IS A HARD AND BORING TASK, IT IS AN INDISPEN-SABLE ONE IN EVERY SERIOUS INVESTIGATION.

IN SPITE OF THE SERIOUSNESS, DEEPNESS AND BROADNESS OF THE REPORT, THERE ARE SEVERAL ISSUES THAT SHOULD BE STRESSED IN THE RESEARCH AGENDA OF THE BANK AND ARE NOT MENTIONED YET: THE AGRARIAN PROBLEMS. AND DEVELOPMENT STRATEGIESS THE EMPLOYMENT POLICIES OF THE PUBLIC SECTOR IN DEVELOPING COUNTRIES WHICH DIFFER RADICALLY FROM THOSE FOLLOWED UP BY THE PRIVATE SECTOR (IN LATIN AMERICA, FOR EXAMPLE, WHERE MOST OF THE COUNTRIES HAVE VERY LARGE PUBLIC SECTORS, WE MAY NOTICE HOW OFTEN THE OFFICIAL FIRMS DO NOT PURSUE PROFIT MAXIMINI-ZATION GOALS AND FREQUENTLY THEIR HIRING POLICIES RESPOND MORE TO INSTITUTIONAL AND POLITICAL VARIABLES THAN TO THE SEARCH OF EFFICIENCY) THE MUCH TOO LARGE DEGREE OF AGREGATION IN THE BANK'S STUDIES REFERING TO PUBLIC WORKS (THEY SHOULL BREAK-DOWN PROJECTS TO THE ACTIVITY OR PROCESS LEVEL) AND THE EFFORTS THAT SHOUDL BE MADE TO INTEGRATE INCOME DISTRIBUTION INTO NATIONAL ACCOUNTS IN AS TIME SERIES FRAMEWORK ONCE THE REPRESENTATIVE COUNTRIES HAVE BEET SELECTED. LEOPOLDO SOLIS È

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WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Mr. B. B. King

DATE: February 2, 1978

FROM: Michael Beenstock

SUBJECT: RAPIDE Report: First Draft

Al asked me to provide you with a copy of the First Draft and to emphasize that it is for your eyes only. The Second Draft reflecting the Panel members' comments should be available around the end of this month or the beginning of next month and will be for circulation within the Bank. A Final Report would reflect the reactions we receive from this more general circulation and should be available towards the end of March.

We would greatly appreciate your reactions to the First Draft by say February, 15.

MICHAEL BEENSTOCK

RECEIVED

RAPIDE

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WASHINGTON DC

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COMMUNICATIONS DIVISION

Mr. Beenstock

MESSAGE FOR ALBERT FISHLOW:

I READ CAREFULLY TEXT OF RAPIDE REPORT AND AGREE WITH THE ANALYSIS AND CONCLUSIONS SUBSTANTIALLY.

THE REPORT CAPTURES WELL SOME OF THE POINTS I HAD WISHED TO ARTICULATE SUCH AS RESEARCH LINKAGE TO LDC INSTITUTIONS

AND ON CRITERIA OF CHOICE OF COUNTRIES FOR STUDY.

I WOULD LIKE TO CONGRATULATE AL FISHLOW FOR A REPORT WELL-DONE AND FEEL GLAD HAVING BEEN ASSOCIATED WITH IT.

WHILE I HAVE SOME COMMENTS. THEY SEEM MINOR ENOUGH AND THEREFORE I AM NOT SENDING THEM. BEST REGARDS

GERARDO SICAT NEDAPHIL

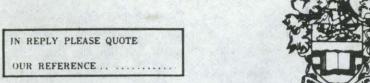
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THE UNIVERSITY OF THE WEST INDIES

FACULTY OF SOCIAL SCIENCES



TELEX NUMBER: UNIVADOS WB 257
CABLES: "UNIVADOS" BARBADOS
TELEPHONE: PBX 02191

DEPARTMENT OF ECONOMICS

15 February, 1978

Mr. Michael Beenstock World Bank 1818 H. Street, N.W. Washington D.C. 20433 USA

Dear Michael,

Albert's draft was a long time coming, because of bad weather and an airline strike.

I have given extra special attention to Appendix C, on the Employment papers. It does what I did not want to do, namely to point out to each author the defects of his paper. The danger is that of crying for the moon; i.e., expecting more from the author than the current state of knowledge permits, and criticising him for what he does not add to knowledge, instead of praising him for what he does add. I think there is some element of this in the present draft, and I do not see what good the Appendix does; but Al says that this is what the Bank wants, so I am willing to live with it as it stands.

I have two small points relating to the main body of the Report.

First, in the summary of our recommendations on page 1, reference is made exclusively to the priority of data collection and processing to the exclusion of our finding that the research is not sufficiently project-oriented or policy-oriented (penultimate sentence). This occurs again on page 31 (last sentence of second paragraph). Again on page 32 the summary upgrades data collection and downgrades orientation.

My priority is the reverse of this. I think the Report exaggerates what the Bank can do towards data collection. I agree that it should not use data in which it has little confidence, even in cross-country comparisons. I also agree that it should help the country's statistical and research institutions. But data collection is very expensive indeed (how much does it cost to get the figures of LDC debt?) and uses a lot of bodies at different levels, and depends on local statistical legislation for its powers. The Bank cannot take on either the finance or the organisation required. Neither can it "take responsibility - intellectual and financial - for the accuracy" of all the data it uses, as is demanded of it on page 6.

Mr. Michael Beenstock

15 February, 1978

I give equal or higher priority to the recommendations about project and policy orientation. All I am asking is that they should get equal billing on pages 1, 32 and 33.

My second point is that there are a great many recommendations scattered throughout the Report, most of which will get lost in the present format. It would help the reader and the Bank if they were all listed on pages 32-33 under the heading "List of Suggestions" (I prefer to call them that) instead of the current selective summary. This would be facilitated by numbering the paragraphs of the Report.

Al deserves special congratulations on an excellent job. With best wishes,

Yours sincerely

W.A. Lewis

c.c. Professor Fishlow

ALBERT FISHLOW

C/O MICHAEL BEENSTOCK

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Mr. Beenstock for addresses

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REF. RAPIDE. MY GENERAL IMPRESSION ABOUT THE DRAFT OF THE FINAL REPORT IS THAT IT ADEQUATELY COVERS IMPORTANT ISSUES SUCH AS:

THE NECESSITY THAT THE BANKS'S RESEARCH PROGRAM BE MORE CLOSELY

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TOKYO 28/27 13 0958

Feb. 13, 1978

LT

MR. MICHAEL BEENSTOCK

POLICY PLANNING

WCRLDBANK

WASHINGTONDC

DIST. _ MR. BEENSTOCK

RAPIDE REPORT EXCELLENT.

IT AND APPENDIX A NEED EDITING.

TRUST AL AND YOU.

WILL BE HOME FEB. 20.

SIMON

Distribution: Mr. Beenstock

0207 1325 011

D441

FOR MICHAEL BEENSTOCK. RECEIVED RAPIDE REPORT. I CONGRATULATE YOU AND FISHLOW FOR SUCH EXCELLENT REPORT. COVERS WELL MOST ISSUES WE DISCUSSED. I WOULD ADD TWO POINTS. ONE, WE DO NOT COME OUT ENCOURAGING THE BANK TO DO SOME RESEARCH ON BASIC NEEDS WITH THE CAVEATS I POINTED OUT IN MY REVIEW. I SUGGEST THIS BE ADDED IN VIEW OF ITS IMPORTANCE AND INTEREST. SECONDLY I SUGGESTED THAT THE BANK'S PRIMARY CONCERN ON POVERTY WITHIN NATIONAL BOUNDARIES IS WELL TAKEN

BUT MUST BE SUPPLEMENTED BY ITS CONCERN WITH INTERNATIONAL DISPARITIES, AND FOCUS PART OF ITS RESEARCH THERE.

J. RWEYEMAMU

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REMARKS

Some initial connents on it.

FROM

Benjamin B. King

ROOM NO.

EXTENSION

Page/Line	Comment
1/9 & 10	Research is covers
3/5	Current P&B estimate 4 percent.
4/top	Source of quote?
4/17	Missing tables
5/-	Underline headings (1) and (2). (Same on next page).
7/3	"Project-related enquiry." Muda (671-17), Nigeria (671-30)
	and Indus (671-45) are examples. Requires acceptance of
	DPS involvement in project evaluation design. Hitherto a
	problem; still is to large extent.
7/13	Some examples of project design assistance in CHAC chapter
	in last year's report. Limited initiative in regions;
	limited resources in DPS.
7/15	Large, additional; no. Small, additional; yes. Actually 7
	it is no additional, but more commitments (WDR).
8/5	Agricultural prices project (671-42) was directly designed
	for policy paper; too tight a schedule as a result. Point
	well taken, but additional point is that policy/issue papers
	should discuss research needs explicitly and thoroughly.
	In fact research/policy link has been bad; still is.
10/17 ff	Shouldn't the discussion acknowledge the examples of close
	country collaboration (Malaysia, Colombia, Korea, Mexico)
	before saying this is not enough? How about (a) the resource
	cost and (b) the acceptance level in the country? Text
	should show awareness that major changes and increases
	might be involved.

11/20	See also 8/5 comment.
13/25 ff	Again the cost; the acceptance of developing countries;
	the interagency jurisdictional problems.
18/6	benefit from
18/9	effective protection
20/17	One should go beyond Do not understand this sentence.
22/16	laissez- <u>faire</u> /
22/17 ff.	(a) On user side, no mechanism for expressing positive
	opinion (requirements).
	(b) On researcher side, very little sense of presentation
	needs.
23/11 ff.	Very important issue. Trouble is that <u>all</u> sides of the
	house are intent on pushing results upwards. No brownie
	points for lateral communication; despite this, a fair
	amount goes on.
24/1	See 8/5 comment.
25/-	My own view, for what it is worth, is that the kind of
	paper required (intellectually respectable, but not
	narrowly technical) needs production <u>first</u> . The
	publication medium is secondary.
27/1	An Editorial Subcommittee already exists.
27/20	Research results are presented from time to time, but
	there is sometimes a conflict with operational relation-
	ships (Brazil, 670-73).

28/17

No reason why working papers themselves shouldn't be adapted. We have encouraged (with only mild success) papers with lay introductory chapters. Better than yet another series? See also 25/- above.

28/25

A second initiative

29/last

Seems garbled.

30/5

I have my doubts whether the R.C. as present constituted could perform the function. Confess the right alternative escapes me.

30/9

This, in effect, means giving producers more funds for initial phases of projects on their own, perhaps notifying the R.C. in advance in a "no objection" basis (to prevent disappointments if R.C. doesn't think much of the whole idea). As presented, I find this suggestion too vague, though its thrust is in the right direction. Not as simple a problem as it seems; we have a lot of guffy departmental stuff as it is.

30/19

Right on. This process was imposed by the Board.

BANXICO EXT MEX

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MR. ALBERT FISHLOW

1977 DEC 13 PH 2: 00

C/0

MR. MICHAEL BEENSTOCK

POLICY PLANNING DIVISION

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BANK RESEARCH

EMPLOYMENT

WE WILL LOOK AT THE SUBJECT FROM THREE DIFFERENT POINTS OF VIEW.

FIRST OF ALL, WHICH ARE THE RELEVANT TOPICS THAT ONE WOULD LIKE

TO BE INFORMED ABOUT WHILE APPROACHING THE SUBJECT. IN SECOND

PLACE, WE WOULD LIKE TO PICK OUT THOSE A TOPICS WHICH ARE IMPORTANT

TO THE BANK DURING THE DESIGN OF LENDING PROJECTS AND FINALLY,

WE WILL POINT OUT IN WHAT DIRECTION SHOULD THE BANK'S RESEARCH BE

ORIENTED TO IMPROVE KNOWLEDGE OF THE AREA AND SUPPORT OF ITS

LENDING OPERATIONS.

WHILE STUDYING THE EMPLOYMENT LEVEL IN THE DEVELOPING COUNTRIES

ONE WOULD LIKE TO KNOW IF THE LABOR MARKETS ARE FRAGMENTED OR NOT,

IF THEY ARE INTEGRATED TO WHAT DEGREE?, IF ARE EFFICIENT TO WHAT

DEGREE?, AND TO WHAT EXTENT THE SOURCES OF FRICTION ARE IMPORTANT

(INFORMATION, RISK COST, CULTURAL AND INSTITUTIONAL BARRIERS,

SKILL DEFICIENCIES, ETC.)

IT IS DESIRABLE TO KNOW TO WHAT DEGREE NEOCLASSICAL PRESCRIPTIONS

APPLY: IF MINIMUM WAGE REGULATIONS (AND THE FRINGE CENEFITS THAT

THEY INVOLVE) CONSTITUTE OBSTACLES TO EMPLOYMENT CREATION.

DOES IT FAVOR THE SUBSTITUTION OF CAPITL FOR LABOR? TO ANSWER THESE QUESTIONS WE WOULD LIKE TO KNOW HOW MINIMUM WAGE DEGULATIONS HAMPER LABOR ARSORPTION AND HOW, IT IS AFFECTED BY ITS COVERAGE AND REGIONAL DIFFERENCES. ALSO WE SHOULD INVESTIGATE THE LABOR UNIONS ROLE IN THE WAGES BARGAIN, THEIR LEGAL AND INSTITUTIONAL SITUATION, THEIR COMPOSITION, STRUCTURE AND LINKS WITH THE STATE AND TO WHAT EXTENT THEY ALSO CAUSE UNEMPLOYMENT. ALSO, IT IS IMPORTANT TO SEARCH OUT ABOUT THE DEGREE OF CONESION AND INDEPENDENT UNIONS AND THEIR CONDUCT PATTERN.

