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R.A.P. Income Distribution & Employment
1977-78 (RAPIDE) (4)



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Issues

1. Role of Bank Research in Income Distribution and Employment

What kind of research should the Bank engage in? What are the advantages and limitations of research that is:

- Project-oriented
- Primary data collection
- Comparative
- Country specific
- Development process policy related

What weight should it give to:

- Original theoretical and empirical research
- Adaptation of existing research
- Measurement and tools
- Role for a 'think tank'

2. Definition of Priorities in Income Distribution Research

Has the focus on the size distribution been the right one compared to an emphasis upon socio-economic groups?

Should the emphasis have been on poverty rather than relative inequality?

Has the cross-section analyses been adequate?

Should there be more time-series emphasis?

Should there be more concern with policy instruments and politics?

Are the general equilibrium, price endogenous models a useful genre?
Should there be more emphasis upon behavioral relationships involving socio-economic groupings?

3. Definition of Priorities in Employment Research

Should there be more concern with the allocation mechanisms and institutions of labor markets over time?

How much continuing emphasis should there be on the informal sector? on rural-urban migration? Has the research in the Bank on these subjects been adequate?

Has the research on labor supply of households been useful?

How effective has the project-related research on capital-labor substitution been?

Is small scale industry a useful focus?

4. Data Collection and Processing

Should the Bank play an active role?

What are the principal needs?

- Size distribution vs. socio-economic groups
- wages and wage differentials
- price deflators

Where should it be done?

1980 Census cooperation

5. Relationship of Research to LDC's

Has the research been responsive to needs?

Has it been collaborative?

Has it been too selective?

Should it be institution-building?

6. Organization of Research

What role should the Research Committee play in establishing policy and defining strategy?

How can the "right" projects be brought forth?

Is the present vetting function performing well?

Is the evaluation process adequate?

Should outsiders have a larger role?

7. Articulation of Research

Is the relationship between the research and operating divisions of the Bank an effective one?

Does the kind of research determine the articulation?

Does the quality of the research influence the link?

Should the Income Distribution and Employment Divisions be tied more closely?

8. Dissemination of Research

Is the present system of working papers and seminars an effective internal channel of communication?

Are results communicated to policy makers and researchers in member countries?

Review of Research on Employment

Introduction

This Review is based on a sample of over twenty papers^{1/} that have been produced during the last few years and which is likely to be representative of the Bank's research efforts into employment. Perhaps the most 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 while many of the authors are "acknowledged experts in the field." On the other hand, it was often difficult to see the relationship between the subject matter of the research and the operational needs of the Bank and in this Review we take into consideration both the scholastic qualities of the research as well as its operational significance for the Bank. Indeed, it is curious that the research standards are not apparently complemented by high quality country economic work.^{2/} In an institution such as the Bank, perhaps the most important objective of research should be to improve the Bank's operational performance, and there was generally little indication, if any, in the various papers of how this objective was to be met.

The impression that is created is one of a series of one-off jobs that do not relate to any clear research strategy that the Bank might find relevant although a number of papers seemed especially pertinent. Three of the papers^{3/} were prepared for a Research Workshop on Rural-Urban Labor Market Interactions although it is unclear how the results of this Workshop

1/ Enumerated in the annex.

2/ As suggested in (4).

3/ 10, 17 and 20.

are being processed for general consumption within the Bank apart from the intention to publish the papers and proceedings. However, the papers as a whole happened to relate to a number of clear subject categories although here too the papers within these categories did not seem to relate with each other. In several cases the subject matter^{1/} overlapped fairly closely although successive or even contemporaneous authors did not seem to be working together. Therefore from a variety of perspectives the research output seemed unplanned and haphazard.

The main subject categories and the associated papers are listed on Table 1.

Table 1: Subjects Covered

- (i) Education
12, 16, 15, 18
- (ii) Rural Employment
1, 2, 7, 10, 17
- (iii) Urban Employment
13, 11, 21, 9, 22
- (iv) Shadow Wage
14, 20
- (v) Rural-Urban Migration
3, 10, 11, 14, 15, 17

The basic framework continues to be based on the distinction between the urban sector and the rural sector with a theory of rural-urban migration to link the two. However, only education is singled out for special attention and while this is linked with the issue of juvenile employment there was virtually no coverage of the following important issues:^{2/}

1/ E.g., 10 and 17, 9 and 20, 19 and 5.

2/ Discussed further in the main text of this report.

- the employment of juveniles
- the employment of women
- direct employment generation of projects
- secondary employment effects of projects
- effects of job training schemes

The subjects on Table 1 (especially ii, iii and v) are concerned with the overall functioning of the labor market. While this is important and has a bearing on the calculation of shadow wage rates, it does not relate so directly with the day-by-day involvement that the Bank has with employment matters in its project work. It is reasonable to ask that the Bank be expert at its own work which is to design urban and rural projects which among other considerations utilize labor in an efficient manner. This is very micro research rather than the macro or aggregative effort this is implied by Table 1. It should seek to know what the employment effects of its own project work are likely to be and while these considerations will require some aggregative market analysis it is unclear how the project work of the Bank is likely to benefit from a research strategy that is aggregative in nature.

The Bank should therefore be experimenting with project designs especially in the rural areas since it is there that the employment pressures (in terms of pure numbers) are likely to be greatest in the future. Rural-urban migration is a reflection of this pressure so that the roots of the urban employment situation are in the countryside. In these respects, contributions 1, 2 and 7 were particularly appropriate although only 2 was concerned with parameters that would be directly relevant to project design.

We shall review the various subjects that have been covered in more detail below. However, Table 1 indicates that relatively little direct attention was paid to the practical matter of shadow-pricing labor which is a requisite in each project that the Bank performs. It is most probably the case that the project economists are pressed even to apply fairly simple rules of thumb for shadow-pricing labor and that they lag well behind the latest theoretical developments. Therefore, as far as shadow-wages are concerned, the main priority should be the implementation of plausible yet simple formulae rather than to be refining existing concepts that have yet to be implemented. Instead, both 14 (p. 417) and 20 seek to complicate the formulae. If indeed the matter is so complicated, the challenge facing applied researchers is to identify the practical formulae that provide good-enough approximations under the circumstances.

To develop further this part of the discussion, Table 2 summarizes some of the characteristics of the papers that were reviewed.

Table 2: Character of Research

- i) Theory
2, 3, 9, 11, 14, 15, 16, 19, 20
- ii) Data Analysis
6, 7, 13, 16, 22
- iii) Econometric
2, 12, 15
- iv) Review
4, 5, 10, 17, 19
- v) Policy-related
1, 8, 18
- vi) Project-related

Table 2 underscores the theme that the work has had relatively little direct bearing on the operational concerns of the Bank (items v and vi). Instead there has been a considerable focus on theoretical subjects although as 2 indicates theoretical and econometric analysis may shed light on project-related issues. The Bank should not focus its attention on the more theoretical issues^{1/} since these may be covered in the universities and appropriate research institutes. The comparative advantage and need in the Bank is to relate existing developments to its own functional needs. Such applied research provides sufficient challenges in its own right.

The reviews were on the whole very well executed, bringing together what is known about the respective subjects in an authoritative, critical and enlightening manner. Some of these studies were more germane to the Bank than others and 5 is a fine example of how the subject of employment in developing countries may be reviewed from a very practical angle - that of labor market efficiency.^{2/} This may be contrasted with 19 which was much more theoretical. The operational arm of the Bank could benefit greatly from critical reviews of the literature that have a practical orientation. This would benefit both the project and program staffs provided that the channels of dissemination were satisfactorily established. For example, on the project side, the Bank should seek to establish itself as a repository of knowledge on the employment aspects of a range of different projects by continuously reviewing its own project experience as well as the experience outside the Bank. In this context, it is not clear how far in the past the Bank has tried to learn by doing. On the program side, the reviews listed on Table 2 are useful. Additional subjects might include:

^{1/} Excepting, of course, in the free time of the staff.

^{2/} It is ironical that 5 was written for external publication rather than internal dissemination.

- juvenile employment
- labor market modelling
- female employment
- manpower training

We now turn to a more detailed discussion of the subjects covered in Table 1.

Education

None of the papers that were reviewed sought to analyze the determinants of graduate (especially from secondary schools) unemployment which is reported to be widespread. Unless this issue has been covered elsewhere this would be an important omission in view of the Bank's involvement with education projects. The study of education and employment in urban Malaysia (12) was very useful especially in relation to the calculation of the private returns to additional years of schooling. The results on the duration of unemployment were less satisfactory and the methodology that was used in this context was not derived from any clear hypothesis about the costs and benefits of job search. For example, it might be expected that the duration of unemployment would be larger the greater the financial support that was available and the job interests of the subjects concerned would be important too. Instead the regression analysis amounts to a statistical search for significant associations rather than hypothesis testing per se. The regression results on the returns to education include many explanatory variables which may be correlated (e.g. race and education) and it is difficult to judge how robust the results really are. Most probably the presentation would have benefitted from a sensitivity analysis in terms of the specification. If, for example, racial type and

education are correlated the estimate of the private returns to schooling might be biased upwards.

Another problem is that the quality of education should be taken into consideration and not merely the quantity of years of schooling. This factor leads to a further complication that since the quality of education may vary over time (hopefully it improves) it may be inappropriate to ascribe the same importance, as is the case in 12, to the six years of schooling of a forty year old as to the same years of schooling of somebody fifteen years younger. This suggests that from a statistical point of view "age" and "education" as explanatory variables may be related thereby upsetting the interpretation of the results. These considerations would also apply to 15.

Both 12 and 15 do not consider the social returns to education in detail although the result that the private rates of return to prolonged education are high is of obvious interest. However, it might have been more appropriate to compute the expected returns to education in view of the sometimes protracted periods of unemployment that the educated sometimes have to endure. If it is the case that the educated are paid artificially high wage rates, e.g., in the civil service, the social return would be below the private return. In the Malaysian (12) and Zairian (15) cases it would be relevant to determine for policy purposes whether the private returns to education justified a complementary allocation of public resources.

In the same context, it would be important to determine (as discussed in 19 p. 26) whether the productivity effects of education were attributable to the human capital approach to education or whether education merely acted as an "egg sorter" in relation to employment openings. In the former case, investment in education could be judged along the usual lines of capital allocation,

while in the latter case this may not be so. The pursuit of education as an "egg sorter" is most probably an inefficient and ultimately self-defeating means for proving ability which developing countries in particular cannot afford to emulate. Once students have, on the whole, completed X years of education the "egg sorter" syndrome requires the completion of X + 1 years, and so on regardless of the human capital value of the marginal year of education. This important issue was not broached in the papers that were reviewed.

However, in 16 an attempt was made to compute a desirable social policy on education on the restrictive assumption that those with the greatest ability would benefit most from education rather than those who are not able to afford it. While it is obvious enough that it would be socially inefficient for wealth to be the determinant of the allocation of education resources, it would be a poor social policy that allocated the resources purely on the basis of ability. The poor of low innate ability would be vulnerable and it may be socially desirable to "redistribute" effective ability by allocating resources to this group in particular. Apart from this 16 entails a number of methodological difficulties. For example, it is assumed that wealth and innate ability are independent although it is recognized that innate ability is most probably partly genetic. Able and subsequently wealthy parents are likely to produce children who have a greater share of innate ability in which event wealth and innate ability are likely to be related in a manner which is not reflected in the model.