WHILE FOCUSING ON THE UNEMPLOYMENT SIDE OF THE PROBLEM WE MUST

UNDERSTAND ITS COMPOSITION (TO WHAT EXTENT IT IS FRICTIONAL AND

TO WHAT EXTENT IT IS STRUCTURAL?), THE DIFFERENCE DETWEEN TRUE

UNDEREMPLOYMENT AND SEASONAL UNEMPLOYMENT, IS THE DIVERGENCE

EETWEEN THE SOCIAL AND THE PRIVATE COST OF BEING UNEMPLOYED AND

WHICH IS THE UNEMPLOYMENT'S COMPOSITION OF THE "INFORMAL" SECTOR.

BECAUSE IN THE DEVELOPING COUNTRIES THE GOVERNMENT ITSELF IS OFTEN

A MAYOR EMPLOYER, WE SHOULD INVESTIGATE THE DIFFERENCES BETWEEN

PUBLIC AND PRIVATE EMPLOYMENT POLICIES AND PRACTICES. IN THESE

COUNTRIES THE RURAL SECTOR PLAYS AN IMPORTANT ROLE AS A LABOR

SUPPLIER TO THE REST OF THE ECONOMIC SYSTEM, THEN, WE SHOULD ANALYZE

THE STRUCTURE CT THE LAMD-TENURY, 'ITS RELATIONSHIP WITH EMPLOYMENT

AND THE POSIBILITY AND EFFECTS OF AN AGRARIAN REFORM AND ITS IMPACT

ON LABOR ASSORPTION.

THE WORLD BANK PERFORMS TWO FUNCTIONS WITH DIFFERENT REQUIREMENTS,

THE FIRST ONE IS TO ADVICE ECONOMIC DEVELOPMENT POLICIES IN ITS

BORROWING MEMBERS AND, SECOND, TO HAVE THE RELEVANT INFORMATION

FOR SPECIFIC PROYECTS DESIGNS. IN THIS MATTER THE FOLLOWING POINTS

SHOULD BE PAST OF A RESEARCH AGENDA:

- 1. UNEMPLOYMENT COMPOSITION OF THE 'INFORMAL' SECTOR
- 2. UNEMPLOYMENT STRUCTURE
- 3. LABOR MARKETS EFFICIENCY

LABOR UNION PERFORMANCE AND COVERAGE

MINIMUM WAGE REGULATION

WE HAVE INDENTIFIED STUDIES BY THE WORLD BANK'S STAFF ONLY ON ISSUES 1 AND 3. THERE IS A WIDE GAP TO BE FILLED IN THIS MATTER,

ESPECIALLY SINCE AMONG THE ASPECTS THAT HAVE RECEIVED LESS ATTENTION FROM THE BANK STAND OUT THE INSTITUTIONAL VARIABLES LIKE LABOR UNIONS, MINIMUM WAGES AND LAND OWNERSHIP.

PUBLIC WORKS

PUBLIC WORKS PROGRAMS HAVE APPEARED ATTRACTIVE TO AN INCREASING NUMBER OF GOVERNMENTS WHILE ATTACKING THE UNEMPLOYMENT AND POVERTY PROBLEMS. ON THIS TOPIC IT WOULD BE USEFUL TO KNOW IF THE SUBSTITUTION OF LABOR FOR CAPITAL IS TECHNICALLY FEASIBLE, IF IT IS SO, WE WOULD LIKE TO FIND OUT UNDER WHAT CONDITIONS IT IS EFFICIENT FROM AN ECONOMIC POINT OF VIEW.

WHILE SUBSTITUTING LABOR FOR CAPITAL, WE SHOULD KNOW THE DIFFERENCE
BETWEEN PROYECTS BUILDED WITH LABOR INTENSIVE TECHNIQUES AND THOSE
BUILDED WITH CAPITAL INTENSIVE METHODS. DOES THE DURABILITY

OF THE WORK DEPEND ON THE TECHNIQUE USED? WHAT ARE THE COST ADVANTA
GES IF ANY OF LABOR INTENSIVE METHODS?. IN THIS MATTER IT LOOKS LIKE
THE BANK'S RESEARCH HAS A LONG WAY TO GO AND THE DEGREE OF AGREGATION
IS TOO HIGH FOR USEFUL PROYECT ADVICE AND POLICY DECISIONS: I.E.,
PROYECTS SUCH AS, ROAD SHOULD BE BROKEN DOWN TO SPECIFIC ACTIVITIES
BEFORE BEING ABLE TO DEDUCE WHICH OF THESE CAN BE EFFICIENTLY
UNDERTAKEN BY LABOR-INTENSIVE METHODS.

NB

ONE OF THE MAIN OBJECTIVES OF THESE PROGRAMS IS TO CREATE NEW SOURCES OF EMPLOYMENT AND INCOME FOR LOW INCOME GROUPS, SO WE SHOULD LOOK OUT FOR THE ROLE THAT PUBLIC WORKS PLAY IN THE INCOME DISTRIBUTION PROCESS. IS IT POSSIBLE TO ARMONIZE THE EFFICIENCY AND THE EQUITY GOALS? IT IS INTERESTING TO NOTE THAT THE BANK'S RESEARCH POINT THAT EMPLOYMENT SOURCES ARE OFTEN ONLY TEMPORAL AND FIND THAT PUBLIC WORKS WILL GENERALLY SERVE -DURING THE OPERATION PHASE TO REINFORCE PATHER THAN CHANGE THE EXISTING DISTRIBUTION OF ASSETS AND INCOME.

WHILE DESIGNING THE PROJECT IT IS ESSENTIAL TO IDENTIFY'
THE INSTITUTIONAL VARIABLES THAT WOULD HAVE INFLUENCE OVER THE
RESULTS.

PUBLIC WORKS PROJECTS REQUIRE CAREFULLY CONCEIVED AND MANAGED

ADMINISTRATIVE SYSTEMS, THEN, IT IS NECESSARY TO PAY ATTENTION TO

THIS ISSUE WHICH HINGES OVER ITS COATS. WE ALSO MUST INVESTIGATE

WHICH ARE THE BEST HIRING METHODS.

THE BANK'S RESEARCH MUST BE FOCUSED TO COVER THESE ISSUES, PAYING SPECIAL ATTENTION TO THE SPECIFIC CHARACTERISTICS OF EACH PROGRAM, LOOKING TO ATTAIN THE DEGREE OF DETAIL NECESSARY TO ANSWER THE QUESTIONS: IF BUILDING A ROAD DISTINGUISH BETWEEN MOVING DIST, (SOLID OR SOLFT), BUILDING THE EARTH WORKS, CONSTRUCTING THE DRAINAGE, ETC.

INCOME DISTRIBUTION IN LATIN AMERICA

OF SPECIAL CONCERN TO THE BANK -AND ALSO TO SCHOLARS STUDYING THE ECONOMIC PROBLEMS OF LATIN AMERICA- IS TO PUT INTO FOCUS THE IN EQUALITIES THAT EXIST IN INCOME DISTRIBUTION, THROUGHOUT THE AREA. IN ORDER TO DO THIS, IT IS NECESARY TO HAVE A MORE DETAILED EXPLANATION OF THE MAIN CAUSES OF THESE INEQUALITIES, SUCH AS ASSET OWNER SHIP, EDUCATION, SOCIO-ECONOMIC STATUS, ETC. MOREOVER, THE WAY IN WHICH THIS IS MEASURED MUST NECESSARYLY BE REVISED, IN AN ATTEMPT TO FIND A MORE APPROPUNE AND ACCURATE METHOD TO OBTAIN THE BANK' GOALS. ASIDE FROM THESE TWO POINTS, THERE IS A NEED TO IMPROVE UPON THE PERIODICITY AND SYSTEMATIZATION OF CENSUESES, AND SAMPLE SURVEYS, AS WELL AS DESIGNING FORMS OF EXPRESSING THE DATE THAT ARE APPLICABLE TO THE WHOLE REGION. EFFORTS SHOULD BE MADE TO INTEGRATE INCOME DISTRIBUTION INTO NATIONAL ACCOUNTS IN TIME SERIES FRAMEWORK.

THE BANK'S RESEARCH IS PAYING ATTENTION TO CERTAIN ITEMS IN WHICH
THERE ARE COMMON SHORTCOMINGS IN PREVIOUS RESEARCH WORK DONE IN
THE REGION: I) UNDERESTIMATION OF INCOME II) TAKE INTO ACCOUNT
THE SOCIO-ECONOMIC STRATUS AND DEMOGRAPHIC AND OCCUPATIONAL
STRUCTURES III) COVER THE ENTIRE COUNTRY IN BOTH, RURAL AND URBAN
AREAS AND, IV) WEIGH SUCH SIGNIFICANT VARIABLES AS EDUCATION AND
AGE.

MUCH MORE CAN BE DONE ONCE THE NATURAL AND QUALITY OF INFORMATION ON INCOME DISTRIBUTION IN THE AREA FIRST, THE POSSIBILITY SHOULD BE SSTUDIED TO CONDUCT RESEARCH WORK IN INCOME DISTRIBUTION IN THE COUNTRIES OF THE REGION, ENDER SOME COMMON GUIDELINES PROVIDED BY THE BANK, AND WITH THE BANK'S FINANCIAL SUPPORT. SUCH RESEARCH SHOULD COVER BOTH, IMPROVEMENTS OF DATA ON INCOME DISTRIBUTION AND RELATED VARIABLES OVER TIME, AND REFINING OF THE ANALYTICAL FROMEWORK TO ASSES IT. PROPER COVERAGE COULD BE GIVEN TO THE IMPACT THE BANK'S LENDING POLICIES UPON DISTRIBUTIONAL EFFECTS.

BANCO DE MEXICO, S.A.-LIC. LEOPOLDO SOLIS.-

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RAPIDE

Income Distribution

by

Albert Fishlow Chairman, RAPIDE

December 12, 1977

Income Distribution

1. Comparative Advantage

The Bank research program in income distribution has been characterized by a self-conscious effort to assess research prospects and priorities in this area. Early on, a comprehensive overview was undertaken. In addition, the Bank has been prominent in stimulating and pursuing research in the subject. The initial Bellagio conference in 1973 and the publication of Redistribution With Growth soon after, and the research of outside consultants it has supported has meant a leadership role for the program.

A conscious strategy, elaborated for and endorsed by the Research Committee in 1975, has emphasized three principal sets of studies for the Bank itself to prosecute:

- 'empirical, information-oriented, analyses
- ' construction of economywide models for policy experiments
- examination of consequences of policy interventions within a more limited framework.

The first two components up to now have constituted the largest part of the agenda.

These priorities were explicitly chosen to conform to Bank opportunities and needs. One of the earliest requirements was a better and comparative sense of how the income distribution in fact moved and changed with different levels of income and growth; what effect public consumption might have in rectifying inequalities of monetary receipts; the characteristics and identity of those who were poorest; etc. There was much discussion at that time of iron laws of development, inevitable impoverishment

and the like, whose factual basis was questionable at the least, but which influenced attitudes and policies. An initial compilation of statistics on the size distribution was undertaken and published, and subsequently used in cross-section analysis. The intent was not only to organize the extant data base but to extend it. Two large projects were financed to exploit substantial survey data available in Latin America and East Asia. These were undertaken in conjunction with regional research institutions in both instances.

These comparative, data-intensive efforts were complemented by country-specific research. Among countries singled out for attention have been Taiwan, Thailand, Colombia, Malaysia, Brazil, and Korea. These have been the objects of empirical analysis - the first four - and more elaborate modeling exercises - the last three. Malaysia, as is clear from the overlap, has been a particular focus of interest. In addition to descriptive study of the income distribution and its determinants, and the preparation of a price-endogenous model to describe the processes, the Muda Regional Economy has been a subject of attention, related to Bank project finance in the region.

That the program undertaken was consistent with Bank comparative advantage (and needs) is both clear and insufficient. It is also relevant to ask what kinds of research were not undertaken, as well as to examine more closely the kinds of specific results obtained in those that were. For one, the focus on economywide modeling meant less attention to more narrowly directed sectoral or regional exercises emphasizing distributional processes in more detail. In particular, the rural sector - in view of

Pary !

its contribution to poverty - might have been worthy of more concern.

Backward regions are another evidence of dualism contributing to aggregate inequality whose attributes could have been singled out. In retrospect, at least, the general equilibrium issues featured in the large and complicated models might better have been grappled with theoretically rather than empirically. But even prospectively, since one of the principal pillars of Bank policy has become the objective of increasing productivity among the rural poor, more research directed to even the first-order distributional effects of alternative policies might well have been indicated.

Moreover, these economywide models for Korea and Brazil, at least, were not in fact country intensive, and capable of providing much guidance to policy makers. They did not replicate in a convincing fashion the behavioral characteristics of the economies in question - in part because the general equilibrium approach was a competing style of research that detracted from efforts to understand the production relationships, investment behavior, or nature of technological change in those economies. Ultimately, these characteristics define the scope for a distributive policy as much or more than the second-order interactions that an explicit general equilibrium framework allows. Both of those large models instead became more academic exercises, skillful and informative, but too complicated for evaluation of policy options. The end results were thus not consistent with the original rationale for their undertaking.

So, too, it may be said that the chosen emphasis on the empirical side - the cross-section choice - meant less attention to analyzing in detail the experience of distribution changes over time in selected

countries, and their sociopolitical content. Instead of compiling and organizing various surveys, attention might have been directed to historical processes, and the considerable role that political decisions have played in affecting the distribution as well as the frustrations encountered by market diluting effects of relative price changes, scarcities of supply, etc. The nature of the trade-off between the efficiency criteria emphasized by the Bank and other international agencies and the attempt to pursue policies to ameliorate inequality also might have warranted more concern - particularly by the Bank.