Ability is represented by IQ. Few educationalists would recognize this as a satisfactory basis for learning ability while the true picture is complicated by the fact that children flower at different times. The dull

seven-year-old may blossom into a diligent adolescent. Furthermore, the paper attaches too much importance to income as a determinant of ability. While the social returns to education is an important area of operational concern, the approach in 16 does not seem to be a useful one while 12 serves as a more appropriate starting-point for such a discussion.

Likewise 18 serves as a useful basis for policy analysis on pre-school age investment which might also be an aspect of a "basic needs" strategy. The argument here is that IQ incrementation due to schooling is relatively weak and that on the whole the IQ of school-leavers is largely determined by their initial IQ, i.e., when they started school. This also reflects the assumption in 16 that those with the highest IQ will benefit most from education. An important policy objective should consequently be to encourage pre- school IQ development, e.g., by providing adequate nutrition, health care, etc.

The problem here is that the evidence is ambiguous and other studies indicate that the long-term effects of child undernourishment on a range of performance indices (not only IQ) is insignificant. What is clear, however, is that the undernourished child will be a listless pupil who will fail to take full advantage of his schooling years.

The Urban Sector

Another extremely interesting contribution is 13 which exploits the comparative research advantage that the Bank has in reviewing the evidence for a range of countries. Whatever the conceptual difficulties with the notion of "informality" it seems clear enough that the urban areas in developing countries are in a crucial transitional stage in economic development and studies such as 13 are welcome indeed. In this context, 22 and 23 are especially appropriate and should be of considerable interest, although it was unclear how these studies will shed light on the matter of "trickle-down." Also it would be a

pity if they degenerated into statistical reportage (such as 6) and some theoretical superstructure about the functioning of the urban labor markets would be desirable, although these studies might well stimulate new lines of thought.

However the theoretical structure suggested in 11 needs considerable reworking and this has affected some of the arguments in 13 (p. 672) and 14 (p. 419). The main problem here is that the model that is proposed is a curious mixture of neoclassical and Keynesian assumptions and no clear reason is given for this. The urban-rural relationships are couched in terms of the familiar 2-sector neoclassical model of wage, price and quantity adjustments while the intra-urban model specifies informal sector output as a separate variable but without specifying a price to go with it. Consequently, the relationship between the two urban sectors is purely of an income-expenditure nature since no price adjustments are allowed. The framework that is suggested in 15 (pp. 2-11) is more consonant with the urban-rural structure of the model in 11.

A further conceptual problem in 11 is the use of a single time-period model for investigating what is essentially a multiple period issue since the migrant enters the informal sector in the hope of eventually funding a higher wage in the formal sector. This leads to considerable confusion in the interpretation of time in the model and the usual claim that time may be compressed does not seem to be appropriate in 11. Similarly the specification of the expected urban wage in 11 (p. 8) does not accord with the stated assumption that the migrant hopes to earn the formal wage and only settles for the informal wage as a second choice. It is therefore necessary to multiply the informal wage by the product of the probability of not obtaining formal employment and the probability of finding informal employment

once he has failed to obtain the former. The probability calculus that follows from the assumptions actually made may therefore be improved.

The notion behind the "informal" sector in 13 is that the formal sector has barriers to entry and is therefore protected while the informal sector is not. However, these "barriers to entry" amount to little more than a process of selection so that employees cannot just walk in through the door and begin work in the "formal" sector while it is assumed that they may in the "informal" sector. Most probably a closer examination (such as 22?) will reveal that even informal employment is not so accessible since street traders, etc. will have established their "territories" etc. and that the barriers to entry are merely different. Even if there were no such barriers in the informal sector, it does not seem reasonable to lay such great weight on employee selection. This suggests that the distinction between the two sectors is blurred and that there is a range of additional characteristics to consider, e.g., rigid work hours, wage contracts, etc. The main point is that urban employment is sufficiently heterogeneous so that the employment opportunities are in fact wider than might have been thought. It is a focus on this heterogeneity that is more important than a search for a 2-sector intra-urban model of employment that might revolve around the elusive concept of the "informal" sector.

13 also points out that the "informal" sector does not predominantly consist of migrants yet 14 (p. 418) suggests that "informal" sector incomes might be depressed by migrants. The reasons for this disparity are unclear since the two arguments appear to contradict each other.

The Rural Sector

While the distinction between the two urban sectors is of debatable value the rural-urban distinction continues to be important. A significant development in this context is the growing recognition that in the rural areas it is important to consider the role of more-agricultural activities in the rural areas and 1 and 7 are particularly valuable contributions. Whereas in the previous century, urban population growth was little more than one percent per annum and the urban centers could absorb the increase in the rural population, present population growth rates are so high that this model is unlikely to be workable. In addition, unlike the 19th century, there is currently no population escape valve such as the Americas to which people might emigrate en masse. Therefore the rural areas themselves will be the main source of employment growth and with the limits to agricultural growth rural non-farm employment should be a major focus of attention.

The Bank might well repeat studies such as 7 for other countries in order to ascertain what can be learned from past experience in non-farm rural development. Perhaps more important than the issues raised in 8 about small scale development is the question of non-farm rural employment irrespective of scale. 7 is indeed an excellent example of how existing data might be deployed. However, the paper offers little insight into the all-important question of why it was in Taiwan that work gravitated towards the workers in the countryside rather than the workers gravitating towards the work in the urban areas. The main intellectual challenge is the search for an appropriate model of rural-urban factor location. In the past, a disproportionate amount of attention has been paid to the question of rural-urban labor mobility to the neglect of rural-urban capital mobility. An integrated

model of all factors is required. Future work might also extend to project studies of non-farm rural employment in order to further knowledge about project design in this potentially strategic sector.

While it does not specifically appertain to the rural areas, we may consider 8 here since it is in the rural areas that the bulk of the population lives and where small scale enterprise employment might be most significant. The paper fails to draw on the previous history of small scale enterprise initiative which was not particularly successful, nor does it pitch itself at the project level which would have been more appropriate. The central case that small scale enterprises use capital more efficiently and are labor intensive is poorly presented. For example, the log-linear regressions in Annex 2 might imply different conclusions to a linear specification which the scatter indicates as being more appropriate. Also since production functions might differ between industries, the data analysis in Annex 1 does not adequately reflect this source of heterogeneity in factor use. The paper does not seem therefore to be a reliable basis for the main recommendations about small scale enterprises; however, it is noted that a research project has been prepared in this area.

The Bank has been making a determined effort to improve the productivity of the small-holder. It therefore is appropriate that it should be aware of the direct and secondary impacts of its operations in this area. Indeed, the Bank should seek to become expert on the employment effects of its own "new-style" projects. The economics of the rural household is therefore a particularly relevant objective for study and 2 constitutes a spring-board for further investigation in this area. As they stand, the results are provisional. For example, it is not clear whether the recursiveness of the model is to ease

the burden of econometric estimation or whether it is specified out of a belief that the household's production is independent of its consumption. The self-employed often produce in order to consume and a more general statement would recognize the simultaneity of both production and consumption. At the very least, this is a hypothesis that may be put to the test.

Further investigations might also experiment with production functions that are not restricted to constant returns to scale and it would have been useful to test the restrictions in the model by checking whether the calculation of leisure as a residual has satisfactory statistical properties. In subsistence, agriculture the pursuit of leisure may be more important than in western economic systems and a more direct analysis of leisure time may be appropriate.

More attention should have been drawn to the fact that the capital coefficient in the production function was not significant since many of the later derivations were dependent on this parameter, while for policy purposes it is questionable whether the unavoidable (due to data availability) short term format of the model is an appropriate idiom for policy purposes. The main concern is for the longer term behavior of the agricultural household and it seems likely that the response to price changes in particular would be distributed over a period of time which is longer than the actual observation interval.

In many respects, the Bank's projects give it the unique opportunity for virtually controlled experimentation. By gathering the appropriate information as it is generated by the project, it may be possible to observe household (and other) behavior directly over time. With the exception of the research project on capital-labor substitution in construction it is not clear how far the Bank is availing itself of the unique opportunities for data generation.

generation from its own very extensive project work.

Both 10 and 17 provide perceptive reviews of the effects of out-migration on rural productivity and income distribution and it seems a pity that the overlap between them is considerable. 17 pays more attention to the complementary out-migration of capital than 10 and both papers point to the fairly rich taxonomy regarding the relationship between out-migration and the welfare of the remaining rural community.

If indeed the remaining population becomes worse off, i.e. in contrast to the conclusions in 5 that migration is most probably stabilizing - what is the appropriate policy response? Important as these issues are, they are more matters of national policy rather than of immediate concern to the Bank and might be more appropriately research elsewhere.

Rural-Urban Migration

Table 1 indicates how pervasive rural-urban migration hypotheses are in the papers that have been reviewed. Indeed most probably a disproportionate amount of attention has been paid to the Harris-Todaro model (or elaborations on it) where it is assumed that one of the wage rates is rigid. Moreover, it is not clear how relevant this model may be as shall be discussed in the next section. While quite obviously economic "push" and "pull" factors are likely to generate migration the simple migration relationships in 3, 5, 14, 15 and 20 are unlikely to do justice to what is an immensely complicated process. Moreover, the search models that are usually incorporated within a migration hypothesis are most probably not a suitable focus for research efforts as far as the Bank is concerned although this general area is of obvious concern to the governments concerned.

Having said this it is curious that despite the attention drawn to the riskiness of urban employment (following Harris and Todaro) and various specifications of probability functions, so little attention has been paid to the issue of decision-making under uncertainty. Instead it is simply assumed that expected wage rates will be equated through migration. The question of risk is raised in 20 and 3 but the remainder of 20 does not explore the methodological consequences of this observation. This has fairly obvious implications for the calculation of shadow wage rates since wage differentials might not necessarily indicate labor market inefficiency but rational risk differentials or even less tangible aspects of expected utility. The absence of an analysis of risk was also significant in 2 where the efficiency criterion was the equation of marginal products with factor prices. Expected marginal products may have to be greater than corresponding factor prices if the variance on the former is greater than the variance on the latter in order to compensate for risk bearing (especially in agriculture) when people are risk averse. In general, the relationship between risk and decision taking (for migration, investment, employment, etc.) on the basis of the studies that were reviewed is an area where improvements might be made.

The migration work tended to neglect the inherent riskiness of rural welfare where the vagaries of weather, agricultural prices, crop diseases, etc. are at least as significant as the vagaries of urban employment. If indeed research is to continue in this area it would most probably be beneficial to break with the restrictive ground rules that were originally set up by Harris and Todaro and which continue to appear in 3, 11, 14, 15, 19, 20.

Wage Determination and Shadow Wage Rates

It is pointed out in 4 that in country economic reporting analysis of wage rates and their determination and projection are areas where improvement is desirable and possible. Some of the notions about wages and their determination that are reviewed in 9 and 19 may form a sound basis for such improvement. However, both of these studies focus on urban wage determination^{1/} and attention should be focussed on rural wage determination too in view of the importance of the rural sector in the development process.

Perhaps a greater distinction should be made between the efficiency wage hypothesis and the labor turnover model. In the former case, it would only be the very poor whose productivity might be raised through higher wages unless of course more importance is attached to the purely psychic contributions that higher wages might make to productivity (considerations that are more applicable perhaps to the alienated labor forces in the industrially advanced countries). In any event, assuming that there is a significant body of healthy and strong unskilled labor it would not be necessary for employers to pay over the odds to raise the productivity of the less fortunate unskilled work force.

Of greater interest is the labor turnover model where employers regard the wages they pay as a form of investment in the labor force and where an excessive turnover of labor would be costly. This model is particularly applicable to activities where the technology is more sophisticated. All too often it is argued that modern sector wage rates are "too high" when closer scrutiny usually reveals a series of sound reasons for the payment of higher wage rates. In this regard, the turnover model is a useful analytical device and it is likely that it will shed light on the conceptual problems associated with the "informal"

1/ If we exclude the disproportionate amount of concern with the share cropping model.

sector. What is needed more generally is an appropriate economics for the wage-employment contract which reflects the heterogeneity of both the work force and the technological needs of different employment situations. But the turnover model does not necessarily imply an unemployment equilibrium as is stated or implied in a number of the studies. Indeed, this model is part of the microeconomic theory of the Phillips Curve as developed, e.g., by Phelps and has been integrated into a macro-economic theory that leads to the so-called "natural" rate of unemployment.

In many studies (3, 5, 9, 11, 13, 14, 15, 19, 20) there was a concern for the effects of wage rigidities on the functioning of labor market. Most probably this amounts to a disproportionate degree of concern and it would be useful to ascertain how widespread fixed or minimum wage rates in fact are. This once more reflects the excessive concern with the ground rules that were originally set up by Harris and Todaro. Even if a minimum wage exists, it may not be enforced as a number of surveys imply or it may follow rather than lead wage developments elsewhere in the economy. Furthermore, the sectors affected might only constitute a small part of total employment.

Both in this context as well as that of the effects of trade unions on wage rates, it is important to distinguish between real and nominal wage rates. If they do enjoy a degree of monopoly power (and it is not clear how relevant this model is to the developing countries) trade unions may only determine nominal wage rates in the first instance since the price level is beyond their control. Therefore greater care should be taken in distinguishing between rigidities in real and nominal wage rates. Likewise with regulated wage rates.

The previous discussion has an obvious relationship with the issue of shadow pricing labor and the complex taxonomy that is identified in 20 may well be a messy truth to face. The provisional conclusion that unless there is

reason to believe the contrary (20, p. 42) the shadow wage is the market wage in the urban sector begs the question of which market wage. Given the constraints that face the Bank's project staff, the focus might more usefully be on the search for practical rules of thumb rather than a preoccupation with further refinement and the focus in 14 might have been on the application of Harberger's view that the wage rate in the informal sector should be accepted as the shadow wage rate rather than in identifying peripheral reasons for rejected this view. All that is being sought are good enough formulae that will be better than nothing at all and a secondary consideration might be to check how crude these formulae are in practice.

Sample of Papers

1. 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 Our Agricultural Household,' undated.
3. Beenstock, M., 'Risk, Unemployment and Rural Urban Migration,' May 31, 1977.
4. Beenstock, M. and M.W. Leiserson, 'Functional Review of the Treatment of Employment in the Bank's Country Economic Work,' July 1977.
5. Berry, A. and R.H. Sabot, 'Labor Market Performance in Developing Countries,' November 1977.
6. 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.
8. Industrial Development and Finance Department, 'Employment Creation and Small Scale Enterprise Development,' March 22, 1977.
9. Lal, D. 'Theories of Urban Wage Structures in Developing Countries,' September 30, 1976.
10. 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.
15. 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.
17. 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.
20. 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.

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Income Distribution

by

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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

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 particular groups can frequently be estimated on a consistent basis even though their relative size can be influenced by different sampling procedures 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

6.

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 than 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 attitude 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 by 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

upon 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 led 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 equilibrium 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 changes. 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 contributions 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 creation 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, economists 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 endorsement 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 technically 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.

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 made. 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 under-employed 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 capitae in 1960 (in 1974 prices) were 108, 221, and 489--a ratio of only 4.5 to 1. The 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.

(b) 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 Scychele 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 World Bank Atlas 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 capitās 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 countries; and that there may be discrepancies among all these three sources relating to the same country and period. Thus, the data in the World Bank Atlas 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 income. 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.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 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 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 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 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, is 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 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

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 size-distribution 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 accounts 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. 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 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 workers, 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. It 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 not 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 each 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. 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. 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 any 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 Malnutrition and Poverty (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). This tie 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.

(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-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 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 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 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 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 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. The reasons given do not appear convincing. In commenting on the weakness of the data, the discussion in Chapter I.1, Source A, states: "We assume that until better data become available, cautious use of existing 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 non-comparable 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 production-sectoring 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 size-distributions 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 than 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

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. In 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.

(1) 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, in spite, (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.

(2) The research program of the World Bank on income 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 internal income inequalities (the other factor in "absolute poverty") could be considered.

(3) The data and estimates relating to internal (intra-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 size-distributions 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.

(4) 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 industrial-occupational 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 international differences in per capita product (or variants of it) among the developing 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 non-comparable 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 largely 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.

RAPIDE FILE

WORLD BANK / INTERNATIONAL FINANCE CORPORATION

OFFICE MEMORANDUM

TO: Distribution

DATE: September 22, 1977

FROM: John H. Duloy, Director, DRC

SUBJECT: Research on Income Distribution

Arising out of the February 1977 discussion of the Bank's research program at the Board, it was decided to set up an external Research Advisory Panel on Income Distribution and Employment (RAPIDE), to review and make recommendations on Bank research in these interrelated areas. The attached briefing note on income distribution was prepared by M.S. Ahluwalia for RAPIDE. You might find it informative and useful.

Attachment

Distribution

DPS Directors
CPS Directors
Regional Chief Economists
Program Department Senior Economists

September 20, 1977

BRIEFING NOTE ON INCOME DISTRIBUTION RESEARCH
IN THE DEVELOPMENT RESEARCH CENTER

M. S. Ahluwalia

- I. Background
- II. Research Strategy Followed
- III. Future Research Directions

Annex I. List of Development Research Center Output on Income Distribution Submitted for Review

Annex II. Income Distribution Research: An Overview of Research Prospects

Annex III. Abstracts of External Research Proposals on Income Distribution Conducted in the Development Research Center

BRIEFING NOTE ON INCOME DISTRIBUTION RESEARCH
IN THE DEVELOPMENT RESEARCH CENTER

This note provides a brief review of the research program on Income Distribution conducted over the past five years in the Development Research Center. It describes the context in which the program was developed, the research strategy followed and the present state of the program in terms of progress in on-going projects and our current perception of future research directions.

A list of available research output is provided in Annex I. The object of this note is to provide a guided tour through this list for the Review Panel and not to present a detailed evaluation of this output. However, some indication of our evaluation of broad lines of work is given in the section discussing future research directions.

I. BACKGROUND

The Income Distribution Division was established in October 1972 with the broad objective of developing a research program focussing on problems of income distribution and poverty, its relationship to development, and the scope for policy intervention to achieve distributional objectives. These issues had already begun to surface in the Bank's perception of the nature of the development problem and its position on policy issues, and the establishment of a research program in this field was a natural extension of this process.

From the very outset it was amply clear that research in this field would have to be conducted along somewhat different lines from other

Bank research. It was recognised that our understanding (and that of the profession generally) of these issues was much more limited than our understanding of other areas in development economics. Not only was the empirical base for analysis of distributional problems in LDCs much weaker than in other fields, but also there was no generally accepted economic theory of distribution from which to proceed in macro and micro level studies.^{1/} Indeed, there was (and is) a strong presumption that the core of the distributional problem was political in nature and conventional economic analysis was ill-suited to studying such problems.

These considerations had an important impact on the manner in which the work program of the Division evolved. The absence of a prior research program to build upon, and the uncertainty about methodologies to be followed, implied that a substantial diversification should be attempted in the lines of research being pursued, at least in the initial phase with a subsequent narrowing of focus in the light of experience with these initial explorations. This, in turn, implied a heavy reliance on outside consultants. Furthermore, the overwhelming importance of improving the empirical base for research in this area dictated the need for an institutional arrangement in which access to primary data would be possible. This was achieved through joint research arrangements with the Economic Commission for Latin America (ECLA) and the Economic and Social Commission for Asia and the Pacific (ESCAP), both of which institutions had established contacts with statistical agencies in their respective regions.

The heavy involvement of the Division in experimental and essentially medium-term work implied that the Division should not be required to provide

^{1/} This is particularly true of the size distribution of income among households, which is the principal focus of study, especially for welfare purposes. Such theory as we have which commands wide but not general acceptance is a theory of functional distribution among factors. Even this is inextricably linked to competitive models of neo-classical theory which factor and asset endowments across households and the theory explains returns to these factors. The applicability of this theory to different LDC iterations is questionable to say the least.

regular assistance of a short-term nature to the Operational Departments of the Bank for analysis of distributional problems in operational reports. As a result, interaction with operational departments takes the form of (i) interaction in the context of research projects which are part of the medium-term research program and which may be of use and interest to operational departments, and (ii) interaction on broader policy issues such as preparing papers for senior management on issues relating to distributional objectives, poverty alleviating strategies, etc.

II. RESEARCH STRATEGY FOLLOWED

The research strategy followed was evolved over the first two years of the Division's existence, during which a number of research projects were launched and work on Redistribution with Growth was completed. This volume presented a perspective on many of the issues to be explored in greater detail by individual research projects. An explicit discussion of the different research directions which could be followed is contained in the paper "Income Distribution Research: An Overview of Research Prospects" (Annex II of this note).^{1/} This paper was discussed by the Bank's Research Committee in 1975 and these discussions were followed by a general approval of the research strategy chosen by the DRC.

This strategy may be summarised in terms of three broad areas of work, within which resources would be concentrated in the first two at least in the initial period.

^{1/} This paper was prepared for an inter-agency meeting of research funding agencies convened by the World Bank to discuss research directions in different areas of development including Income Distribution and Employment as one area.

- empirically oriented studies focussing on the facts
- quantitative models of distributional processes at the economy-wide or regional levels focussing on causal relationships and policy interventions
- studies of the impact of specific policies operating on a part of the economy conducted outside a general equilibrium framework.

A brief description of the work undertaken in each of these areas is given below.

(1) Empirically Oriented Studies

The Division undertook a number of research projects aimed at studying available data on the distribution of income in LDCs. As a result of an early attempt at compiling cross country data (Item 2(a)1 in Annex I) it became clear that the published cross country data were of extremely varying quality, and in general, not enough information was available purely from published sources even to permit evaluation of these data. It was, therefore, felt that an attempt should be made to study patterns of income distribution on the basis of primary data sources. A number of research projects were launched in this area, including two joint research projects with ECLA and ESCAP, with a wide country coverage to provide some idea of cross country differences. In addition, it was decided to study Malaysia, Thailand and Taiwan in greater depth. The output available to date is listed in 2(a) and 2(b) of Annex I.

The principal objective of these studies was to get at a better understanding of the facts rather than attempt to understand causal relationships or to draw tightly argued policy conclusions. As such they focus on

describing the degree of inequality and the extent of absolute poverty, the socio-economic characteristics of different income groups, and the nature of rural-urban differences. The draft monograph on Malaysia (Item 2b(1) in Annex I) is an example of such a study, which was used in the Bank's economic reporting in Malaysia, and in the work of the Economic Planning Unit in Malaysia in connection with the formulation of the Third Malaysia Plan. These projects have also focussed on identifying various correlates of income as revealed in the surveys (age, sex, education, employment status, occupation, sector of employment, etc.) as a first step in developing hypotheses about causal mechanisms. The Altimir-Pinera paper (Item 2(a)3) provides a summary of results for nine Latin American countries of this type of investigation.