The research program, while consistent with the Bank's comparative advantage, thus perhaps did not exploit it fully. Equally important, it did not anticipate continuing Bank needs. In focusing on income distribution per se, the mandate was perhaps too narrowly conceived. Rather than the size distribution - difficult to measure, especially continuously, and also difficult to model without simulating individuals - an object for more intensive study could have been the shares of particular socioeconomic groups. Their average incomes are more easily ascertained from information related to production and the national accounts, and can therefore be followed more readily over time. Even from surveys, mean incomes of particul-r groups can frequently be estimated on a consistent basis even though their relative size can be influenced by different sampling procudres at different times.

Although identification of the characteristics of poverty and methods for its alleviation have figured prominently in Bank policy discussion, research in the Division was brought to bear on the issue only

modestly. Generalizations about the composition of target groups, their constancy over time, the extent of access to public consumption, and its varying quality, the elements of such consumption most complementary to production, have not been among the topics receiving most attention. This has meant a smaller input from the research program into definition and meaningfulness of absolute poverty lines, for example, than might have been desirable. It has also meant that current discussions within the Bank on the appropriateness of a "basic needs strategy" has not been much informed by prior empirical analysis.

In the execution of the research itself there has not been full realization of the potential gains of institutional, as contrasted with individual, research: structure emphasizing cooperative and cumulative analysis. Much of the income distribution research has seemingly been conducted in isolation. Three large studies using decomposition analysis have proceeded using methods that are different and not purposefully. No explicit comparisons of the techniques, and their relative advantages and disadvantages preceded the choice. Indeed, simultaneous theoretical work within the DRC on the measurement and decomposition of the Gini coefficient had little impact upon its use in the Taiwan study. Two large general equilibrium models were pursued with little intersection and mutual learning. The Colombia study on public expenditures and income distribution made no reference to other survey results for the same country being used in another project.

One of the explanations for this diversity is the large reliance upon consultants on many projects. There was limited Bank staff involvement in their design and execution. In some cases the research would

on whom.

have been pursued independently of Bank support, and relied only marginally upon Bank association for access to data, collaboration, etc. The large external component of the program can be ascribed to a desire for early results, and more inexpensively that greater reliance upon bank personnel would have entailed. Unfortunately, there were also costs, and the benefits of timeliness were not in fact attained in most cases. There were typically longer lags in such instances.

Examined more closely, then, the structure of the income distribution research program has been less articulated and exploitative of Bank comparative advantage than initial impression, or perhaps export rationalization, suggests. Many of these lessons have already been learned, and have progressively influenced priorities. Some could probably not have been anticipated. Some could not have been acted upon sooner because many projects have extended beyond anticipated termination, locking the Division into research for which only a dwindling audience remains. The rapidity with which Bank policy in this area has changed has made long term research more precarious than at other times or in other subjects. Hindsight constitutes a considerable comparative advantage.

2. Quality

Quality, like beauty, is not independent of the beholder. Objective measures are few. Even academic publication is suspect when the initial objective was primarily to influence Bank and member country policy rather than to advance general knowledge. Yet some general remarks are relevant, primarily as a guide to the future.

A first generalization is that the data intensive research has not fared as well as it should have. Many aspects of this question have been treated extensively as a separate topic. Here I wish to stress the ambiguity of the Bank view about the importance of data. Until it is resolved, much of the empirically oriented research will be suspect, inconclusive, and necessarily unsatisfactory. At one level the Bank compiles and issues considerable information; at another it insists that this is not its responsibility, but that of others. Mere compilation in a field whose statistical difficulties are as thoroughgoing as that of income distribution inevitably must lead to making available data of limited reliability, comparability and continuity. A serious program must pay attention to all three. The careful assessment of the reliability of household surveys and censuses in Latin America runs the high risk of coming to naught, despite considerable investment up to now. Unless the collection is influenced by the research, and consistent efforts are made to replicate the process for successive surveys, an important opportunity will have been lost.

Alternatively, a different framework for data collection (and analysis) might be selected, such as one focusing on the shares of particular socio-economic groups. But unless the Bank attitute undergoes change, merely substituting a different accounting system will lead to little better results. Choices must be made, and also followed through.

The distribution data, both the international as well as the regional, have been used in two blocs of research. One has been a cross-section analysis of the "U-curve" of income distribution as per capita income

varies. It is the most sophisticated of its genre, both with regard to its caveats and its carefully qualified conclusions. It is perhaps disappointing nonetheless that other Bank research on real income comparisons was not utilized more integrally in the comparisons, and that the exercise was not an introduction to a more serious comparative study of fewer countries with more reliable information. The subject is of importance, because it raises the larger questions about the trade-off between growth and inequality that inevitably influence predispositions and attitudes. Had the objective been a study to illuminate that issue, and drawing upon a variety of data and results, the research might have been more effective. Then the issue of different development strategies could have been confronted directly. The parallel series of inequality decompositions conducted for many Latin American countries remains devoid of substantive discussion of differences among them in the way labor and capital markets functioned, or of differences in the relative supplies of skilled labor. What might have been a rich comparative analysis among diverse countries has been reduced to large scale statistical manipulation that falls short of interpretative conclusion.

The country study of Taiwan, to take another illustration, focuses upon Gini coefficient decompositions of all shapes and kinds over time, while failing to look more deeply into the factors favoring the spread of rural industry, the diminishing dualism within sectors, the reasons why technological change favored wage income, etc.

A last example, the distribution of public consumption in Colombia, starts with a well defined and relevant policy problem. Yet it too fails

to take into account differences in the quality of such services to persons in different income strata, although that would seem central to the issue. The policy context of deciding upon the distribution of public services is absent. Nor does it substantively compare its rather different results with earlier ones, and seek to assess whether the services have in fact become more widely available, or whether it is statistical artifact.

Empirical studies necessarily suffer from a certain inconclusiveness, whether because of the character of the underlying data or the difficulty of decisively rejecting alternative hypotheses. What makes for high quality is either new and reliable information, or rich insights that transcend the statistical manipulation per se. The Bank efforts have given too little weight to both.

The completed research upon general equilibrium models shares some of the same attraction to the mechanics at the expense of some basic questions. This seems true in three respects. One is the inattention to the limitations of what remain "standard" economic models for the analysis of the size distribution of income. The basic rules for distributing income to persons, as opposed to economic agents, are not an integral part of such models. Calculation of the distribution then must rely on static and nonbehavioral constants that have been computed from earlier Census or survey data. Indeed, in the Korea model, for every socio-economic group but one, the within-group variance among persons is simply assumed unchanged. In the Brazil model this component is excluded from consideration. Yet the level of such variance, and its changes can play and have a large

role in the observed economywide changes.

Until the focus is upon individuals, moreover, it will not be clear how the permanent, lifetime distribution varies in response to economic change and economic opportunity. Changing aggregate measures may be consistent with very different profiles of inequality in welfare among individuals. The class of models selected for experimentation do not attack such questions.

Secondly, although the models are quite large vy virtue of their inclusion of many groups and sectors, what counts for many of the results are the macroeconomic specifications, and their effect upon the functional distribution. Such models, despite their complexity, seem to equilibrate by taking some nominal magnitudes as given, and adjusting the overall price level, or the terms of trade, to accommodate real demand and supply. Such effects are the basis for much of the potential distributional change and its generally limited extent. Whether those processes accurately define the structure of those economies is not a decided matter. Yet despite its importance it has not been a subject for empirical investigation in the particular economies prior to model construction and specification.

In the third place, too little attention has been paid to capturing the differences among sectors (or groups) rather than multiplying their number. It is such differences, in the general equilibrium framework, that permit of distributional changes. The level of the elasticity of substitution in the diverse sectors can count considerably over the medium term. Model specification largely calibrated to a few years cannot adequately assign values, differentiate structures, or accurately decide

uponupon the nature of technological change. These questions are partially obscured rather than illuminated by the computational complexity of the models undertaken.

It is important to note, however, that these criticisms have not only come to be appreciated internally, but that they and others have tended to evolve from critical discussion within the DRC. That is, of course, a highly useful and positive development. It cancels some, but not all, of the disappointment with model results.

This discussion of the models makes clear a disparity between academic and Bank quality criteria. Both projects have lead to published results that have satisfied outside references. Yet for internal purposes neither successfully was able to subject distribution policy packages in their particular countries to believable second-order, general equalibrium effects. In Korea increased agricultural productivity apparently goes to naught, but largely because the model imposes limited demand for output and hence induces adverse terms of trade changed. In Brazil, it takes wage increases and inflation to improve the distribution - but only because some service incomes are nominally specified, and the consequences of inflation are not fully traced through.

The quality of research has been best - professionally - where the projects have been small, and the output an intermediate input rather than a final product. These have frequently been efforts in which the particular interests of the researcher have dominated and in which the proof of the tractability of the problem consists in the paper itself. In some cases the research has taken the direct form of more theoretical

writing; and in others application has been stressed, but only illustratively rather than substantively. These are constributions that are publishable and useful, and advance thinking on a range of problems. But they are not particular to the Bank or directed immediately at the various issues it is most concerned with.

Overall, the quality of work - with the prominent exception of that dedicated to expanding the data base - compares favorably with that done elsewhere in this field. Bank personnel and consultants are, on the whole, highly qualified and professional in their research; the Bank has attracted a research staff of international repute. What has not emerged, yet, in the field of income distribution, is seminal research that has changed the conception of the field, or an articulated body of output that has advanced design of policies to ameliorate inequality. Such criteria may be unfairly rigorous. But perhaps not. To put it yet another way, the research product has not consistently fed into Bank policy papers, nor as yet had a large catalytic impact upon the way outsiders have proceeded with their own work.

3. Articulation

This leads directly into the question of the relationship between

Bank research and operations. There is an undoubted tension in that

association. Some is inevitable because research activity involves a

longer perspective, solves few immediate problems or meets close deadlines.

Some disappointment can be explained by the large role of consultants in

income distribution research. Such persons have not been on the scene to

interact informally with other Bank staff in operational or regional offices.

Some lack of direct impact, however, also derives from the decision to isolate the Income Distribution Division from operations. This has been tantamount to the crea tion of a small research center, within a larger institute on development economics. That has some positive consequences. One of the attractions to research staff has been a lively and congenial atmosphere in which to pursue their work, and an independence that is an important element in creative scholarship. It also makes possible a concentration on research that can have an eventual, and sometimes indirect, effect upon the way that the Bank conducts its affairs.

Such an arrangement can therefore be defended. But it is not entirely consistent with widely expressed expectations that in-house research capacity should contribute to other parts of the Bank more immediately. The research product, and its service function, is certainly not fully understood, and values not entirely shared.

This reflects itself in the style and extent of intellectual interaction. Despite the institutional structure governing research that has evolved, and a variety of requirements that regions and operating divisions be consulted at many stages, there is relatively little effective collaboration in the planning or conduct of research. The position elsewhere in the Bank is typically one of laissez faire rather than active interest; and expressions of disappointment and lack of usability of the results is not infrequent.

In the case of the extensive research on income distribution surveys in Latin America, Bank staff in the region were unaware of its output. That information should have been helpful in analyzing the recent state of the

income distribution and its characteristics for purposes of country reports. The international cross-section study carries an academic rather than practical connotation, and does not seem to have altered a priori views regarding the trade-offs between growth and distribution. The economywide models are of modest interest to the country desks involved. For Brazil, the results are not even a matter for tolerant study. For Korea, a virtually completely independent exercise has been undertaken to construct what is regarded as a more useable analytic tool. These are not isolated examples.

In some cases, more fruitful contacts than these have been established. These seem to be based more on personal and informal ties than rule, and to vary considerably. Some research results, moreover, undoubtedly do trickle down through the Bank rather than by close reading of working papers and direct application of methods or findings. In the instance of income distribution research, moreover, there have been few directly useful techniques that could be readily adapted. Yet even when these links exist, they are not strong and inclusive. Malaysia, a much-studied country in this context, and one where researchers and country economists have gotten on well, is a case in point. While aware of, and keenly interested in the economywide descriptive monograph and the model, economosts working on Malaysia were not at all involved in any of the research relating to the Muda region.

Research results that are communicated by formal reports evoke limited response. Frequently the papers are already sufficiently advanced to be submitted for publication. Earlier drafts do not seem to be widely

distributed to those who should be among the intended audience. While seminars are held regularly during the project cycle, they tend to be technical in orientation and directed internally to the research sector of the Bank. This discourages Bank consumers from attendance and participation. Even after conclusion, it is not standard practice to arrange for discussion of the research report with potential users, motivating their interest and possible application. Nor are technical reports rewritten and reoriented so that they might be more easily accessible by putting into broader context the problem studied and the results obtained.

No formal requirement now exists to present the results of research in the particular countries that have been studied, or to disseminate techniques that have been employed in one or more countries to others that might be interested and able to apply them. While country reports are discussed in detail, involving as they do policy strategic considerations, research is not a matter for regular communication. Again there are exceptions, as in the apparent close ties with the Malaysian Office of Planning. But these again are irregular and dependent upon individual and sometimes even personal circumstances.

Not all research would qualify for such transmission, but much should - whether empirically oriented or more theoretical. The former is obviously directly relevant to the country studied; even for selected others the findings might motivate parallel research that could and should be performed internally. More theoretical advances that relate to measurements, say, would qualify in similar fashion. Such dissemination seems especially appropriate for the problem of income distribution

where hard internal decisions must be taken, and where a significant
Bank contribution could consist in improving the quality and expanding the
resources dedicated to such questions.

While it is common to place much of the blame on the research apparatus for this state of affairs, users cannot be held entirely innocent. It is not usual practice to examine research results with care to see what may or may not be applicable, even when the focus is one's own country, let alone more general reports. There are few instances in which detailed criticism and suggestions are received, or expected, from even a narrowly defined, interested set of consumers. The typical excuse of lack of time is not wholly acceptable or convincing.

It is fair to say that articulation constitutes one of the principal concerns that have been expressed, by researchers and users alike. In the field of income distribution it has been complicated by the fact that the most prominent results have not been of immediate interest. This has made the research still more marginal as other, and parallel investigations have been launched into urban poverty, basic needs, etc.

4. Developing Country Experience

5. The Research Structure

Over time, as the general research program has evolved, its administrative structure has become more formal and diligent. More careful

attention has been paid to pre-Research Committee evaluation, to phasing projects involving large expenditures, to assuring that adequate internal supervision is available when consultants are employed, etc. Efforts were further made to involve the Research Committee in policy decisions by requiring a series of overviews of research in specific fields. Income distribution was among these. Finally, the evaluation process has been progressively formalized to require responses to a small set of direct and relevant inquiries.

It is apparent that these measures have required projects to be better defined before they are financed. The Research Committee has even created a new category of small grants at the preproject stage to encourage more careful specification of methods and results. The vetting process, which also has an internal dimension with the DPS, has been effectively refined to avoid expenditures that are unlikely to yield useful results. The research structure has been able to learn lessons and make modifications.

What may be questioned, however, is whether that structure can positively stimulate research that will be both technically sound as well as highly relevant to the Bank. The Research Committee has not played, and does not seem capable of playing an initiating role; that responsibility has been left to individual researchers and various levels of management. Such a perspective lacks full understanding of where Bank policy is heading, and what the needs are for more effective performance. Individual researchers are not always aware of the particular issues that are recurring in different groups of countries, and which could benefit from systematic research attention.

What also is arguable, at the other end of the process, is whether the present system of evaluation, which takes individual projects in isolation, is adequate. Looking at the Brazil economywide model alone cannot lead to much more than endoresement of its scientific quality, on the one hand, and lament that there was not more interaction with the country desk, on the other. Considering that product jointly with the Korea exercise, and also in the context of other research being performed in Brazil, might induce more thoughtful statements by the relevant originating Divisions and more than a mere technical assessment by the evaluators. The present system is geared to the project and project cycle; an alternative evaluation framework could be subject defined and recurrent, independently of the particular phases of any individual projects.

Outside evaluation is presently employed at the publication phase, and not before. Its principal role has been one of quality control before external dissemination on a wide scale. An alternative procedure inserting such judgments earlier in the process is not a promising solution for the principal problems that have been identified. The issue is not one of inadequate rigor, and a technica-ly deficient staff. Rather, it is one of limited personnel resources, modest supervisory capacities, a very diversified research agenda, and an imperfect relationship with other Bank activities. Outsiders would either skew the objectives of the program to the scholarly needs of the field, or interpret the Bank's requirements secondhand. Neither is a substitute for internal decision.

FOREWORD

The enclosed tentative draft presents a critical examination of the empirical foundation of income distribution research, as excemplified by samples of <u>published</u> results by the World Bank. Section (E), pp. 43-51, contains a brief summary, and suggests the difficulties in trying to formulate defensible implications for the Bank's income-distribution research for the future.

November 1977

Simon Kuznets

Income Distribution--The Empirical Foundation Simon Kuznets

I. Introduction

Income distribution usually implies a social collective, comprising members who contributed to the product and all those whose material needs the product is designed to satisfy. The distribution is of claims (direct and indirect) to product secured by recipient units distinguishable in the population of the collective; and differences in real income so secured are of obvious interest-for orientation, analysis, and policy application. Cooperation among members of the collective in production calls for a view of the income distribution from the standpoints of adequacy, equity, and productivity-either currently or in the perspective of growth. Influences of income differentials on responses in the supply of labor and of other productive factors, on the one hand, and on structure of final demand, on the other, suggest analytical interrelations basic to understanding the economic functioning of the social collective, in the short and in the long run. The variety of policy tools available for modifying the income distribution or its effects in desirable directions, calls for adequate information on the income differentials, so that judgments of shortfalls relative to accepted goals, and knowledge of their analytical relations to other major aspects of the collective's

economy, can be used to explore policy options available for dealing with the shortfalls.

The social collective used can differ in scope and basis of identification -- from the widest, represented by all of mankind and reflecting the growing cooperation and interrelatedness among the world's peoples, to groups of countries within world population distinguished by geographical or economic criteria, to the narrower units of individual nation-states, to groups within the latter. Recipient units can vary from the narrowest and basically indivisible, such as a single person, or more realistically, a single family household (including one-person units), to significant socio-economic or related groups; and some groups may appear as recipient units at one level and as social collectives at another. Income concepts can differ, with differing emphasis on productivity and on relevance to needs; and on long vs. short-term levels. Finally, the approach may stress the full range of the income distribution -- from the lowest to the highest income per unit; or concentrate on a distinctive sub-range within it, e.g. on the low income groups as loci of low productivity and income inadequacy, or on the high income groups as of particular interest in revealing the conditions under which the high incomes are secured.

The differences in scope of the social collective, in the nature of the recipient unit, in the concept of income, and in the range of the income differentials studied, obviously yield a wide diversity of income distributions, full or truncated. The unifying element is that it is the individuals or groups of them, the living members of society, who are the indispensable recipient units -the differences in whose productivity or in access to means of satisfying material needs are the essence of any income distribution. And for our purposes here, of evaluating the adequacy of the empirical foundation of the work by the World Bank, and the major directions of its work on income distribution, further limiting choices can be Thus, it is the less developed countries that should be emphasized, in terms of the cross-section differences among them and of differences in growth rates in per capita (or per consuming unit) income over time. When we shift to internal income distributions within the countries, we can distinguish between those that use individuals or households as the basic recipient units, and classify them by the size of income per unit (referred to, for convenience, as size-distributions); and the distributions that allocate income among distinctive socio-economic (or related) groups, without dealing with the intra-group differences in income (referred to as group-distributions).

We begin the discussion with (II) -- differentials in per capita product levels and in recent growth rates among the less developed countries, for these differences loom large in world poverty and in the widening gap among the LDCs themselves, and the empirical foundation for such comparisons ought to be examined ahead of the review of the data base for internal income distributions. We then shift to (III) -- the internal size-distributions, in cross-section and over time, estimates used widely in the work of the World Bank in the field; and consider the limitations on the supply and quality of the data. In view of the apparent difficulties in establishing adequate measures of income differentials, or income adequacy, on the basis of the available size-distributions, particularly in the LDCs, we consider next the possible distributions among significant socio-economic and related groups (IV)xx--leaving aside the study of single distinctive groups such as the landless laborers, or small farmers, or the underemployed urban labor force still in adjustment as recent migrants from the countryside. The selection of priorities for World Bank work in the field of income distribution requires an examination of the purposes that such work is to satisfy: the difficulties in supply and quality of the data and the comparative advantages that the Bank may have in research aimed at overcoming these difficulties (V).

II. Income Inequalities among Developing Countries

In a recent report (no. 1674, July 1977), the World Bank divides developing countries (total population 1.93 billion) into five groups, four of them by level of GDP per capita, and the fifth comprising capital-deficit oil exporters. Omitting the latter group (with a population close to 290 million) and the top income group among the others (per capita in 1975 from over 1,000 to 2,000) comprising only 62 million, we find that of the remaining countries with some 1.58 billion population in 1975, the lowest group with a population of 1.00 billion showed an average GDP per capita of \$136, and the top (intermediate middle income) with a population of 0.36 billion showed a per capita GDP of \$905. Thus, the lowest group with over sixty percent of the total population had a per capita product less than a sixth of the top group with over 20 percent of the population. This may be wider income inequality than that suggested by crude findings for internal size distributions -- particularly considering that the latter are inflated by transient elements far more than in the country comparisons.

Furthermore, these income inequalities among the developing countries have widened in recent years—so that a substantial proportion of the spread in 1975 (or other recent year) is due to the disparities in growth rates in per capita product in the decade

to decade and a half back to 1960. Using the data for developing countries with a population of 10 million or over each in 1975, grouping them for 1974 in the lower income, lower middle income, and intermediate middle income groups (as they were classified in 1975), and using the growth rates for 1960-74 (and other data) given in World Bank Atlas 1976, we find that for 1974 the per capita GNP was 132, 314, and 837 dollars respectively, a ratio of 6.3 to 1, whereas the extrapolated per capitas in 1960 (in 1974 prices) were 108, 221, and 489 -- a ratio of only 4.5 to 1. implicit growth rates in per capita product were 1.4% per year for the lowest income group, 2.5% for the lower middle income group, and 3.9% for the intermediate middle income group (incidentally, the growth rates in population were 2.3, 2.7, and 2.9 percent respectively). Thus, over a third of the total relative spread in income inequality among the developing countries in 1974 (i.e. 6.3-1.0 = 5.3) was due to the differences in growth rates over the period back to 1960 [i.e. 5.3 minus (4.5 - 1.0) = 1.8].

Finally, one should note that per capita GDP or per capita GNP is, even given adequate basic data for estimation, a poor approximation to supply of means of satisfying consumption and other needs of the population. It is not only that the numbers of consuming units may differ substantially from the numbers of people, given internal age and sex differentials. More important is the inclusion in aggregate product of government consumption (only part of which is of

direct service to the needs of the population) and of capital formation. Approximate data, this time from the United Nations, Yearbook of National Accounts Statistics, Vol. III, 1975, indicate that for Africa (ex. South Africa) private consumption expenditure per capita grew from 1960-62 to 1972-74 at the rate of only 1.0 percent per year, whereas GDP per capita grew at 2.7 percent and government outlays at 4.3 percent. While this outcast may be affected by inclusion in the African total of oil exporters, we find that for Asia (East and Middle South), the growth rate of per capita consumption expenditures was, for the same period, only 1.5 percent, whereas it was 2.1 percent for per capita GDP and 2.9 percent for per capita government outlays. Interestingly enough, for the Latin American group, with its distinctly higher per capita product among the developing countries, the growth rate in per capita consumer expenditure was as high as 3.0 percent over the period, about the same as the 3.1 percent for per capita GDP and higher than the growth rate in per capita government outlays of 2.6 percent per year. The implication of these crude figures is that over the span from 1960 to 1974, the growth rate in per capita consumer expenditures among the low income developing countries was further below that for the upper income developing countries, the differentials in growth rates for this important component of product being greater than in the growth rates of aggregate product per capita. Thus, the gap in consumption per capita may have widened appreciably more than the gap in product per capita.

The purpose of these brief notes is to emphasize that international inequalities in income per capita, even if limited to developing countries and neglecting the gap vis-a-vis the developed regions, are an important component of the income distribution problem, and of world absolute and relative poverty. Thus, the appraisal of income distribution studies at the World Bank should include the appraisal of its work in this part of the field. While I am inhibited in contributing to such an appraisal by a question as to whether it is within the terms of reference, and by the sheer magnitude of the task. Two sets of comments seem appropriate.

(a) The first raises the question as to where within the Bank's research program on income distribution is the work on international income inequalities, which would take account of variant concepts of the total (aggregate product, consumption inclusive flows to consumers from government, etc.), of the recipient units (differences between persons and consuming units etc.), proper conversion of currency totals to purchasing power, taking into account the proper total and the components of it that are in kind. The topic of international income inequality is touched upon in just one chapter in Redistribution with Growth, a chapter that provides a single table based on the Bank's World Atlas but fails to advance our knowledge substantially. There is repeated reference to this inequality in Mr. McNamara's recent annual addresses; and there is, of course, reference to it in the

annual prospects and projection reports, at least in the one published in 1977 and quoted at the start of this section. And, of course, there is the major study by Kravis and others, on international gross product and purchasing power, to which the Bank has contributed generously. But has there been, in concentration on internal income distribution and its presumable changes in the course of growth, an attempt also to examine more intensively the international comparisons, experimenting with different measures of consuming units, aggregate consumption, and approximate purchasing power differentials in cross-section and in movement over time-to provide a better framework for considering the implications of differential growth rates among the developing countries (and the latter and developed countries) as compared with possible differences in internal distributions and their changes over time? Have there been systematic studies within the Bank, attempting to explore the factors that would account for low growth rates for some developing countries or groups of them, and high growth rates for others? These are questions to which I have no answers at present: yet they should be considered in thinking of the research program of the World Bank on income inequality and poverty problems.

The second comment relates to the quality and adequacy of the data for international comparisons of aggregate product, and its major variants, particularly among the developing countries. Here, the first impression is that the developing countries are less developed not only in capacity to attain high levels of output per worker or per capita, but also in the capacity to generate adequate basic data on population and aggregate product; and perhaps more important, have only limited numbers of data-oriented scholars whose critical use and evaluation of the data in their relevance to properly defined economic concepts, is an indispensable step in the continuous improvement of the data framework. The reasons for this situation need not be detailed here, but they clearly lie in what might be called statistical and analytical implications of underdevelopment for quantitative knowledge about the countries affected. And the problem is clearly aggravated by two other trends. The first is the dominance of developed countries in formulating the statistical and economic concepts and accounts, with resulting unsuitability of some of them for the conditions in LDCs and omission of aspects important in the latter. The second is increasing "politicization" of the supply and use of data, which may be involved in domestic conflicts over policies or in international conflicts over contributions and aid; with the pressure of group and country interests resulting in generation of data and estimates that are bricks without straw, and possibly biased to boot.

This is not to deny the enormous contribution made by the striking acceleration of basic population and economic data the world over during the last two to three decades -- an acceleration that becomes obvious when one compares the compilations of demographic data and national economic accounts and their components available now in the publications of United Nations and its various agencies, or in the World Bank and IMF, with what was available before World War II under the aegis of the League of Nations or other international institutions. But it is also true that with the rapid spread of statistical reporting and estimation the world over, the supply, even if we exclude Communist countries some of which treat basic data as secret weapons hidden behind a blackout curtain, is of highly uneven coverage and quality. Any adequate use of them requires critical examination; and for many analytical purposes a ruthless exclusion, if various tests do not allow for removal of major deficiencies.