One of the important objectives of these projects was to provide a systematic assessment of data quality in order to determine comparability over time and across countries. An assessment of reliability for Latin American countries is attempted in the paper by Oscar Altimir (Item 2(a)4) which points to the enormous gaps in our knowledge of rural income distributions and the discrepancies between available surveys and national accounts data. Anand's Malaysia study contains a detailed discussion (in Chapter 2) of the non-comparability of the 1957 and 1970 surveys and shows that judgments about changes in inequality and poverty based on these surveys are likely to be seriously misleading.^{1/}

Because of the inevitable inconsistencies between survey data and national accounts data, and the need to develop a mutually consistent data set for economic analysis of the relationship between production and distribution, a substantial effort was mounted in the Social Accounting Matrix approach

^{1/} Such comparisons have been made in a recent ILO study.

to reconciling the two. A Social Accounting Matrix was constructed for Malaysia, in collaboration with the Malaysian Department of Statistics, presenting an integrated set of disaggregated production and distribution accounts explicitly linking the structure of production to the functional distribution of value added to different factors and hence to the distribution of household incomes across different household groups, linking these incomes in turn to patterns of commodity demands on the production sectors. The draft report is listed as Item 2(b)5 in Annex I.

In monitoring the progress of these data-intensive research projects over the past two years, it has become clear that the output from different projects is likely to be extremely heterogeneous and diffuse. It was therefore planned to pull together what we have learned from these projects into a unified overview on the important issues in the measurement of inequality and poverty. An important audience for such an effort is the country economists in the Bank who are likely to be called upon to analyse problems of inequality and poverty in their operational work. This attempt at synthesising what has been learned was originally planned for 1977 but has been postponed due to delays in completing the ongoing projects.

(b) Quantitative Models

The study of causal mechanisms determining the distribution of income through economy-wide models (later extended to include regional models) was the second major area of research. The principal reason for emphasising this area was the perception that the distribution of income is quintessentially a "general equilibrium" problem, and as such must be examined in an economy-wide

context. Equally important was the consideration that the Bank makes extensive use of (relatively simple) economy-wide models in its operational work, and the development of a capacity to handle distributional issues in these models had a high potential payoff in terms of impact on Bank practice. Since the DRC had substantial experience with the construction and implementation of large scale numerical models, it was felt that this was an area in which a concentrated effort should be launched.

The Research Program included two studies of Korea and Brazil by outside consultants (Items 3(a)1 and 3(a)2 in Annex I), and a study of Malaysia (Item 3(a)4) conducted within the DRC. All three can be described as multi-sector, multi-factor, multi-income group Walrasian general equilibrium models, with endogenous competitive price determination and price responsive demands. By and large the models succeed in the original research objective of building in a high order of substitution and price responsiveness into a general equilibrium framework with considerable disaggregation among factors and household types. However, in retrospect there is considerable skepticism about whether these are the crucial features which determine the distribution of income, especially in a dynamic long-term context.

The fruitfulness of this approach has been extensively discussed within the DRC over the past several months, and with non-Bank researchers at a Bank-sponsored conference in Bellagio in April 1977. Although there is no consensus on this issue at this stage, and indeed a workshop to discuss this line of work is planned for later this year, there is a strong suspicion that these studies do not take us very far in capturing the relevant forces

determining the distribution of income at an economy-wide level. For a concise statement of this evaluation, see Srinivasan's summary paper on the Bellagio conference (Item 1(2) in Annex I).

In addition to economy-wide models, an attempt was made to study the distributional consequences of large agricultural investments in a regional context. A study of the Muda River Valley in Malaysia was undertaken attempting to identify the distribution of benefits of the Muda project and the nature of linkages between agricultural and non-agricultural activity (Item 3(b)2).

(c) Specific Policy Instruments

The third element in the research strategy was the study of the impact of specific policy instruments operating on a part of the economy. As stated above, because of the concentration on the first two areas of work, this area received relatively limited attention. Nevertheless, a number of studies were undertaken in this area including a study on the distribution of benefits of public expenditure in Colombia (Item 2(b)2), and the role of food price and food supply instruments in affecting poverty and malnutrition (Item 4(a)1).

The role of price intervention in Agriculture was recognised as an important candidate for study and an early review paper was commissioned (Item 4(d)1). Subsequently an attempt was made to develop a formal methodology for studying the direct and indirect effects of price intervention in agriculture without having to specify a general equilibrium model (Item 4(d)2). No decision has been taken so far whether to pursue this methodology in empirical applications although some of the ideas developed therein have been applied in research studies on agricultural price policies conducted by the operational departments.

III. FUTURE RESEARCH DIRECTIONS

While the bulk of the effort at present is directed at completing the ongoing research projects in the areas described above, some effort has been devoted to identifying future directions of work, many of which are a natural outgrowth of ongoing work. The following directions are particularly important.

(a) Time Series Studies of Income Distribution

The research program described above has produced little by way of time series analysis. The Taiwan study (Item 2(b)1) presents an account of change in inequality over a period of about a decade. This project was originally expected to cover two other countries (Colombia and the Philippines), but in the course of the project it was decided to limit its coverage to Taiwan in order to permit a more thorough examination of the available data. The only other attempt at time series analysis is Ahluwalia's study on rural poverty in India over a period of two decades (Item 2(b)4). This study shows that many of the generalisations about increasing absolute poverty over time are not borne out by the data. Furthermore, it shows that given the inconsistencies between survey data and national accounts data, much depends upon how these two data sources are combined.

What is needed is systematic time series studies which tell the story of distribution and poverty over time, using both survey and national accounts data, and examining distributional change in terms of both aggregative measures of inequality and absolute poverty, as well as in terms of indices of living

standards of different socio-economic groups.^{1/} At present, we are considering two different studies along these lines. Discussions are underway with the Latin American Region of the Bank to undertake a time series study of Mexico and data availability is being negotiated with the Mexican authorities. A second research project being prepared for discussion is a comparative study of Sri Lanka, and the Indian states of Kerala and Punjab which display very different growth and institutional experiences.

(b) Aggregative Economy-Wide Models

As a reaction to the limitations of the multi-sector general equilibrium models, it has been argued that research should focus on much more aggregative models of the development process in the tradition of the literature on economic dualism. The principal objective of this research would be to develop innovative theoretical structures for the analysis of distributional questions. A research project along these lines is currently under preparation and will be available for discussion in October 1977.

(c) Rural Factor Markets

The ongoing work on economy-wide models and agricultural sector models shows that the treatment of factor markets in these models is extremely inadequate. In particular, rural labour markets cannot be viewed in isolation from other factor markets in the system, especially the conditions of tenancy and the nature of access to capital. A research project examining these problems and focussing on the inter-linked nature of labour, land and capital

^{1/} The story of distributional change in terms of inequality may be quite different from the story in terms of income levels of different groups. A systematic treatment of the relationship between development and distribution over time calls for a description of the impact of development on both the incomes and relative sizes of different groups.

markets is currently under preparation. It should provide a better understanding of the functioning of factor markets in rural areas, which should help to improve the treatment of these markets in quantitative models such as (d) below. An early review paper focussing on labour markets is listed as Item 4(b)3.

(d) Agricultural Models

A major research project is being prepared aimed at studying the distributional implications of different types of interventions in the farm economy. Three different regions in India will be chosen and quantitative models will be constructed of the farm economy distinguishing between farms of different sizes and with different terminal arrangements. Landless labour households will also be distinguished as a separate group. A proposal for a six-month phase of defining more precisely the methodology to be followed in the project has been submitted to the Research Committee. This proposal was developed jointly with the South Asia Region.

(e) Food and Nutrition

Some thought has been given to developing a research project in this area focussing on the scope for price and quantity intervention in the food-nutrition area following on the ideas explored in the Selowsky-Reutlinger book (Item 4(a)1 in Annex I). This item is the least defined in areas of future research being considered.

The above areas of research are all in the stage of research preparation. If all these areas are to be pursued, it would absorb resources at least at the scale currently deployed in the Division and probably even going beyond the current level.

Annex I

LIST OF DEVELOPMENT RESEARCH CENTER RESEARCH OUTPUT
ON INCOME DISTRIBUTION SUBMITTED FOR REVIEW

LIST OF DEVELOPMENT RESEARCH CENTER RESEARCH OUTPUT
ON INCOME DISTRIBUTION SUBMITTED FOR REVIEW

This list of research output is grouped according to the major areas for research demarcated in the research strategy followed. The list includes papers at very different stages of completion. In general only those papers that are at the stage of drafts for outside circulation have been included. However, in some cases, papers that are at a preliminary first draft stage have been included to provide a flavour of ongoing research. Items underlined are of book or monograph length.

1. GENERAL OVERVIEW

- (1) H.B. Chenery, et. al., Redistribution with Growth: An Approach to Policy, Oxford University Press, 1974.
- (2) T.N. Srinivasan, "Development Policies and Levels of Living of the Poor: Some Issues", Summary Report on Bellagio Workshop on Analysis of Distributional Issues in Development Planning, May 1977 (mimeo).

2. EMPIRICALLY ORIENTED RESEARCH

(a) Cross Country Studies of Income Distribution

- (1) S. Jain, Size Distribution of Income: Compilation of Data, Johns Hopkins University Press, 1975.
- (2) M.S. Ahluwalia, "Inequality, Poverty and Development", Journal of Development Economics, 3, 1976, World Bank Reprint Series 36.
- (3) O. Altimir and S. Pinera, "Decomposition Analysis of the Inequality of Earnings in Latin American Countries", August 1977 (mimeo).
- (4) O. Altimir, "Income Distribution Estimates from Household Surveys and Population Censuses in Latin America: An Assessment of Reliability", November 1976 (mimeo).
- (5) P. Musgrove and R. Ferber, Finding the Poor. On the Identification of Poverty Households in Urban Latin America: A Study of Bogota, Medellin and Lima, August 1976 (mimeo).

(b) Country Specific Studies

- (1) J.C.H. Fei, G. Ranis, S.W.Y. Kuo, Equity with Growth: The Taiwan Case, 1977 (mimeo).
- (2) M. Selowsky, The Distribution of Public Services Across Income Groups: A Case Study of Colombia, May 1977 (mimeo)

- (3) S. Anand, The Size Distribution of Income in Malaysia, Nov. 1977, (mimeo)
- (4) M.S. Ahluwalia, "Rural Poverty and Agricultural Growth in India", June 1977.
- (5) M.S. Ahluwalia, F. Lysy, G. Pyatt, J. Round, J. Nolan, and Tendulkar, A Social Accounting Matrix for Malaysia, November 1976, (mimeo).

(c) Problems of Measurement

- (1) N. Kakwani, Income Distribution Methods and Applications, February 1977 (mimeo).
- (2) R. Eckaus, "Report on an Indirect Approach to Measuring the Size Distribution of Income", March 1977 (mimeo).
- (3) S. K. Singh and G. S. Maddala, "A Function for Size Distribution of Incomes", Econometrica, Vol. 44, No. 5, September 1976.
- (4) 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).
- (5) G. Pyatt, "Distribution of Income and Wealth: On International Comparisons of Inequality", American Economic Assn., February 1977, Vol. 67, No. 1.
- (6) G. Pyatt, "On the Interpretation and Disaggregation of Gini Coefficients", The Economic Journal 86 (June 1976), World Bank Reprint Series No. 38.
- (7) C. U. Chiswick, "On Estimating Earnings Functions for LDCs", Journal of Development Economics 3 (1976), World Bank Reprint No. 44.