Turning now to the work of the World Bank on international comparison of product and growth rates among the developing economies (and others), one may assume that a great deal of work and country expertise has gone into the estimates that have been published. But one has to judge on the basis of what was published, and I shall center my comments on the World Bank Atlas, perhaps the most widely used and quoted Bank publication.

The latest available to me, for 1976, contains estimates of population (mid-1974), GNP at market prices in US\$ (1974), and per capita GNP (1974) -- plus, most important, growth rates (% per year) for 1960-74 and 1965-74 for population and GNP per capita. These valuable data are given for 55 countries in Africa, ranging in population from Nigeria with 73 million to the Scychelle Islands with 56 thousand; 39 countries in Asia, ranging in size from 809 million for Mainland China to 116 thousands for the Maldive Islands; 35 countries for Europe, ranging from USSR with 252 million to Gibraltar with 28 thousand; 30 countries for North and Central America, ranging from the USA with 212 million to the Canal Zone with 45 thousand; 13 countries for South America, ranging from Brazil with 104 million, to French Guiana with 58 thousand; and finally, 10 countries in Oceania and Indonesia, ranging in population from Indonesia with 128 million to New Caledonia with 132 thousand . This is a total of 182 countries. If we omit the small countries, of less than 1 million population each in 1974, the total is still of 125 countries, ranging in population from Mainland China to Trinidad and Tobago, and with respect to per capita GNP in 1974 from US\$7,870 for Switzerland to US\$70 for Lao PDR. What does the publication tell us about the quality of the data and the methods of obtaining the comparable dollar GNP totals and per capitas, and particularly the growth rate of product per capita, for this enormously wide collection (I am omitting the appended table on 1973, 1974, and 1975 on population, GNP, and per capita)? In considering

this question, one should note that the <u>World Bank Atlas</u> provides estimates of per capita product and of growth rates in the latter for a period as long as from 1960 to 1974 for many more countries than those for which such per capitas and particularly growth rates are provided in the United Nations Yearbooks of National Accounts Statistics, or in the OECD Research Center reports on the accounts for developing countres; and that there may be discrepancies among all these three sources relating to the same country and period. Thus, the data in the <u>World Bank Atlas</u> presumably reflect the distinctive experience and data of the World Bank.

The technical note in the 1976 Atlas discusses largely the conversion to comparable dollars, and refers to the valuable study by Kravis et al, on international purchasing power comparisons, already noted above. But there is no discussion of methods of estimation or of quality of the data. The only specific reference to the latter is a note attached to some of the country entries reading "Estimates of GNP per capita and its growth rate are tentative" (I am quoting from the table on p. 5, which covers 125 countries, each with a million or more population and showing GNP per capita and the growth rates). Of the 125 countries in the table just referred to, 22 countries are so marked. But 15 of these are Communist countries, all of which appear to have been put in the tentative category. Of the market economies only 7 are so classified,

all of them developing countries (Sudan, Yemen AR, Lesotho, Burundi, Rwanda, Somalia, and Bhutan). All of the remaining 103 countries, most of them LDCs, are shown undifferentiated with respect to the firmness of the estimates of population, GNP, and the growth rates in them. Yet it would not be difficult to select a number for which the data base is relatively weak, and for which the growth rates are proximate indeed. One obvious drawback of such an undifferentiated and bare presentation is that the critical users, knowing of the serious weaknesses of the estimates for many countries, are likely to extend this impression also to firm estimates; while the wider public is likely to exercise indiscriminate use of estimates so indiscriminately presented.

The comments above should be amplified, and tested, by reference to other issues of the World Atlas, and other Bank publications of international data on per capita product and growth rates in the latter. But they suffice to suggest three questions.

The first is whether it would not be possible and desirable to supply to the wider circles of interested scholars and students the results of the accumulated experience and knowledge within the Bank, which has been used to derive the estimates of levels and growth rates—by revealing supporting evidence of more general interest, indicating judgments as to differences in reliability and rough magnitude of possible errors, and the like? The second is whether, in the light of serious limitations on the validity of many estimates, now

presented without any qualifications, it serves a useful purpose to aim for encyclopedic and universal inclusiveness, and in full country detail? Granted that for some internal uses in the Bank it may be better to have a rough approximation to either aggregate levels or to recent growth rates, need it be released for general use if the approximation is rough indeed and if the small size of the country does not warrant major efforts to improve it? Third, the World Atlas is, at present, a bare collection of detailed tables, with only geographical groupings, and little analytical discussion of differing trends or interesting aspects of comparative growth. Is there any regular, widely available publication of the Bank (not including Mr. McNamara's annual address) that would contain such a discussion and the underlying data? I am raising these questions in the possible view that the role of World Bank research and publication is not only to serve the needs of its operating staff, but also to help formulate the general orientation of Bank policy and to inform the general public of scholars and interested persons, thus assisting their understanding of the major problems of growth and welfare in the developing countries.

III. Internal Size-Distributions of Income

As indicated, the term in the title refers to distributions of an income aggregate among recipient units, the latter classified by the size of their inocme. These units may be households usually predominantly family households, ranging from one to several persons; or individual income recipients, whether generally defined by receipt of some minimum amount of income or limited to recipients among the economically active population. It is such data that have been largely employed in the research and publications of the World Bank on income distribution. Our concern here is foremost with the adequacy of the data base, the coverage and quality of the data relative to the concepts of recipient unit and income that should be employed. One may state at the outset that the data requirements, for a proper coverage of the distribution of income, free from transient disturbances and of the effects of different phases of the life cycle of income, among recipient units that are comparable basic family household units with allowance for their differing size, and with needed adjustments for purchasing power differentials among various distinct groups of such units within the economy, are highly demanding. The result is that adequate measure of such distributions are difficult to secure even for developed countries. One can expect a variety of non-comparable estimates, deficient in many respects, requiring critical scrutiny and diverse experimentation to reduce non-comparability, and extreme caution in deriving differentials claimed to be significant.

In view of the wide range of the difficulties with the supply and quality of size-distribution data and estimates, and of the dominant use of the latter to secure our knowledge of internal income inequalities, it may help to indicate the several problems that will be touched upon in the discussion that follows. These are problems of: (a) inadequate international coverage of the data and estimates; (b) errors in those estimates, relative to what they claim to cover; (c) disparities between the recipient units in the distributions and the ones required for analysis of income inequalities associated with economic growth; (d) disparities between the concepts of income and its variants used in the distributions and those required in proper analysis. The discussion will necessarily be incomplete for any of the problems touched upon, but it is important to cover the full range of difficulties. We conclude by (e), reflecting on the effects of limitations of the data on some of the findings.

(a) Redistribution with Growth (1974, designated Source-A) contains in Table I.l perhaps the most comprehensive cross-section of ordinal income shares from size-distributions of income for 66 countries, 5 of them Communist and almost two-thirds of the remaining developing market economies. The compilation by Shail Jain,

Size Distribution of Income (1975, designated Source-B) provides income shares, measures of aggregate inequality, and a few other measures, for 71 countries, 6 of them Communist. A more recent cross-section, in M.S. Ahluwalia, Inequality, Poverty, and

Development (World Bank Reprint Series, no. 36, 1976, designated Source-C) covers 60 countries, selected almost wholly from the Jain compilation, the choice having been "dictated by particular judgments about the reliability of data in some cases". (p. 339). This list contains 41 developing market economies, 13 developed market economies, and 6 communist countries. We shall not discuss the Communist countries, since both the data base and the instituttional arrangements for them involve major incomparabilities with the freer market economies—concealing costs (and returns) so different from purely economic as to shift any attempt at proper comparison to a different level of discourse. *

The large number of market economies for which size-distributions could be assembled and compared suggests a wealth of data. But this impression is dissipated when the lists are examined, even if we disregard for the moment the errors and conceptual deficiencies that remain even in the data of the selected 41 developing market economies in Source C (see Table 8, pp. 340-41). The first observation is that some major developing countries in important regions are missing. Thus, for Subsaharan Africa, Source-C fails to cover the more populous countries: Nigeria, Ethiopia, Zaire, Sudan, let alone South Africa (which, by its over-all per capita income would belong to the developing group). And were we to possess a proper typology of developing countries, we might find other important omissions for some type-classes within them. Second, the size-distributions refer each to a single year, with few relating to a time span * See next page.

* Another publication of the Bank that contains ordinal income shares (and Gini coefficients) for a number of countries is the Chenery-Syrquin monograph on Patterns of Development, 1950-1970 published for the WB by Oxford Press, 1975. Two sources of such data are given. The first, used for the scatter diagram relating, for 55 countries, the income shares of the lowest 40 and highest 20 percent to per capita GNP, for 1965 (Figure 12, p. 62, and discussion on pp.60-63); and for income shares of the same ordinal groups in the last two columns of Table S-4, pp. 196ff covering some 52 countries (including Yugoslavia, but no other Communist country) is an August 1973 Discussion Paper no. 4, by Shail Jain and Arthur E. Tiemann, "The Size Distribution of Income: A Compilation of Data" (see p. 187). This apparent predecessor of the Jain compilation (Source-B) contained according to another reference, data for 66 countries (see footnote 14, p. 60).

We assumed that the data coverage was similar to that now in Source-B.

The other source, used for income shares in Table 16, p. 103, covering some 34 countries and showing, in addition to ordinal shares also Gini coefficients—is given as Redistribution and Growth, presumably the shares as given in Table I.1 of this source. But a spot check reveals some puzzling discrepancies. Thus, for Ivory Coast the shares in Table 16 are 17.5 and 55.0 percent respectively, in Table I.1 for 1970—10.8 and 57.1; for Malaysia 17.7 and 43.9 in Table 16, and for 1970 in Table I.1—11.6 and 56.0. The discrepancies, for these and a few other countries, may be due to different dates, but one would have to check further.

Since the coverage in the Chenery-Syrquin monograph is not unlike that in Sources A and B, no further detailed examination of the income distribution data in this monograph seemed necessary. (e.g., Lebanon for 1955-60). The dates vary widely within the period from the mid-1950s to 1971. Thus of the 41 developing countries in Source-C, in which we are most interested, 10 countries are covered by estimates relating to years from 1955 through 1960; another by estimates relating to years from 1961 through 1965; and 22 countries by estimates within the time range from 1966 yo 1971. Since the estimate of size-distributions can be affected not only by transient elements peculiar to a particular single year, but also by changes over time--even in relation to the changing per capita product in constant prices--there is an element of non-comparability in a cross-section comparison in which the estimates are spread over a time range as long as one and a half decades.

But the most serious limitation in the supply of size—distributions is revealed not in Source-C, which fails to contain inter-temporal comparisons for one and the same country (but discusses some temporal implications of the major cross-section comparisons). It is to be seen in Source-A, in which Figure I.1 (p. 14) presents a graphic comparison of the growth rate in the income share of the lowest 40 percent with that of GNP--for 18 countries, of which 13 are developing market economies; and Table II.1 (p.42) which shows growth rates in the income of ordinal groups for 13 countries, or 12 developing (excluding Yugoslavia). This is a rather limited sample; and even were we to assume full statistical comparability, the short stretch of the interval (six years and not more than ten) combined with the possible differences in transient elements in the

terminal years may mean that temporal trends in income inequality, if any, could not be easily discerned. Such scarcity of time series relating to size-distributions of income in developing countries, is a major gap in the supply of data--significant even if we were able to adjust fully for any incomparabilities in inter-temporal comparisons of such estimates as are available.

(b) Size-distributions are usually estimated from sample studies of household income (or/and expenditures) or census income questions. Under certain but limited conditions, they can be derived from the national accounts data using industry-factor income cells or other components that lend themselves effectively to a size-of-income array. But given the usual source, the commonly observed result is that the totals of income and components so derived tend to fall appreciably short of comparable totals in the national economic accounts; and the shortfalls are both substantial and significantly different in relative magnitude among different income components associated with different levels within the income distribution.

Since the results of the intensive study by Dr. Oscar Altimir and his colelagues at ECLA, "Income Distribution Estimates from Household Surveys and Population Censuses in Latin America: An Assessment of Reliability (Bank Staff Working Paper, November 1976) are available, there is little need to labor the point further here (see especially chapters VII and VIII, Summary of Findings, and Main Consequences for Income Distribution Analysis). The wide incidence of major shortfalls in the sample and Census income data, with differences in relative shortages for various income types, is not limited to Latin America. In another World Bank study, by Sudhir Anand,

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Size Distribution of Income in Malaysia (manuscript, fall 1977), the author states (Chapter III, p. 22) that the "mean household income estimated from the Post enumeration Survey (for 1970) is M\$264 per month ... and the degree of under-statement in PES income relative to the National Accounts is on the order of 25%." Then the author adds: "Although this might seem quite large, it is in fact not particularly great by the standards of household surveys conducted in LDCs." Indeed, he adds, it is "only fractionally" greater than the understatement in surveys in developed countries, and refers to a paper by Mr. Sawyer, "Income Distribution in OECD Countries", OECD Economic Outlook July 1976 (which I did not consult). My own work on the data for Taiwan also revealed large shortfalls of the family income and expenditure survey totals relative to the comparable totals in the national accounts.