3. QUANTITATIVE MODELLING OF DISTRIBUTIONAL PROCESSES

(a) Economy Wide Models

- (1) 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 Run, Stanford University Press, November 1977 (joint publication Stanford University and Oxford University, U.K.).
- (2) E. Bacha, L. Taylor, F. Lysy, Models of Growth and Distribution in Brazil, 1977, (mimeo).
- (3) M.S. Ahluwalia and S. Tendulkar, "Annex V. Input/Output Analysis of Growth 1970-1980", 1976, (mimeo).
- (4) M.S. Ahluwalia, F. Lysy and G. Pyatt, "A Price Endogenous Model of Malaysia: Some Static Experiments", May 1977, (very preliminary draft) (mimeo).

(b) Multi-sector Regional Models of Distributional Processes

- (1) C. Bell, S. Devarajan, P. Hazell, R. Slade, "A Social Accounts Analysis of the Structure of the Muda Regional Economy", November 1976, (mimeo).
- (2) C. Bell, P. Hazell and R. Slade, "Autonomous Growth and Project Impact in the Muda Regional Economy: 1967-1972", September 1977 (mimeo).

(c) Aggregative Models of Distributional Processes

- (1) L. Taylor and F. J. Lysy, "Vanishing Short-Run Income Redistributions: Keynesian Clues about Model Surprises", Bellagio Workshop on Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).
- (2) C. Bell, "A Simple Dualistic Economy in a Comparative Statics Setting", Bellagio Workshop on the Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).
- (3) C. Lluch, "A Model of Employment and Income Distribution", Bellagio Workshop on the Analysis of Distributional Issues in Development Planning, April 1977, (mimeo), revised July 1977.

4. OTHER AREAS OF ONGOING AND FUTURE RESEARCH

(a) Food Nutrition and Other Human Capital Aspects

- (1) S. Reutlinger and M. Selowsky, Malnutrition and Poverty: Magnitude and Policy Options, World Bank Staff Occasional Paper No. 23, 1976.
- (2) S. Reutlinger and M. Selowsky, "Policies to Increase Calorie Consumption in Children: Cost Effectiveness Comparisons", Notes for the Bellagio Workshop on the Economics of Nutrition Oriented Food Policies and Programs, August 25-28, 1977, (mimeo).
- (3) M. Selowsky, "A Note on Preschool-Age Investment in Human Capital in Developing Countries", Economic Development and Cultural Change 24 (July 1976), World Bank Reprint Series No. 32.
- (4) S. Pinera and M. Selowsky, "The Economic Cost of the 'Internal' Brain Drain: Its Magnitude in Developing Countries", World Bank Staff Working Paper No. 243, September 1976.

(b) Labour Markets

- (1) M. Selowsky and S. Pinera, "Unemployment, Labor Market Segmentation, the Opportunity Cost of Labor and the Social Returns to Education", World Bank Staff Working Paper No. 233, June 1976, (forthcoming in the Quarterly Journal of Economics).
- (2) R. McCabe, "Education, Administered Wage Rates and the Size Distribution of Income in Urban Zaire", November 1976, (mimeo).
- (3) N. Stern, "On Labour Markets in Less Developed Countries", March 1977, (mimeo).
- (4) G. Pyatt, "Labor Markets and the Efficiency of Labor", Bellagio Workshop on Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).

(c) Sharecropping

- (1) C. Bell and P. Zusman, "A Bargaining Theoretic Approach to Cropsharing Contracts", World Bank Reprint Series No. 45, (American Economic Review 66 (September 1976)).
- (2) C. Bell, "Some Tests of Alternative Theories of Sharecropping Using Evidence from Northeast India", December 1976, (mimeo).
- (3) P. Bardhan, "Variations in the Extent and Forms of Agricultural Tenancy: An Analysis of Indian Data Across Regions and Over Time", December 1976, (mimeo).
- (4) C. Bell and P. Zusman, "Sharecropping Equilibria with Diverse Tenants", June 1977, (mimeo), (forthcoming in the Economie Appliquee).
- (5) C. Bell, "Production Conditions, Innovation and the Choice of Lease in Agriculture", (forthcoming in Sankhya: The Indian Journal of Statistics, 1976, Vol. 28, Series C, Pt. 4).

(d) Price Policy in Agriculture

- (1) J. Mellor, "Agricultural Price Policy and Income Distribution in Low Income Nations", World Bank Staff Working Paper No. 214, September 1975.
- (2) T. Bertrand, "Market Interferences and Income Distribution: A Methodology for Studying the Agricultural Sector in Less Developed Economies", August 1977, (mimeo).

Annex II

INCOME DISTRIBUTION RESEARCH:
AN OVERVIEW OF RESEARCH PROSPECTS*

*Paper prepared for the Inter-Agency Group of Research Donors for Discussion of Directions of Research in Income Distribution and Employment.

March 14, 1975

INCOME DISTRIBUTION RESEARCH:
AN OVERVIEW OF RESEARCH PROSPECTS*

Montek S. Ahluwalia

<u>Contents</u>	<u>Page No.</u>
I. Introduction	1
II. The Need for a Theory and Some Research Implications	2
III. Development of a Factual Base.	9
IV. Inequality Measures and Welfare Indices.	13
V. Economy Wide Modelling	15
VI. Modelling a Part of the Economy.	22
VII. Studying Isolated Relationships.	23
VIII. Historical Experience of Some Countries.	28
IX. Studies of Particular Policy Instruments.	30

*My perceptions as recorded in this paper have drawn heavily upon many discussions with Messrs. Chenery, Bell, Duloy, Jolly and Rao during the writing of Redistribution with Growth.

I. INTRODUCTION

In recent years problems of income distribution have come to occupy the centre stage of development economics. Policy objectives are no longer defined solely in terms of accelerating the rate of growth but also of ensuring that growth is sufficiently broad based to reach the poorer groups in society. These considerations have naturally focussed attention on our long neglect and scant knowledge of this area. The extreme expression of this point of view would have us believe that we lack both an adequate theory of the determinants of income distribution and reliable data on the state of income distribution in most underdeveloped countries. On this view, we are in an area of "basic ignorance" in which major intellectual breakthroughs are needed before we can begin to handle the problem. Those who reject this uncompromising agnosticism offer at best a half-baked alternative. While accepting that we lack an adequate theory of distribution in the sense of a precisely formulated and widely accepted system capable of answering most of our questions, they argue that we are not wholly in the dark. On this view we have a fairly substantial understanding of the basic ingredients of such a theory, although we may not know enough about each of these ingredients and indeed may know very little about how to put them together!

The purpose of this paper is to document the extent of our ignorance in this area and to provide an overview of the research directions that should be followed to close this gap. In presenting this overview, I have attempted to identify research directions that are particularly important from the point of view of policy making. Where relevant I have

referred to research projects currently underway which are pursuing these directions.

II. THE NEED FOR A THEORY AND SOME RESEARCH IMPLICATIONS

The first step in developing an overview is to consider how far we are from having an adequate theory of the determinants of income distribution. This is best done by outlining the kind of distribution theory we need, and then considering whether anything currently available meets these requirements.

The detailed shopping list of "features" for a theory of income distribution will obviously vary with the nature of the particular problem and this will differ from country to country. In general, however, a satisfactory theory must meet the following requirements.

- (i) The theory must explain the distribution of income between the various income groups in which we are interested. These groups will not necessarily correspond to the familiar theoretical distinction between capital and labour. In most underdeveloped countries they include groups such as small farmers and other "self-employed" categories.
- (ii) It must integrate this explanation with the process of growth in the economy. This integration of distribution and growth is crucial for both analytical and policy purposes. It is important analytically because the distribution of income and the rate of growth are both the end products of a set of complex interactions in the economy which must be modelled

as a simultaneous determination problem.^{1/} It is important for policy purposes since distributional objectives are best defined not statically in terms of distribution shares, but dynamically in terms of the growth of income of different groups.

(iii) Finally, if the theory is to be relevant for policy making, it must isolate and quantify the impact of "instrument variables" on "target variables" such as the incomes of different groups. The scope for policy is then determined by our ability to manipulate policy instruments to achieve desired outcomes in terms of "target variables". A distribution theory which does not point to any such policy instruments is of little interest to policy makers concerned with **changing the distribution of income.** In other words, we need

In other words, we need a theory of growth for a "segmented economy" consisting of several income groups; a theory which explains the level of income in **each group and the growth of these incomes over time.**

The formal structure of such a theory can be described as follows:

It would consist of m income equations, expressing income of each of m "segments" of the economy in terms of endogenous variables $(x_1 \dots x_n)$ and

^{1/} This is implicitly recognised even in the popular literature in which much is made of the fact that income distribution affects growth and growth affects income distribution. Attempts to treat these relationships as isolated relationships are doomed to failure.

exogenous variables ($z_1 \dots z_p$). It would also contain n equations for the determination of the n endogenous variables. The potential complexity of such a theory can be easily seen by writing these equations in completely general (and therefore somewhat trivial) form.

$$\begin{aligned} y_1 &= f_1(y_1 \dots y_m, x_1 \dots x_n, z_1 \dots z_p) \\ &\cdot \\ &\cdot \\ &\cdot \\ y_m &= f_m(y_1 \dots y_m, x_1 \dots x_n, z_1 \dots z_p) \\ \\ x_1 &= g_1(y_1 \dots y_m, x_1 \dots x_n, z_1 \dots z_p) \\ &\cdot \\ &\cdot \\ &\cdot \\ x_n &= g_n(y_1 \dots y_m, x_1 \dots x_n, z_1 \dots z_p) \end{aligned}$$

A system of equations of this type meets the requirements for a theory listed above. For given values of the exogenous variables $z_1 \dots z_p$ we can solve these equations to obtain $y_1 \dots y_m$, i.e. income levels of the m income segments. The time path of $z_1 \dots z_p$ then gives us the dynamic solution of the model in terms of the growth of income of each group (thus solving simultaneously for the rate of growth of the economy and changes in distribution). Finally, the scope for policy intervention is given by the extent to which we can set one or more of the variables $z_1 \dots z_p$ to behave as desired.

The complexity of such a system is illustrated by two features of this system of equations. Firstly the income of one segment y_i may be linked to the income of another segment y_j either directly or indirectly through the effect of y_j on an endogenous variable x_s which directly affects y_i . An example of direct linkages is provided by the phenomenon of income transfers from migrant workers in urban areas to families in rural areas. Similarly, indirect linkages are exemplified by the effect of income in one group through its consumption pattern on the demand for particular factors and thence to the income of other groups. Secondly, policy instruments may affect more than one income group and sometimes in opposite directions. The net effect of a particular policy instrument on the income of a particular group therefore depends not only on its first round impact but also on its second round impact via income linkages from other groups. Optimal design of policy must obviously take account of these various linkages -- no easy task when we consider that some of these instruments (e.g. education) only become effective over medium to long run time horizons.

Judged by these standards we are far from having a satisfactory theory which integrates growth and income distribution in a unified explanation of the development process. At best we are beginning to piece together some of the components of the total picture, but our knowledge of each of these components and most of all our ability to put them together with any confidence is still fairly limited. The task of research is to expand this piecemeal appreciation of the problem into a syste-

matic whole and the alternative directions of research discussed in this paper should be viewed in this context.

In order to better organise the discussion of these alternatives I have found it useful to distinguish between the following categories.

- (i) Developing a factual base for the study of income distribution is universally regarded as a high priority area. Knowledge about patterns of inequality in LDCs, changes over time and differences across countries is a necessary first step in defining the dimensions of the problem.
- (ii) Measurement of inequality and social welfare. This is an important area of research for the formal incorporation of distributional considerations into economic planning and performance evaluation.
- (iii) Formal modelling of growth and distribution processes in an economy wide context. The central feature of this approach is the precise mathematical specification of a set of equations corresponding to Equation 1 above.
- (iv) Modelling a part of the economy which is particularly important from the point of view of income distribution by treating it as "linked system" or "submodel". Here the techniques used are as formal as in (iii) above but the exercise focusses on a part of the economy.
- (v) Studying isolated relationships in an economy which are of particular interest from the point of view of income distribution. Such relationships may be technological such as for example the degree of substitutability

between capital and labour (or between high skilled and low skilled labour) or they may be behavioural such as the degree of substitutability between commodities in consumption or the variation in savings behaviour across socio-economic groups, etc.

Items (ii) - (v) above are all characterised by the use of formal quantitative techniques. Such techniques have a strong intellectual appeal because they permit "logical consistency" within the postulated framework of inter-relationships. This appeal is greatly strengthened when research is addressed to a technocratic policy making audience where results are easiest sold when they quantify the impact of this or that policy intervention. But to limit ourselves to this rarefied world would be wholly unwarranted. A balanced research program should therefore include research activity following much less formal methodologies in pursuit of the same broad objectives. The following are particularly important in this context.

(vi) Studying the historical experience of growth and distribution

in particular countries, in order to isolate causal relationships and policy lessons. Such studies differ from (iii) above in that they do not use formal models, but they are nevertheless based on some implicit "theory" which underlies the identification of causes.

- (vii) Studying the impact of particular policy instruments in one or more countries by a combination of partial equilibrium techniques, heuristic reasoning and ad hoc hypothesis testing. In this category I include studies of agricultural price policy, modern sector wage policy, trade policy, public expenditures, etc., and at a more micro level studies of particular schemes for rural development, land reform, land settlement, co-operatives, urban housing, etc.

The classification scheme presented above illustrates an important aspect of research in income distribution, viz. that a balanced research program would consist of disparate components, each tackling the problem from a different perspective at different levels and using very different methodologies. Direct comparisons of research projects across such differences are obviously extremely difficult, if not actually impossible.^{1/} It is easier to determine the relative priority to be given to a broad direction of work, and then to evaluate and compare projects within these broad classifications. In the sections that follow, I have attempted to deal with each of the seven components outlined above. Not surprisingly, I have dealt with some more extensively than others.

^{1/} Such comparisons are particularly difficult for someone currently involved in research in this area and therefore suffering from all the professional and methodological prejudices one would expect to find in a protagonist.

III. DEVELOPMENT OF A FACTUAL BASE

It is widely recognised that the lack of "good data" on income distribution imposes severe limitations on further research in this area, and as a result there is considerable agreement on the priority to be accorded to remedying the situation. But what exactly does it mean to say we need more data? It is useful to distinguish in this context between two types of data requirements. Firstly there is the need for information on the degree of inequality as measured by one or the other index (the Gini coefficient, income shares of ordered percentiles, etc.) and secondly there is the need to study the "anatomy" of income distribution, i.e. to relate the observed patterns of income distribution to the various socio-economic factors which we analyse in studying the economy. The present state, and future prospects, of research in both fields is summarised below.

(a) The Degree of Inequality

Information on the overall degree of inequality is in some sense "basic information". It is important, if for no other reason, because it serves to describe an important dimension of development, and can be used to compare the distributional situation across countries or for the same country over time.

There has been a substantial increase in the availability of data on the size distribution of personal income and various summary measures of inequality derived therefrom. This expansion of the data set is due to both the results of new surveys being published and old surveys being dug out in response to the greatly expanded demand. The

quality of the available data varies enormously from country to country and even for the same country over time. In general, data on income distribution is very inaccurate -- much more so than national accounts. As a result, comparisons between any two observations is subject to quite substantial error.^{1/} These data are however useful for cross country analysis through which we can search for broad empirical regularities between income distribution and various aspects of economic development and economic structure. To date the major contributions in this field are Adelman and Morris (1974), and Chenery and Syrquin (1975). Further cross-country analysis is currently being conducted on a recent, and much expanded, compilation of data prepared in the course of the ongoing work on income distribution in the Development Research Center.^{2/}

My summary assessment of research in this field is that sufficient effort has already been devoted to compiling available data, and further mining will simply produce low grade ore. Much more important is to look towards the future expansion of the data base as the more recent (and generally much better) survey results are processed and made available. The systematic compilation and presentation of such data with adequate documentation about the nature of the surveys on which they are based would permit further testing of hypotheses, through cross

^{1/} This is particularly important in making intertemporal comparisons for particular countries where the differences in distribution indices are usually very small and may not therefore be statistically "significant".

^{2/} The data are reported in Jain [1974]. The cross-section results are currently being written up and will be reported in a paper by M.S. Ahluwalia entitled "Income Distribution and Development: Some Stylised Facts".

section analysis and its long awaited extension (for some countries at least) into time series analysis. This extension into time series analysis is perhaps the more important of the two. Policy makers are most concerned about short and medium term possibilities, and these simply cannot be captured by the very broad sweep of cross section studies.

(b) The "Anatomy" of Income Distribution

Despite the potential for hypothesis testing as described above, it is increasingly being felt that mere measurement of inequality is ultimately of limited interest. What is much more important as a direction of research is to examine the socio-economic characteristics of different income groups in particular countries, in order to be able to move from statistical frequency distributions to economically definable "segments" which can then be incorporated into an analysis of a segmented economy.

This is best done when different income groups correspond to homogeneous socio-economic groups, which relate to the aggregate economy in a particular way. What are the kinds of groups that are relevant from this point of view? The usually quoted examples of low income "target groups" are small farmers, landless labourers, urban unemployed, urban low-wage informal sector employees, unskilled workers, low income self-employed, etc. While this list is easily enumerated on the basis of casual empiricism, we do need to develop a stronger factual basis for the enumeration, and we need to identify various characteristics of these groups. What are their income sources? To what extent do they rely on wage employment? What are their skill characteristics and education levels? This requires analysis and cross tabulation of existing income (or consumption)

Research of this type is useful because it identifies target groups not simply in terms of income, but in terms of groups which can then be studied more carefully and for which special policies can be devised. The identification of "relevant" groups in this sense has an immediate impact on the kind of models we use, the theories we embody in them, and the policy instruments we apply. If a large number of the poor are small farmers and landless labourers both our theories and models need to direct more attention to this sector. If many of them are self employed we need to focus not simply on the determination of wage employment and wage rates but on income and productivity constraints in this sector.

While emphasising the importance of such research, we should also recognize its limitations. Although it suggests policy directions it does not by itself give us a quantitative guide to policy. From the fact that a large number of the poor are self employed it does not follow that the optimum long term solution is to raise productivity in this sector. More likely, it is a mixed strategy in which development proceeds by absorbing more and more of the low income self employed in the modern sector, but in the interim (which may be quite long), something must be done to raise productivity in the traditional sectors. The problem of policy formulation is to make "realistic" assessments of what this length of time is likely to be in order to determine an appropriate mix of policies directed at the modern and traditional sectors. The difficult problem is not whether something must be done for the informal sector but how much should we do and in what way? The answers to these questions come from the more "analytic" research efforts to which we now turn.

IV. INEQUALITY MEASURES AND WELFARE INDICES

In my view this is for the most part a sterile field for future research because contributions recently made will stand for quite some time. A brief summary of the state of the art is given below and an attempt made to justify the dismissal.

Until very recently papers on income distribution were almost always papers on measures of inequality dealing either with the properties of these measures or the application of one or other measure to observed distributions. Into this placid atmosphere A.B. Atkinson [1970] injected the perceptive (if now somewhat obvious) observation that if we are ultimately interested in social welfare we should examine whether the various inequality measures give the same ranking of alternative distribution as a measure of social utility applied to the same distribution. The following propositions sum up the resulting debate to date.

- (i) None of the traditional inequality measures gives the same ranking for two distributions (with the same mean income) as any (concave) social utility function. In other words, if all that we know is that the utility function is concave we cannot be sure that between two alternative distributions the one with lower "inequality" yields the higher social welfare.
- (ii) If we know the parameters of the utility function, we can, of course, construct an appropriate inequality measure. Atkinson's measure, for example, reflects the loss of utility due to inequality. But, of course, if we know the utility function, we can choose between distributions by calculating social welfare without reference to an inequality measure.

- (iii) It follows that normative judgments should be made not on the basis of inequality measures but on the basis of measures of social utility. The search for an appropriate social utility function is therefore an important line of research to emerge from this area.

Do we need social utility functions in practice? We may distinguish between three types of uses to which they may be put.

- (i) Monitoring historical performance. Social utility functions can be used in conjunction with data on the level of income and the distribution of income to determine the growth of social utility -- a better index of performance than growth.^{1/}
- (ii) Derivation of project analysis weights. Project analysis techniques which reflect distributional preference may require "weights" derived from the parameters of a social utility function. These weights reflect the social preference to be accorded to the same increment of income accruing to different income groups.
- (iii) Specification of maximand in optimising models. If a model generating incomes of different groups $y_1 \dots y_m$ were to be constructed and used as an optimising planning model we would need to define a welfare function $U = U(y_1 \dots y_m)$.^{2/}

Of these three possible uses the most likely practical need is that of estimating distributional weights in project analysis. The social utility

^{1/} For an illustrative application of this approach to some countries, see Ahluwalia and Chenery [1974].

^{2/} The optimum solution of the model would then be given by the time paths of $Z_1 \dots Z_p$, which maximises U over time.

function most frequently used in this context is of the individualistic additive separable type in which $U = y^{-\beta}$. Although a number of objections can be advanced to the use of this function it is also extremely convenient in many respects and certainly capable of reflecting quite different degrees of egalitarianism in social preferences.

V. ECONOMY WIDE MODELLING

The construction of economy wide models, which solve simultaneously for growth and income distribution, has obvious appeal as a direction of research. The structure of such a model gives us a precise statement of the "integrated theory" we need and it is only through intensive study of such structures that we can come to a thorough understanding of the complex forces involved. Nor is this simply a matter of intellectual aesthetics. Such models are also needed for policy purposes; in a highly simultaneous system, we cannot expect to trace the final impact of a change in one or more policy instruments without solving the system as a whole.

(a) The State of the Art

What sort of model would begin to meet these requirements? Clearly, the object of the exercise is not simply to produce a model, which determines the distribution of income in a purely mathematical sense but one which does so while reflecting the important underlying processes in a sufficiently realistic way. The identification of these processes gives us the ingredients of the theory we are working towards. Of these, the

following are particularly important in producing a realistic policy oriented model of "distribution cum growth".