Even close agreement between the two sets of totals, of the survey-census base underlying the size-distribution estimates and of the national accounts, is no ground for assuming that there was no understatement (or overstatement) within the size-distribution: it may mean that the shortfalls and excesses at different ranges within the total roughly offset each other. This applies to any pair of totals, whether they be for comprehensive income or consumption aggregates, or for subcomponents such as wages and salaries, entrepreneurial income, and the like. Thus within wages and salaries, a close agreement in totals may still mean that partial omission for low paid casual labor was compensated by partial over-representation of workers at above average level of wages and salaries—so that the

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actual distribution is distinctly more unequal than that measured and estimated. And even a substantial shortage in the survey total relative to the national accounts aggregate does not fully eliminate the possibility that there was, in the survey, over-representation of some groups, only partly offsetting undercoverage of others.

Still, granted that close agreement between the sizedistribution and national account totals is not a full guaranty that the measured disparities in the former reflect properly the true income inequalities, a substantial shortfall (or excess) of the former totals relative to the national counts is justifiably a matter of concern. The implication is of major errors in either one, or both, sets of totals. If, for obvious reasons; one tends to assume the error in the sample or census data on income, either because of inadequate weights by which the sample has been converted into nationwide totals or because of misreporting by the sample or Census covered units, the crucial question that arises is whether it is possible to adjust for the effects of such errors on the major findings that the uncorrected distribution reveals. Shortages of as much as 20 to 25 percent of nationwide income totals, if they be so common among the results for developing countries, imply adjustments that can substantially modify the ordinal shares and affect international and intertemporal comparisons. Presumably it would be desirable to attempt in each case a test comparison between the findings of the sample or census data on income distribution with the comparable totals in the national accounts, and

then experiment with various ways of making the reconciliation, by adjustments in one or both sets of totals--rejecting the inferior data if no adjustments can be devised and the over-all discrepancy is too large to permit valid inferences as to significant magnitudes. But this is a difficult undertaking. The publications of the World Bank in the field recognize the problem, when unadjusted estimates are used in cross-section or intertemporal comparisons, and present some arguments that still justify these uses of non-comparable estimates. But it would be best to consider these arguments after we have reviewed the other problems, relating to the definitions of recipient units and of income whose distribution is being studied.

(c) The present subsection deals with scope of coverage and the recipient unit of the size-distribution used in the World Bank cross-section comparisons, in Sources A-C already referred to.

We begin with findings of a comparison of scope and recipient unit in the size-distributions used in Table I.1 of Source-A with the information on these distributions provided in Source-B. We again exclude the Communist countries; and find that of the 61 market economies in Table I.1, one (Madagascar) is not reported in the Jain compilation in Source B. Of the 60 market economies, the size distributions for which are identifiable in both sources, the check reveals that the coverage is short of national for 8 countries, with some question about the 9th. For Argentina, Burma, Dominican Republic, Greece, and Iran, the coverage is either of the capital city alone, or of urban only; for Guyana, Sierra Leone, and Uganda there are major geographical or group exclusions. For Thailand (1970), source

B shows distributions for rural and urban households separately,
but not together; and the ordinal shares shown in Table I.1 are
close to those for the urban distribution in the Jain compilation.
For these 9 countries there is an unknown element of non-comparability,
associated with limitation of coverage, relative to the distributions for other countries with full national coverage.

For the remaining 51 countries, the size-distributions are for the following types of recipient units, using the terminology of Source-B: households--25 countries; income recipients--12 countries; economically active population--5 countries; total population, a rather vague category--6 countries; and per capita--1 country (this being the United States, the reference indicating a combination of the distribution among unattached individuals with that among families reduced to a person basis). The dominant groups are then of distributions among households, or among recipients--whether all, or among the economically active population.

A check on the data base of a more recent summary of cross-section evidence on size distributions of income, in M.S. Ahluwalia's paper on "Inequality, Poverty and Development," in Journal of Development Economics 3 (1976), pp.307-342 (Source C) indicates that of the 41 developing countries only 2, Uganda and Guyana, relate to an area or group short of the national total; that of the remaining 39 developing countries, the distributions in 16 countries are among households; the distributions among income recipients, or economically active population, or total population cover 7 countries each; and for 2 countries the distributions are for per capita income. With the distributions for the 13 developed countries being among

households for 8 and among income recipients for 5, the distribution for all 52 market economies, with 24 distributions among households, and most of the other among different variants of personal income recipient, is not unlike that for the 51 countries in Table II (Source-A) described in the preceding paragraph.

Before commenting on the problems of cross-section comparisons of size distributions using a mixture of types of recipient units of the kind just observed, one should note a problem with the lack of adequate information of these various types of recipient unit in the Jain compilation. Thus, the descriptions of the type designated "Population" (p. xii) provides no clear view of it; and the difference between unit types designated PC and PCH (p. 6) if any, is not clear. No information is provided on how the "income recipients" are defined, particularly with respect to family members working and not receiving any cash income; or with respect to female, young, or old persons who may be receiving minor income amounts. This failure to be explicit about various important aspects of definitions in the size-distributions reported in the Jain compilation will be noted below for other important components.

With respect to the possible results of a mixture, in one comparison, of size-distributions of income employing different, and conventional, recipient units, two comments can be made. First,

the difference in recipient units may result in different ordinal shares for the same country and year--with a marked tendency for distributions among income recipients to show wider inequality than for those among households. Thus, if we distinguish within Table I.1 of Source A the 25 countries for which the distributions use household units (Group I) from the 26 countries for which the recipients units are individuals (Group II), we find that the 9 countries with low income (below \$300 GNP per capita in 1971 prices) in Group I show an average share of the lowest 40 percent of 14.2 percent in the total income; whereas the 12 countries of Group II in the low income category average, for the lowest 40 percent, 11.2 percent of total income. A similar comparison of the 10 countries in Group I with the 5 countries in Group II that are in the middle income bracket (\$300 to \$750 per capita GNP) yield average shares of the 40% lowest of 13.4 and 8.0 percent of total income respectively. For the 6 countries in Group I and the 9 countries in Group II that are in the highest per capita income class (\$750 and over) the average shares of the lowest ordinal group are 17.1 and 14.2 percent respectively. Disparities in ordinal shares of the lowest group of units within similar ranges of per capita income of this magnitude -a fifth to four-tenths of the larger of the other shares -- are too wide and too consistently in one direction to be neglected.

Second, and perhaps more important, neither of the widely used types of unit, whether household or individual income recipient worker, stands for equivalent groups of dependent consumers. holds differ in number of members, and the distribution that classifies households by income per household would naturally show a significant positive correlation between size of household and its income -- so that lower household income is associated with a smaller household, a smaller number of persons dependent upon that income. But the same is likely to be true of the size-distribution among individual income recipients. The earners or recipients of lower incomes, dominated by part-time worker, secondary labor supply, and the like, tend to be associated with fewer dependents on that income than the high individual income recipients, more representative of heads of families and of ages in the life cycle where both income and number of dependents are likely to be large. follows that ordinal shares, say the lowest 40 percent of households or of income recipients classified by income per household or per recipient, represent shares of population (whether persons or consuming units) that are distinctly below 40 percent; while the shares of the top 20 percent of households and income recipients represent those of more than 20 percent of population or of consuming units. Consequently, if we retain the grouping of households by income per household, and then allow for the differences in average number of persons per household in the

successive income classes, the income differentials are reduced in magnitude. Thus, for Taiwan in 1972, the range in income per household, unadjusted, was from 0.3 to 3.6, with the TDP (sum of differences in percentage shares in income and in households) at 42.2; with the adjustment the range narrows to one from 0.55 to 2.58, and the TDM measure shrinks to 30.6 (see my October 1976 paper in Economic Development and Cultural Change Table 12, pp. 41-2). Yet for another country, the result can be quite different. Thus, for the Philippines in 1970-71, the range in the unadjusted income per household was from 0.17 and 6.62, and the TDM was 69.2; whereas with the adjustment the range narrows to one from 0.20 to 5.48 and the TDM declines to 62.0--a much smaller effect than in Taiwan.

But such adjustments of class averages of households grouped by income per household are <u>not</u> a proper approximation to the distribution of persons by income per person. To approximate this distribution in cases where the household is the recipient unit, one would have to convert the income entry for <u>each</u> household (or narrowly defined groups of them by size) to income per person (or per consuming unit)—an operation that is next to impossible if the recipient unit is an individual for whom the number of dependents is not given. It is only then that we can group the per capita or per consuming unit income entries by their level and derive ordinal income shares for groups in the population (viewed as the total in the households, either as persons or as

consuming units). For the few countries for which an attempt was made to shift in this fashion from the distribution among households by income per household to a distribution among the population in the households by per capita income of households, it was found that the smaller households tended to show larger per person income than the larger households (both grouped by size); and, in consequence, the very identity of the low and top groups of households changed from the conventional size-distribution among households to one among persons obtained in the conversion. Even then the results of the latter have to be adjusted for transient income elements and differences in phase within the lifecycle of per person income for the family or household unit—a problem with the income concept to be noted in subsection (d) below.

It follows from the comment just made that it is impossible to pass from the conventional size distribution among households by income per household, to groupings of population (of persons or of consuming units) by income per person or per consuming unit—without elaborate conversion of the type suggested. And such conversion would be even more difficult in any shift from the distribution by individual income recipients by income per recipient to one of income among the population by income per person or per consuming unit. The results of a full conversion, when feasible, for say a conventional size distribution among households may yield findings similar in direction and magnitude to those from comparisons of

conventional size distributions; but one cannot say without attempting such conversions for as many countries as the available statistics permit.

This comment affects not only Table I.1 in Source A and the size distributions in Source C, but also the uses of the original shares (and inequality measures) in other applications. relates to attempts to use the conventional size-distributions to identify people below poverty lines, or deficient in associated consumption levels, or, constituting the rich at the other extreme. Even disregarding the advisability of employing equivalent consuming units rather than persons, and of stressing, at least as an alternative, the distribution and levels of consumption rather than of income, the need of adequate adjustment to shift from households and income recipients, is indispensable. Hence, it is puzzling to find in Table 1.2 of Source A (p. 12) estimates of the proportion of population (presumably of persons) below "poverty" levels of per capita income. Despite assertions in the text, it is difficult to see how it was possible to approximate such proportions in the population, "by combining income share data.... (presumably in Table I.1, SK) with total income estimates obtained from national accounts." (p.10). Or, to put it differently, such estimates were possible only on an invalid assumption that the number of dependents per low income households (or low income recipient) was the same as for the average and hence high income household or income recipient.

The same criticism applies to <u>any</u> identification of the shares in households or among income recipients with shares in population classified by per capita or per consuming unit income; and thus relate, if I follow the procedure, also to the Reutlinger-Selowsky monograph on <u>Malnutrition and Poverty</u> (World Bank Staff Occasional Paper, no. 23, 1976, particularly Appendix B, pp. 56-70). All work on poverty and basic needs must face, in addition to other problems, the conversion of conventional size distributions to shares of properly defined units (presumably equivalent consumer units) in the appropriate income totals.

Another important application of the comments above is to the identification of ordinal shares of households (or income recipients) with those of persons (or consuming units) classified by the size of their income in intertemporal comparisons of the type presented in Figure I.1, p. 14, and Table II.1, of Source A. If the disparities in numbers of persons or consuming units per household or per income recipient, in different conventional size classes change over time—as it well may in the course of economic growth and associated changes in family structure—what appears to have been a shift in the conventional size distributions may prove illusory (or confirmed but in substantially different magnitude) when the proper conversion is made.

One should note at this juncture that in the case of inter-temporal comparisons there are additional major difficulties. These may be difficulties of attaining adequate statistical comparability among two or more samples over the span of time; of

adjusting for the differences in transient characteristics of the two or more years being compared, assuming that the data on recipient units and income relate to single years rather than to averages over several; and, analytically most important, the difficulty of establishing the extent of mobility over time of recipient or dependent units in and out of the lower or upper ordinal groups. This is partly associated with the effects of transient, short-term elements in the income distribution, one encountered in particular with income levels but applicable also to the structure of the household; but it raises the bigger question as to how many of the poor and rich of today were among the poor and rich of, say, a decade ago. Clearly, wide mobility among properly defined ordinal classes by properly defined income per properly defined recipient unit would lend an entirely different meaning to comparisons across time of the shares of the poor and the rich than would be ascribable under conditions of complete or relatively complete lack of such mobility.

Before concluding the brief discussion of the difficulties in passing from the conventional size-distributions among households or income recipient to those among population by income per person or per consumer unit, one other comment must be made--although it raises a problem even farther reaching than the ones noted. It should be clear from the discussion that family household is the most acceptable among the variety of recipient units used in the conventional size-distributions, because it is the family that

represents the basic group of persons sufficiently related by blood and marriage ties (or adoption) to warrant expectation of joint decisions on at least some significant economic choices -- relating either to supply of labor, use of other assets, and allocation of income and consumption. But almost all the available statistics define the household, including family households, by location in the same place -- with only rare exceptions for including dependent members living elsewhere (like students away from home). to common residence, which is required to obviate major statistical difficulties in identifying family composition when members live apart, raises a major question. This question is, to quote a recently completed paper (on "Sizes and Age Structure of Family Households: Exploratory Comparisons," to be reproduced as Center Discussion Paper by the Yale Growth Center) "as to the significance of joint residence in terms of family decisions on economic choices; and the question is brought into sharp focus by the finding that in the developed countries in recent years, over half of all the households were one or two person units, heavily dominated by men and women in advanced ages and secondarily among the young--whereas similar proportions among the LDCs were well below 10 percent for the two small household groups." One may add that for the LDCs the question is also relevant in view of the possible interrelations among distinct (by residence) households large as each may be, because of greater preservation of blood ties, or of tribal affinities.

The question thus applies to blood-or marriage related separate households, regardless of their size. To quote again: "If in the course of economic growth the parental pair stays in agriculture, and suffers a decline in relative (if not absolute) income, while its offspring, having migrated to the city, secures in the longer run a higher relative economic position for its household, do we view this as emerging inequality among households, or do we combine the two households in a cluster on the ground of sufficient community of economic interest?"

The question obviously does not admit of an unequivocal answer, in absence of detailed information on the decision processes in so related separate households. Yet the broader concept of a family as a blood-and marriage-related group that makes joint economic decisions, continuously or intermittently (but the latter on major economic choices), is important as a general background against which to evaluate advantages and limitations of recipients units employed in the conventional size-distributions of income. Some recent trends, such as the rapid morcellization of family households in the developed countries, and such institutional aspects of family structure as prevail in some of the diverse groups of developing countries, strongly suggest the need to be aware of the consequent limitations of the conventional recipient units--over and above the lesser problems with which we are more familiar and experimentation with which is now feasible for a number of countries.

in the definition of the income total that is distributed among recipient units in the conventional and available size-distribution. In attempting to evaluate the supply and quality of the estimates in this particular respect as used by the World Bank in the cross-section or other distribution analyses, we are stymied by the absence of information on the income concept in the major compilation (Source B), let alone in Sources A and C (as well as the Chenery-Syrquin monograph). It would be possible to go back to the original sources, and identify in each case the precise scope of the income total, or the variants of it, used; but such a laborious task is not feasible.

Under the circumstances only two observations relating to the income-total aspects of the size-distributions used in the several World Bank sources already considered can be made. The first is that there must have been some differences in the scope of income totals used among the size distributions for different countries or for different years. We know that for some countries (e.g. the United States) sample studies of family and household incomes are limited to cash income and exlcude income in kind; that for other countries households are grouped by total income receipts including gifts and transfers from other households (e.g. Taiwan): and so on. Hence, the multi-country cross-sections in Sources A and C must include elements of non-comparability in the definitions of the income totals, in addition to those involved in the use of different types of recipient units; but how large such elements of non-comparability are, we cannot tell at present.

The second observation suggests that the size distributions for few countries, whether developed or developing, would be based on the use of income totals that would satisfy the analytically desirable These criteria relate first to the completeness of coverage of the income, in its inclusion of both cash and income in kind; of factor receipts as well as flows from government and other institutional sources, as well as the compulsory drafts that may be imposed by them; and also of receipts and transfers among households in so far as they reflect the ties of common interest among separate households of the type noted above. The criteria relate next to what might be called the time level of the income reported and uased, as distinct from the time level desired for many analytical purposes. refers to the need to eliminate or damp transient, short-term components in annual income, and to adjust for the effect of shorter phases of the lifecycle income path of the recipient unit. In the third place, the possibility of substantial differences in purchasing power between the rural and urban recipient units, and within these large groups, between the lower and higher income groups, has to be considered. And, finally, one should note again for inter-temporal comparisons, the possibility of mobility of recipient units among the distinctive size-classes, even when distinguished by comprehensive estimates of long-term secular income levels adjusted for inter-group differences in purchasing power. Admittedly, these criteria are a counsel of perfection;

but there is value in formulating the analytically desired income totals, if only to induce experimentation designed to provide a better notion of the magnitudes involved and a better understanding of the kind of basic data needed if questions implicit in the disparities between the conventional data and those desired are ever to be answered.

Numerous illustrations of this second observation could be provided, both from the World Bank documents and from scholarly publications elsewhere. But one may hope that the points made are sufficiently clear; and we can turn to considering the implications of the difficulties with the supply and quality of data for some of the aspects of the work on size-distribution at the World Bank.

(e) The discussion above relates solely to the weakness of the empirical foundation provided by the conventional data on size distributions of income among households or among income recipients. The comments should not be misinterpreted as denying the value of emphasis on the distributive aspects of economic growth, particularly in developing countries; of the ingenuity with which the work in the field by the World Bank attempted to distill findings from disparate data, with some caution that increased progressively as the limitations of the data became more apparent; of analysis of distributive implications of different structural aspects of growth illustrated by relatively simple models employing notional but still plausible parameters; and of trying to introduce into project

appraisal and other service operations of the World Bank sensitivity to possible impacts on internal income inequality. But one is left with the question whether much more experimentation and selective treatment should have preceded (rather than followed) the kind of stocktaking and generalization that were exemplified in the empirical summary of the size distributions of income of the type provided in Sources A and C, or in publications relating to poverty, or in the compilations exemplified by Source B.

The puzzle is that many of the limitations of the data used, relating particularly to the nature of the recipient unit and definition of income, were recognized by the authors. Yet the natural inference from such limitations, in the direction of experimentation with different recipient units, different income totals, scrutiny of the disparities between the sample totals and the comprable totals in the national accounts, critical rejection of some country data as resting on too weak a basis, and retesting the findings in terms of the results of such experimentation, was apparently not followed. The reasons given do not appear convincing. In commenting on the weakness of the data, the discussion in Chapter I.1, Source A, "We assume that until better data become available, cautious use of exisitng data -- with all its limitations -- provides some perspective on the nature of the problem." (pp. 5-6). The operative word is "cautious"; and one may legitimately argue that such cautious use demanded far more experimentation, adjustment, and selection than was supplied -- all of them relating to the basic

definitions of recipient unit, income total, time span of coverage, etc .-- and feasible by scrutiny and use of data for various countries already available at the time. In the later, 1976 paper, by Mr. Anluwalia referred to above, the summary of the major limitations of the data in Appendix B is followed by that statement that "our estimates of income distribution are subject to substantial measurement error. In defense of the use of such data for cross section analysis we have only the familiar excuse: the presence of random error in the data serves only to hide cross-country patterns rather than to generate spurious patterns." (pp. 341-342). But the assumption that the error was "random" is hardly compatible with what we know about effects of inadequate adjustments for size of household and phase in the lifecycle as judged by age of household head, of the effects of transient income disturbances on the inequality spread of size-distributions based on annual data rather than on approximations reflecting longer-term levels, and the like. The effects on the observed income inequalities relative to what one can surmise would be the case for data more in conformity with the conceptually required distributions are large. It is precisely because the major errors are substantial and not random that one would have wished for a more explicit treatment of the major difficulties, using the data that were available, even if they be limited to a few countries.

The same comment applies to the Jain compilation (Source B) the Foreword to which indicates that the data are presented as "essential raw material for quantitative research on an extremely important topic." (p. vii). The question is whether the material presented is adequate for quantitative research, even as raw material; or whether additional information on income definition, size of the sample, more information on the type of recipient unit and the procedures involved, and even reference to original sources rather than to secondary sources (for several countries) should have been added, to render the compilation more usable by a reasonably competent analyst. One may wonder whether a closer scrutiny and rejection of a number of shaky estimates (which could be listed, but with the data omitted), and more of useful information on the data included, would have met the needs of furthering quantitative research far more than the present compilation; and whether the failure to provide such selection and information may not result in uses of the readily available ordinal shares or inequality measures that would be more misleading than enlightening.

The discussion of this problem of "non-comparability of an unknown nature in the estimates" in the September 1976 evaluation of the size distribution work program (Dubey Panel) (para 5, p. 2), indicates indecision as to how such non-comparability could be reduced. The judgment of some members

of the panel that further scrutiny of the underlying data would be advisable was countered with the statement that "the number of countries for which data were compiled would have been drastically reduced if it was necessary to provide information of this kind." But this argument does not apply to the kind of experimentation that could be done for a number of countries with already published data, with some inferences for the scope and character of the comparisons that would then follow. Nor is it clear that a large collection of non-comparable data is to be preferred to a smaller collection of data with non-comparability elements greatly reduced.

The argument for greater selectivity and experimentation with the conventional size-distribution of income data is not made on the ground that the results are likely to modify substantially the few findings already derived on differences in internal income inequality among broad groups of countries, or on the time pattern of such income inequality associated with phases of economic growth. Given a variety of biases in different directions, one cannot tell. The understatement of income in the sample or Census data on income would suggest that the observed distributions understate income disparities; the use of short-term income with its transient and phase of lifecycle components would suggest a substantial overstatement of inequality in longer-term income levels; the failure to adjust for differences in purchasing power may result in the

measured distributions overstating the real income disparities; and the conversion from distributions among households or income recipients by income per HH or IR to distributions among persons or consuming units may or may not change income inequality, while changing the identity of units at the lower and at the higher income levels. The outcome is far from certain; nor, in absence of firm comparative costs, can one urge that a major priority be assigned to this difficult task.

Yet one could argue that if findings from weak and noncomparable data are being claimed, it would be intellectually
comforting to observe the effects of greater selectivity and
experimentation, even if observations be limited to a few countries.

Perhaps more important, such a task, if pursued, would involve learning about significant aspects of the data in relevance to a variety
of analytical concepts—learning indispensable, if there is to be
much improvement in the future supply of more reliable and relevant
data. This last comment bears not only on the data relating to size
distributions of income, but also to important components in the
national economic accounts, the weakness in which may be revealed
in the explorations; and the improvement of which would be required
to assure the usefulness of the accounts for a variety of other
major analytical and policy-oriented applications.

IV. Group-Distributions

These are distributions of aggregate income in the country (or another collective) among groups of households or income recipients, distinguished within total population-by criteria other than the size of income per household or similar unit. At a broader level, the income differences among countries discussed in Section II above (differences in per capita income among developing countries) can be viewed as part of a group distribution in which the groups are populations of the different countries distinguished. But we are concerned in this section with internal group-distributions alone, internal to each country. The criteria may be the socio-economic characteristics, of the head of the household or of the individual income recipient; or they may be based on distinction among regions, otherwise known for significant differences in per capita income and in economic structure; or among ethnic or racial groups, of interest because of socio-economic differentials among them and of the consequent concern about changes in their income shares. One should note that while the formal criterion of income per ultimate receiving unit is excluded, thus obviating many of the problems of interpreting size-distribution data, the group classifications noted above, and ordinarily used, all have significant income-differential implications.

The distributions of income among groups of the type noted possess several advantages as compared to the conventional size-distributions. First, the groups, because of their already known characteristics, usually based on a substantial empirical foundation, are far more revealing than the relatively anonymous size-of-income classes, which in a conventional size-distribution reflect the joint effects of a confusing diversity of demographic, economic, social, and personal factors. This is particularly true of groups for which economic and social characteristics identify the effect on them of, and their participation in, the process of economic growth; so that knowledge of the latter would lead to analytically based expectations of changes in the income and size shares of these groups. Second, since we deal here with averages for large collections of individual households or income recipients, the purely stochastic effects of using annual data on the income levels (and sizes) of the households would be eliminated; and differences in the lifecycle phases of the members of the group would be greatly reduced, if not completely eliminated. There will be similarly damping effects on differentials between persons and consuming units, and on problems relating to mobility of units among income classes, mentioned above in connection with the conventional size-distributions. Third, the group classifications,

particularly those based on socio-economic characteristics, (but also regional) would permit easier ties, and hence reconciliation with, the national economic accounts than the more anonymous size-distributions; and indeed, the productionsectoring of the national accounts and the sectoral attachment groups among households provide the obvious locus of the linkage. Finally, since the socio-economic, or ethnic, or regional groups stand not only for different levels of per unit income and different economic structures, they also stand for different conditions of life; and make it easier to identify the differences in markets and purchasing power differentials needed to shift from nominal to real income disparities. Indeed, the value of socio-economic, or regional, or in some cases of ethnic grouping, is so great for better orientation within the total size-distribution that very few of the sample of Census data on the latter fail to distinguish some groups within the population aggregate; and attempt to provide separate sizedistributions for at least the major groups.

To be sure, group distributions presenting averages of income per some relevant unit for a number of socio-economic or related groups within the population do not escape several of the many limitations noted above for the conventional size-distributions. If the sample-or Census-based estimates of the latter yield totals that fall appreciably short of comparable totals in the national accounts, the same shortage will affect the group means derived

from the same data. Yet, because of greater specification and closer ties to the sub-aggregates in the national accounts, the attempt to reconcile the group-means with the national accounts would be far easier than the attempt to reconcile the size-class means in the conventional-size distribution with the countryside totals. Likewise, the group means would still have to be related to the average size of the household (either in terms of persons or of consuming units, but here the identification of the groups in the sample, and, in particular, in the Census data (quite often in the regular census of population) would make the derivation of group means on a per person or per consuming unit basis far easier. The same comment can be made on the distinct possibility that the group means may be affected by such transient elements in the year's income as touched upon large groups (rather than stochastic effects on individual units). For if, let us say, there was in the given year a poor crop, reducing average incomes of the large group of farm households below normal, knowledge of it and allowance for it can be far more easily secured than for stochastic disturbances. Finally, even the problem of shifting from nominal to real income differentials can be more easily handled for income averages for large and economically distinct groups that for size-classes of households, unidentified except by the size of their annual nominal income.

Of course, the group distributions, as defined here, involve loss of information in intra-group income variance and differentials; and thus miss the coverage of the poor (or the rich) within the groups. But the balancing of such losses against the gains of avoiding the inescapable limitations of conventional size distributions should be made, while recognizing that the group means provide easily complementary information of much interest and value; also, one must not underestimate the extent to which the group means, with adequate definition of the groups, can capture the major causes of income inequality within the country. To illustrate: agricultural-nonagricultural household averages of per person income can differ, in nominal terms, in a ratio of 2.5 to 1 in a less developed country like Taiwan (or higher in a number of Latin American developing countries). The typical spread between per unit income of the lower 40 percent share and the upper 20 percent share, in size-distributions of LDCs summarized in Sources A and C, is from 0.35 (i.e. a total of 14 percent) for the lower ordinal group to about 2.5 (i.e. a total of 50 percent) for the upper ordinal group, a ratio of about 7 to 1. But this range is greatly exaggerated by the effect of short-term and phases-of-lifecycle components in the annual incomes used -- and, all other conditions being equal, the spread in long-term levels of income will be substantially narrower, perhaps not much above 4 to 1. Furthermore, having begun with a limited group classification, provided by the

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usually available statistics, one may find it possible, for the larger groups the internal distribution of which is particularly important, to distinguish subgroups, using other statistical sources. Thus, it may prove possible to distinguish within the large group of agricultural households subgroups by size of the farm's productive acreage—not infrequently available in special size—distribution sample studies, or in Census data in conjunction with other income revealing information.