- (i) Substitution in production and in final consumption (including foreign trade possibilities). The need for such substitutability has long been recognised in modelling the process of growth. It is particularly important for employment and income distribution since it permits flexibility in generating appropriate factor demands through the choice of technique for given products and through substitution in favour of labour intensive products.
- (ii) Fragmentation of the market for capital which makes "access" to a key input both difficult and costly for certain groups. The resulting duality in the market typically discriminates against small farmers and small businessmen who -- even if not actually in the poverty group -- are probably more closely linked to these groups than larger producers.
- (iii) Fragmentation of the market for labour whereby the labour market is effectively split into the rural and urban sector, with migration playing an "equilibrating" role between the two. More realistically, the urban labour market may also be fragmented into a high wage "protected" sector and a low wage, market clearing, informal sector.
- (iv) Pre-existing patterns of asset concentration and the processes whereby these patterns are perpetuated over time, (e.g. through differential rates of saving).
- (v) The role of human capital in offsetting the concentration of physical capital and providing an avenue by which large sections of the population might share in the benefits of growth. These

relationships are particularly important since governments may be more willing to adopt highly reformist attitudes in the field of education policy than in areas such as concentration of wealth and landholdings.

- (vi) The role of technical progress in shifting the balance in favour of some sectors (usually the "modern sectors") or some factors, with consequent effects on factor incomes and therefore on income distribution.
- (vii) The determinants of investment behaviour in the system, which determines the allocation of investment between sectors and ultimately the pattern and potential for growth of the economy.

How far are we from having such a model? My own summary presentation of the state of the art would read as follows.

- (i) We do not at present have any model framework that incorporates all the elements listed above even in a purely analytic formulation.
- (ii) Significant progress has been made in incorporating substitution in consumption and production into what are essentially neoclassical growth models with several consumer classes.^{1/} Solution algorithms now available make it possible to solve such models statically, i.e. to produce "market clearing" output prices and factor prices while linking factor incomes to size distributions of income which generate consumer

demand. It should be noted however that this requires quite restrictive specifications of the production function (non-increasing returns to scale) and that no analytical proof is available of the uniqueness of such solutions.

- (iii) The models in (ii) above have not however made much headway beyond the incorporation of a static price theory reflecting substitutability in demand and technology choice. Fragmentation of markets and inequality of access can be imposed on the models, by arbitrary distinctions between modern and traditional sectors which face differential factor prices, but these differentials are not themselves endogenised.^{2/} The models are particularly primitive in their treatment of (a) technological innovation (b) investment and (c) patterns of asset concentration.

Deficiencies (a) and (b) reflect gaps in the existing growth theory. Deficiency (c) represents the major gap in moving from the functional distribution of income generated by the price solution to the size distribution of income in which we are primarily interested. We can of course argue that.

- (iv) The models that are currently in the pipeline present major estimation problems even with their limited ambitions in terms of specification and structure. Further experimentation will almost certainly run into insuperable estimation difficulties given the data availability at present.

^{1/} See for example Adelman and Robinson [1974] and Lopes and Taylor [1974].

^{2/} This would require specification of different market demands and supplies for different capital markets; in other words an extension into flow of funds analysis

The list of difficulties enumerated above may sound depressing but it is not presented to discourage further work along these lines. Quite the contrary it is presented so as to face up squarely to the problems while arguing strongly for further work in this area. Clearly the solution lies not so much in attempting to construct highly complex models, which attempt to endogenise everything, but in constructing models which deal with a part of the problem at a relatively sophisticated level in an economy wide setting while leaving the treatment of other parts at a fairly primitive level as dictated either by difficulties of specification or of estimation.

(b) Alternative Model Types

The following are some examples of the types of economy wide models which are currently in use in the repertoire of research on income distribution. These models represent quite different levels of sophistication so that it is worth considering what their relative merits might be.

Leontief Models without Prices

Input-output models are important if only because planning agencies are familiar with such models and improvements in this field will therefore have maximum impact. From a purely research point of view, such models are too primitive to be of much interest. Nevertheless, they can be easily expanded to permit income distribution and employment simulations by adding on employment coefficients or distribution schemes (somewhat mechanistically) for each sector. Solving for final demand then gives sectoral outputs, employment and distribution of income. The link between consumption and distribution can be closed to give consistent income-consumption solutions.

Substitution and Endogenous Price Determination

Endogenous price determination is necessary if we are to study questions such as the importance of relative factor price distortions and various taxes and subsidies in determining employment and income distribution, the importance of terms of trade changes in affecting the urban-rural income distribution. It is also necessary if we are to examine the role of foreign trade possibilities in generating the kind of commodity demands needed to promote demand for labour in the labour markets.

The Adelman/Robinson [1974] model (which is the outcome of RPO 206) referred to above is an important example of endogenous price determination in which income distribution implications of static price and quantity interventions have been explored [dynamic simulations are currently in progress]. The model's preliminary conclusions are that such static intervention (aimed at changing the functional distribution of income while leaving asset concentration patterns unchanged) has a very limited impact on the overall patterns of inequality. They do, however, affect the composition of poverty, i.e. whether the poor are urban or rural.^{1/} We clearly need more experiments with different countries before we can be confident of this result, but its implications are very important. To quote Adelman [1975],

"...while one can make small gains in the welfare of the poor through large changes within the system the goal of equity cannot be achieved without radical reform".

^{1/} This insensitivity of overall inequality parallels the general conclusion of many Leontief type studies that overall employment patterns are insensitive to demand composition changes. All these conclusions of course are model and data specific.

Non Neo-Classical Models

While the endogenisation of prices is a major step towards incorporating the wisdom of neo-classical thinking into economy wide models, it is also important to experiment with models that depart from neo-classicism in a fundamental way. But are there such models? For all the criticisms of neo-classical theory that have been launched by the Cambridge School the fact remains that no viable alternative system has yet been produced. The most that we have seen are attempts to graft "structural" elements on to neo-classical systems by introducing specific rigidities and dualisms.^{1/} More recently, an attempt has been made by Bacha and Taylor [1974] (also an outgrowth of Bank-sponsored research) to develop a growth cum distribution model combining elements of neo-classicism with the more unorthodox neo-Keynesian distribution theory.

Further work along these lines is absolutely essential, if only because the set of policies that may emerge as relevant would be quite different. A real feel for the nature of the problem can only be obtained by continuously contrasting the alternative approaches available. Contrast, for example, the following quotation from Bacha and Taylor [1974] with the usual assumptions of the neo-classical school,

"...the skilled labour supply curve (depending on the stock of human capital, or of people with middle class parents, or whatever) is irrelevant to output determination ... labour supply considerations can be inter-

^{1/} These models should really be classed as neo-classical in basic inspiration. They differ from the stylised perfectly competitive neo-classical paradigm insofar as they incorporate market fragmentation and non-competitive (but still maximising) behaviour. This is, however, best described as an adjustment of the basic model to reflect particular real world features rather than a rejection of the paradigm. Indeed, our ability to do this is a tribute to the impressive flexibility of neo-classical tools.

preted as influencing the number of people "waiting" for middle class jobs, but the real driving forces in the economy are not to be found in the labour market".

This conclusion seems at least as plausible as the alternative (particularly to someone from South Asia!).

VI. MODELLING A PART OF THE ECONOMY

Sectoral models have for some time now been regarded as the wave of the future -- the model builder's answer to the disenchantment with economy wide models. These models obviously lend themselves to an analysis of some distributional problems as when agriculture models distinguish farms by farm size and solve for distribution of output between farms as well as employment on farms or simulate land reforms and changes in tenancy arrangements.^{1/} Similar models of the industrial sector focussing on the choice between scale of production, labour, intensity of technique, etc. could be equally important although the experience to date suggests that the specification of such models is considerably more difficult in industry than in agriculture. [For one thing, product homogeneity can be more safely assumed in agriculture than in industry.]

The great advantage of these models over economy wide models is that they permit quite substantial disaggregation so as to reflect the nature of production constraints more realistically. Policy makers are much more likely to take seriously a model which determines results while taking into account detailed information of the kind that is only possible in activity analysis. Specifications of aggregate production functions with analytical (almost metaphysical) concepts such as "elas-

ticities of substitution" are not nearly as plausible a basis for conceding the potential for employment creating policies as concrete results in terms of defined technologies, crop mixes and scale choices. Experiments with such models would provide an invaluable basis for comparison with the sectoral results achieved through the highly aggregated sector treatments that are achievable at the economy wide level. This is particularly important if we are to decide whether the frequently reported insensitivity of aggregate models is due to the level of aggregation or more fundamental "balancing forces" which prevent an economy being moved to an alternative time path.

VII. STUDYING ISOLATED RELATIONSHIPS

This is by definition an "open ended" category but it is not meant to be a ragbag in which we can include almost anything that could conceivably be relevant in constructing an economy wide model. Research in this area should be seen as a detailed pursuit of individual relationships that are particularly important for income distribution. Typically, this view of the importance of a relationship is derived from some broad theoretical formulation which postulates a skeletal or stylised form of a particular relationship leaving the rigorous investigation to further research. Examples of such "isolated relationships" are the following.

(a) Substitution in Demand

Further work on demand systems is extremely important from the point of view of income distribution (and also employment). We need to

^{1/} For a static linear programming model with these capabilities see Duloy and Norton (1973).

examine substitutability between commodities which are very different in terms of their employment and income generating characteristics. The theory of consumer demand is fairly well developed and some very important applications have already been completed in LDCs.^{1/}

The available results confirm the existence of substitution as evidenced by high and significant price elasticities and cross price elasticities. The Lluch-Powell-Williams findings point to the particular importance of the price of food both in terms of own price elasticity of Food demand and cross price elasticities of the demand for other commodities with respect to the price of food. These results can have important implications for income distribution especially in the context of urban rural terms of trade changes.

Unfortunately, the existing studies for LDCs are all at very high levels of aggregation. [In general much greater disaggregation has been used in the simpler demand functions which don't estimate price elasticities.] The Lluch-Powell-Williams studies for example distinguish between broad categories such as Food, Tobacco and Beverages, Clothing, Housing, Health and Education, Transportation, Recreation and Durables. Quite obviously we cannot really expect to capture the full scope of demand substitution affecting employment by operating with such broad categories. We need to distinguish between different types of clothing and different types of housing. In effect we should consider the scope for substitution between different commodities which in some sense satisfy the same want but which have very different pro-

^{1/} IBRD research is well represented in this by the various studies of Lluch, Powell and Williams (see papers listed in bibliography).

duction characteristics^{1/}. As far as I know such demand functions have not been estimated for underdeveloped countries although both theory and data to do this are now available. Further work along these lines should be strongly encouraged.

(b) Substitution Between Factors

Although an extensive literature exists on the production function and measures of substitutability, there is a growing feeling that there is little point in pushing for detailed econometric studies of this type. The scope for substitution in technology is best studied by detailed activity analysis. Substantial work along these lines is currently under way outside the Bank. [The Strathclyde group] and a major study has just been completed on Korea [Westphal and Rhee] as part of the Bank's research program. We should review these studies before launching further research in this area.

(c) Savings Rates by Income and Socio-Economic Groups

We need to know much more than we do about the rates of saving and patterns of asset accumulation of different income or socio-economic groups.