The comments above on the advantages of group-distributions reflect a limited experience; and wider experimentation with this approach is likely to reveal more difficulties than were noted here. But in view of the major deficiencies in the supply of data and quality of estimates on the size-distribution of income, particularly in the developing countries, and the long and enormous task involved in overcoming these limitations, an attempt to make greater use of the group-distributions would seem to be warranted. This is the case all the more, because of the apparently greater abundance of relevant data. These are found not only in almost all results of sample or Census studies of size-distributions of income. addition, the periodic censuses of population provide valuable statistics on demographic, occupational, and other economic characteristics of various groups in the population, and also usually of various groups within the economically active or labor force component of total population. There is also periodic reporting, in addition to the census, on occupational, industrial, employment structure of the labor force, which can often be clearly

associated with the industrial or occupational structure (usually the former) of the factor incomes in the national economic accounts (as has already been done, among the World Bank publications. in the Chenery-Syrquin monograph on Patterns of Development; and as is presumably being done in attempts to arrive at "social matrix" accounts in some of the Bank studies under way, with which I am not familiar). But such associations can be carried through for individual countries; and for many of them over substantial time spans, to reveal intertemporal changes. To be sure, inter-sectoral differences in factor incomes per member of labor force are several links away from differences among incomes of households grouped by sectoral attachment of head, and reduced to a per person or per consuming unit basis. But such inter-sectoral disparities are an important contributing factor to income disparities among economically-distinctive groups of households; and one can use sample derived information on structure of households to try to build the links. Meanwhile, the relative abundance of data on economic structure of the labor force and on economic structure origin of factor incomes, in terms of country and time coverage, warrants more emphasis on their possible contribution to study of internal income differentials than appears to have been given to it in the World Bank's work in the income distribution field.

These comments on the relative abundance of data, for some countries extending over a substantial span of time, could be repeated for data relating to regional or sub-national political units; or, in some countries, to ethnic and racial groupings. Such data are particularly likely to be available in those developing countries, and there is quite a number of them, in which regional-state differences or ethnic-racial disparities are substantial; and are clearly perceived by the related groups in the country's population (a perception that explains why data on these differences were collected to begin with). Hence, any analysis relating to internal income-disparities that goes beyond recognition of the material differences and their implications, to the perception of the disparities and their possible effects on policy consensus, would naturally place heavy emphasis on such group differentials, more emphasis than on the much less revealing income disparities in the conventional size-distributions. But regardless of these additional aspects of such group-data, their apparently abundant supply provides another avenue of approach to internal income distribution, in cross-section and over time. One suspects that the work on the country reports in the World Bank utilizes these data and touches upon the problems of the type suggested above; and so do income distribution studies of individual countries (e.g. the Anand study of Malaysia). But the possible availability of such data for

multicountry cross-sections, and particularly for observing the time trends for an adequately large number of countries, is apparently still to be explored and exploited.

V. Summary and Implications

Before considering the implications of the discussion in Sections II-IV for possible priorities in the World Bank research program on income distribution, we attempt to review briefly the salient points. These refer to the empirical foundation of the work on income distribution; and their critical tone is a reflection of the many weaknesses in the data and estimates, particularly on internal size-distributions. These weaknesses are deep-seated in the economic and social structure of developing countries; overcoming them is necessarily a long process, partly contingent on social development associated with economic growth; and they have not been fully overcome even in the developed countries. Whether the use of such weak and deficient data is justified by the urgency of the problems upon which they may still shed some light, and the extent to which such use should be preceded by adequate experimentation to remove or reduce the most significant shortcomings and induce caution in drawing inferences, are matters of judgment. The judgments advanced in the discussion above have been illustrated rather than proven; but they may be useful nevertheless as those of an outside observer viewing some of the work of the World Bank in a larger setting.

- In any concern with world poverty and income inadequacy, the international differences in per capita income among the developing countries themselves, and the striking differences in growth rates over the last decade and a half to two decades among them, loom large. The magnitude of such international differences in levels of per capita income and consumption is as wide as, and may even be wider than, that observed among ordinal groups in size distributions of income properly defined and measured for long-term income levels. Hence, it seems unwarranted to argue that "absolute poverty"...continues to degrade the lives of some 800 million human beings in the developing world, (my underscoring) of the relatively rapid growth of their national economies" (Mr. McNamara's Foreword to the Chenery-Syrquin monograph of 1975). One could reasonably claim that absolute poverty was, in large part but not wholly, associated with the low growth rate of populous developing countries in Asia, low relative to that in many other developing countries.
- distribution has put little stress on international income inequalities and on the contribution of differences in the growth rates in per capita product. The reference publications on the latter topics, e.g. World Bank Atlas, contain a rich collection of data on a large number of countries, the results presumably reflecting the Bank experience in its systematic work on various developing countries and regions. But unless major sources of reporting on such experience, in the way of indicating the differences in

quality of the estimates, experimentation with various totals of population (persons, or consuming units) and of income, have been overlooked, little of the rich experience has been communicated in a systematic way. Thus, the changing background of international differences, even among the developing countries, has not been fully surveyed to provide the framework within which <u>internal</u> income <u>inequalities</u> (the other factor in "absolute poverty") could be considered.

The data and estimates relating to internal (intra-(3) country) size-distributions of income, the major body of data used in the World Bank research and publications on income distribution, are beset by major weaknesses, particularly for developing countries. The coverage of some regions, particularly in Africa, is poor, even for single year cross-sections; but far more important is the extreme scarcity of comparable time series that would permit observation of trends in the size-distribution of income -- in association with different rates of aggregate growth--for a representative sample of developing countries. The quality of the data, in terms of accuracy, for the available sample or census derived estimates, is poor, as revealed by large proportional shortages in income totals, when related to comparable totals in the national accounts. And, to complicate the problem, the relative shortfalls differ substantially for different types of income, and thus presumably for different levels in the observed size-distributions. The sizedistributions are usually provided either among households grouped by income per household, or among individual income recipients, or

among the economically active population. The difference in recipient units affects the comparability of the size distributions; but, more important, the recipient units used represent bundles of dependent consumers of differing size and have to be properly adjusted for such differences in size before they can reflect properly income disparities among persons or among equivalent consumer units. The income is reported in accordance with different concepts in different sample of Census data; but more important than the resulting heterogeneity is the fact that income is usually reported for a single year (or even a shorter time unit), and it has to be adjusted for effect of transient components and difference in phases of the lifecycle of income, and for the likely differences in purchasing power among groups at different levels of nominal income, before one can observe the distribution among persons or consuming units by the long-term levels of their real income. In addition, there are unresolved problems of association of interest among separate households who, despite different residence, are connected by blood-and marriage ties that might make for joint economic decisions; and of internal mobility of household units over time from one income group to another, so that the poor and rich of today may not have been the poor and rich of a decade ago.

- This long list of deficiencies in, and hence problems with, the supply and quality of the data and estimates relating to the available internal size-distributions of income, was discussed, but only briefly, in Section III. These deficiencies affect all work in the field; and the research program of the World Bank only just began the effort of measuring some of the shortfalls (in the study of them in the Latin American data, and in a similar project dealing with a narrower range of country data for South East and Middle South Asia). In its cross-section comparisons and in the limited inter-temporal comparisons, the published work of the Bank suffers from a mixture of weak and non-comparable data, little tested by experimentation, even if limited to a few countries and even if adequate only to emphasize greater caution against reliance on the inferences that one can draw from such an inadequately reflected universe. The broader findings may, or may not be, greatly affected by the needed experimentation, and likely revisions; but one cannot tell until the results of such additional explorations are at hand.
- (5) Internal distributions of income can be studied not only by allocation of income among classes defined by income per household or other recipient unit, but by allocation of income among groups--distinguished by socio-economic characteristics of

households or by the region of residence, or by ethnic-racial characteristics. Such group-distributions may be limited to comparisons of group-averages, thus omitting intra-group variance of income and obviating many of the problems with the conventional size distributions. Despite the omission of a substantial component of total income variance, such group distributions can be far more revealing than the conventional size-distributions, particularly if the groups are distinguished by socio-economic characteristics that bring them in close tie with the industrialoccupational structure of factor incomes in the national accounts. And with greater abundance of data on the average levels of demographic, social, and economic characteristics of such, and related groups, a significant insight into the cross-sectional, and particularly temporal aspects, of internal income distribution in association with economic growth can be secured. This particular approach has not been pursued in the income-distribution research at the World Bank as actively as it might have been, considering the major deficiencies of the available size-distribution data.

In asking now what implications for further work in the income distribution field--including in the latter <u>international</u> differences in per capita product (or variants of it) among the devleoping countries themselves--are suggested by the discussions in Sections II-IV and the brief summary just presented, one

obvious answer would stress the need for more explicit treatment of the international disparities, with variant definitions of population and product and more groupings and analysis relevant to the possible sources of the striking differences in post World War II growth rates; the need for greater experimentation and testing, already initiated but must in its beginning, explorations intended to deal with the multiple deficiencies in the conventional size-distribution data and estimates available; and more work on group-distributions of income, not necessarily aimed at the ambitious task of disaggregating national economic accounts by distinct socio-economic groups, but handling the wealth of varied data more freely, with special emphasis on contribution to insight into changes in distributions over time.

But these suggestions, in themselves, constitute a major and costly research program, costly not only financially but in time and the absorption of scarce human resources. It is not obvious that either the World Bank's view of itself as a research center, or its comparative advantages, warrant undertaking such a program. At any rate, the decision should involve a view of the World Bank comparative advantages: the role of its research program; and the promise and weight of the various research suggestions advanced relative to other claims on the Bank's research resources.

The comparative advantages of the Bank lie in its close contacts with a number of developing countries; the accumulated country experience of its field and other country staff; and its financial ease, certainly as compared with such other international agencies as the United Nations. But there are some disadvantages. The World Bank is under restraint with respect to publication of country or even comparative reports unless there is agreement on the part of the countries involved. The main concern of the Bank is its lending activity, which naturally and warrantedly absorbs the major energies and resources of its staff. The role of what might be called basic research, i.e. solidly founded analysis of economic and social patterns of behavior is inevitably equivocal in what is largely an operational agency.

These comments are not intended to suggest a negative reaction to long-term research programs in the World Bank, but rather the need to think through the major purposes pursued and to appraise the conditions and possibilities for their fulfillment. The comments arise partly out of puzzlement as to why in the published work on income distribution by the Bank staff (even if released in collections of papers etc.) the obvious tests and experimentation, possible with the then and now available data, have not been applied; and, for that matter, why a wealth of country estimates of per capita product and growth rates have been released without adequate indication of their sources and

degree of solidity. It is difficult for an outsider to appraise the situation and the conditions that may have contributed to such handling of research findings; yet such an appraisal is needed to permit judgment of the types of research that can be undertaken.

One may grant freely that the emphasis in the Bank research and publications (and particularly in Mr. McNamara's speeches) on internal income distribution has aroused interest in the problem and led to sensitizing the Bank lending policy to effects on various income groups within the country. But one may ask whether such emphasis on internal size-distributions and on faulty and noncomparable data was needed to stimulate interest in world poverty; or succeeded in providing the Bank lending operations with relatively firm guidance. I have no answer to the second question, except deep doubt as to how manipulation of faulty data could have yielded reliable guides. On the first question, one may note that, given the wide disparities in per capita product among the developing countries (let alone between them and the developed regions), as well as the differences in growth rates between the low income and the higher income LDCs, there should have been no difficulty in recognizing the problem of world poverty--without a variety of necessarily faulty data on size distributions of income The low per capita level of product or of consumption in many LDCs meant necessarily that negative transient elements (e.g., a poor crop) would, in its effect on the lower income groups, jeopardize supply of means of subsistence with limited recourse to any accumulated assets; and it also meant that with even moderate internal inequality

in longer-term income levels among various groups, there would be substantial proportions of the latter at or below poverty levels. Also, any hypotheses as to possible widening of internal income inequalities in the early phases of growth and of rise in per capita product could be weighed in terms of the interplay between the rise in product and the likely decline in the income shares of the lower ordinal groups, which in conditions of substantial growth in per capita income would hardly yield a decline in the absolute per capita levels of the lower income groups. And a decline in relative standing, if largely of political significance and likely to relate to shift in income shares well above the lowest, would have to be examined by analysis and data to which a conventional size distribution of income could contribute little. Given this view of the dominance of international disparities in the absolute poverty area, and the relevance of adverse shifts in the internal income distribution largly to cases of substantial growth in per capita product, i.e. as disruptive accompaniment of vigorous growth, it is not clear that there was a justified urgency to emphasize internal income allocations that would warrant hasty compilations and inferences of the type made.

Under the circumstances, all one can do is point to the weaknesses of the empirical foundation of the size-distribution data so extensively used; the dominant importance in world inequality and poverty of differences in per capita product and growth rates among the developing countries themselves, and the need for wider and more flexible measurement and analysis of such differences; and the possible value of complementary approaches not fully

exploited. The implications of such observations for the future research program of the Bank and its priorities depend on knowledge, and appraisal of, competing fields of research, the conditions of long-term research at the Bank, the purposes of it alongside the operating functions of the Bank, none of which are within clear view. The reason for such indeterminacy is that we viewed the research in the income distribution field for its basic qualities, without reference to special uses for Bank operations. It is the appraisal of the role of such broad-gauged research in the World Bank that is difficult.