The importance of savings has been emphasised in quite different ways by different theories of distribution. The neo-classical approach takes savings to be a basis for asset accumulation. On this view differences in savings patterns projected dynamically lead to differences in patterns of asset concentration as for example in the theoretical model

^{1/} It is worth noting that the theory underlying the Linear Expenditure System implies that the commodities for which the system is estimated are characterised by "want independence", i.e. the utility derived from one is not affected by the level of consumption of any other. Substitution between such goods exists solely because of the budget constraint.

proposed by Stiglitz (1969) and the simulation model by Ahluwalia and Chenery (1974). The importance of saving on this account is further strengthened by the existence of fragmented imperfect capital markets. In such a world (a) the existence of own savings provides greater access to borrowed capital thus reinforcing the patterns described above and (b) the absence of access to finance may lead to a wiping out of accumulated **savings every time disaster strikes**. The likelihood of **small farmers falling into debt and ultimately losing their land is a well documented example** of just such a phenomenon. It is of course true that the wiping out of fortunes also occurs at higher income levels but it should be noted that this has the effect of transferring wealth within the upper income groups instead of transferring wealth from small farmers to large money lending landlords.

In contrast to the savings-asset accumulation-capital market nexus described above, we have other theories of distribution which have placed savings propensities at the core of functional distribution. The neo-Keynesian theory associated with Kaldor and Passinetti for example recently resuscitated by Bacha and Taylor regards savings propensities as determining the amount of income that needs to be directed at a particular group in order to establish the savings-investment equality. If this theory has any relevance to reality [and this is questioned by many critics] savings propensities are again important

(d) Migration

The migration phenomenon is widely regarded as crucial for modelling employment and income distribution in the medium or long run context. Migration is the process which determines the relative location

of the poorer populations^{1/} and as such is a crucial structural change phenomenon. Studies of the determinants of migration are needed if only to develop reasonable projections of the speed of such projected changes.

(e) Human Capital Formation: Role of Skilled Labour

In recent years increasing attention has been paid to the role of skills and other human resources in economic development. In principle this could be studied in two different ways. Firstly, we could attempt to quantify the importance of skilled labour in the context of the production process by detailed studies either of the production function variety or activity analysis. Secondly, we can simply look at the income streams of workers at different education levels with different degrees of experience.

As a rule work in this area has been of the latter sort. It has amply established the fact that workers with a higher level of schooling tend to have higher incomes, and that income rises with experience to a peak and then drops, as predicted by the human capital model. My own reaction to this line of enquiry is that although these estimated relationships are consistent with the human capital hypothesis they are too obviously also consistent with other hypotheses which focus on social structure as a determinant of incomes. This suggests three directions of research. Firstly, the education-age-income relationship should be examined together with these other possible hypotheses in order to distinguish between these competing hypotheses. Secondly, the relationship should be examined across countries to determine whether country characteristics

^{1/} It is best to think of migration as a relocation of the pressure of population rather than the poor as there is some evidence that migrants don't come from the poorer sections of rural communities nor do they end up in poorer sections of urban communities.

make a difference. Finally, and in some respects most important, the role of labour skills in generating higher output and determining higher wages should be examined in the context of production relationships. Precisely how this can be done is still an open issue.

VIII. HISTORICAL EXPERIENCE OF SOME COUNTRIES

Thus far, we have been discussing research which is distinguished by its use of a relatively formal methodology, i.e. specifying and estimating a mathematical model or a single equation. Parallel with such studies there are others which are much less "rigorous" but may well be equally productive. An obvious example is a study of the historical experience of distributional change in a particular country. Protagonists of this line of research argue that such a "heuristic" study of the nature and causes of distributional change with development can have very high pay off.

Detailed discussion and evaluation of this approach is made somewhat difficult by the absence of any finished studies of this type although some are currently in the pipeline.^{1/} The following general comments can however be made and could serve as a basis for evaluating research projects of this type.

- (i) The lack of precise methodology makes it impossible to determine in advance the standards of rigour that will be adopted in the analytical parts of the study. Indeed the whole merit of this approach lies in its open-endedness and the resulting scope for imaginative heuristics.

^{1/} These include studies of Taiwan, Philippines and Colombia financed by the IBRD Research Program and being conducted by the Yale Growth Center.

- (ii) At a minimum such a study should obviously document the "facts". For this it should be able to draw on data for several points in time on the overall degree of inequality and be able to relate these inequality patterns to economic phenomena about which generalisations are possible. For example, if inequality is related to the distribution between profits and wages and the inequality of each component then changes in inequality can be traced to changes in either of these components. Similarly if inequality is related to particular socio-economic groups observed changes in inequality can be "explained" by explaining changes in the relative income position of these groups. The groups identified in Section III above, viz. small farmers, landless labourers, unskilled workers, etc. would be very suitable for this purpose.
- (iii) Underlying the explanation of changes in particular components (whether wage income or income accruing to small farmers) is obviously an implicit or explicit theory of some kind. Attempts should be made to make this underlying theory as explicit as possible.

While accepting the potential productivity of this type of approach, I should also record that I am somewhat skeptical of this approach, especially for international research support of the type we are discussing at this conference. This is essentially because of item (i) above which in my view makes it difficult to subject research proposals of this type to method-

ological scrutiny or indeed to establish reasonable criteria which the results should live up to. This skepticism may be easier to understand if we consider what our position would be on financing a "heuristic" study of growth! Nor is this skepticism derived from any exclusive emphasis on formal methodologies as witnessed by my strong support for narrower policy oriented studies in Section IX below.

IX. STUDIES OF PARTICULAR POLICY INSTRUMENTS

Under this category I include research projects which make a detailed study of the impact of particular policy instruments in specific country situations. In my view studies of this type should have a very high priority if research is to be of value for policy making. It is also an area particularly well suited for international agency funding since the usefulness of studies in one country is greatly enhanced by comparison with similar studies in other countries. Furthermore, these are studies which are best done with direct involvement of institutions and agencies in LDCs and also these are studies which such institutions are most capable of doing especially if pilot studies can be developed to determine research design.

What distinguishes such studies from the study of policy interactions in a formal model framework is the richness of detail and the explicit recognition of the specific historical, social and institutional contexts. Thus it is one thing to "plug in" greater level of fertiliser availability at a subsidised price in a programming model and watch the model pick the new "optimum" and quite another to document what actually happens in different rural societies when attempts are

made to implement such a program. Similarly, we can "simulate" technical progress and its dissipation by "shifting the whole production function upwards at some predetermined rate" but it is quite a different matter to document some actual cases in which the diffusion of a technology is studied in a particular context specific constraints on the diffusion identified and comparisons made with the diffusion process in other similar cases. Studies of this type obviously complement studies of policy interactions in formal models. Formal models examine the impact of policies in a relatively stylised fashion emphasising the quantification of direct and indirect effects. Detailed studies give us an assessment of working level problems (which policy makers must examine) associated with the implementation of such policies. Some examples (by no means comprehensive) are given below.

(a) Rural Development

We need careful monitoring cum ex post analysis of the various types of "rural development integrated packages" and special schemes that are currently being promoted around the world. As far as I know, everyone agrees with this proposition in principle, and the only question is how best to implement the proposal. A major problem is that the research perspective involved must be fairly long term. We must first design suitable monitoring systems and then ensure that as the projects are implemented these monitoring systems are also made to work. There is therefore a substantial lag before the data collected pile up to provide sufficient raw material for analysis.

(b) Technology Diffusion

We need to do a good deal more than is currently being done on the dynamic process of technology diffusion as opposed to the static

perception of available alternatives. Even the systematic documentation of major successes and failures in the diffusion and adaptation of technology in various fields would be of great help. Obvious cases for study are improved seed diffusion, development of small irrigation technology, low cost housing, flexibility in design standards for roads and other construction, etc., low cost energy sources (bio-gas, windmills, solar energy, etc.). What are the factors that characterise successful diffusion and adaptation processes? What role have governments played in the success stories?

(c) Public Expenditures and Taxation

What is the scope for re-designing public expenditure and tax policy to shift the incidence of taxes away from and the incidence of benefits towards the lower income groups? Examination of this question in a particular country as a package would help in quantifying the scope for flexibility in this field while keeping in mind overall budget constraints. Included in the preview of such studies should be the quantitative significance (and distributional impact of underpricing of public services (which accrue mainly to middle and upper income groups)).

(d) Subsidised Provision of Food and Other Wage Goods

This is a familiar instrument of distributional policy in many countries and one that has very strong "political appeal". Attempts to subsidise wage goods are made in several different countries in quite different ways. What is the comparative experience in terms of operating one or other method? Are there countries that have been particularly successful in using this policy instrument?

(e) Credit Policy and Small Business

A good deal of the literature on employment and income distribution deals with the high costs of capital facing smaller firms (presumed to be better for income distribution objectives). Systematic studies of the degree of cost differentials facing different firms either by scale or by a "formal-informal sector" division could be very useful. Such studies could include an assessment of the effectiveness of various types of "specialised institutions" that are usually set up to direct credit to these groups.

(f) Land Reform and Tenancy

The implementation of land reform has long been emphasised as an essential requirement for broad based agricultural development and equitable growth in agricultural economies. Equally, it is true that the implementation (or non-implementation as the case may be) of different types of land reform have been fairly extensively studied. In view of this, it seems reasonable to accord it low priority in future research directions.

By contrast, problems of tenancy and sharecropping have been inadequately studied both at the theoretical and empirical level. Given the widespread nature of such arrangements, and the inevitability of having to live with them in planning rural development, more research should clearly be directed to these problems in the future.

The policy instruments discussed above are obviously only some of the instruments that can be studied in isolation but they suffice to give a flavour of what is involved in this category of research which is all I have undertaken to do.

VIII. SOME TENTATIVE CONCLUSIONS

Perhaps the most important conclusion to be derived from the above discussion is that as an area of research "income distribution" provides about as narrow a focus as "growth." Indeed we have argued that what is needed is theory of growth for an economy segmented into appropriate income groups. Because it is an all-encompassing subject, the research directions that emerge are necessarily far-flung. Choosing the more productive lines from this extensive menu is a difficult task and any attempt along these lines must necessarily be tentative. The following suggestions for developing a balanced research program in this area should be seen in this context.

- (i) There is little point in attempting to add to our considerable collection of available published data. The data already collected provides a valuable basis for cross section analysis of aggregative patterns of inequality and has been extensively used for this purpose both within the Bank and outside. The data and our own cross section analysis will be published shortly.
- (ii) A more fruitful line of research on the data side is the analysis of survey data to study the anatomy of income distribution, i.e. to identify the socio-economic characteristics of the poorer groups as a first step in studying how to improve their income levels. The ESCAP and ECLA projects are a useful start in this direction but we should recognise that being limited to existing surveys, the statistical picture they provide may be

somewhat crude. Pre-existing surveys are unlikely to contain the full range of information on labour force characteristics, employment and occupation status, migrant status, sector of employment, etc. that are now de rigueur in anatomical studies.

- (iii) For this reason it may be desirable to undertake in-depth country studies using national survey data for the "broad brush" income distribution picture but supplementing this with partial data (labour force surveys, agricultural and manufacturing censuses, etc.) to develop a more detailed picture of the socio-economic characteristics of low income groups. Such a phase is currently envisaged for the ECLA project. Equally important in **this context is the development of a time series picture.** of changes in income and relative position of particular groups. Overall changes in inequality may reflect a number of diverse changes in the relative position of different groups. Symmetrically an unchanging picture of overall inequality may hide substantial movement for particular groups. If we are to go beyond cross country analysis to consider whether development necessarily worsens relative inequality, it should perhaps be in terms of an analysis of what happens to each of these groups over time. Historical studies of this type could be extremely valuable in providing the raw material for theorising, model building, and model verification. This raises the question whether the work on income distribution, labour markets, and employment should be co-ordinated. At present there is no mechanism

for ensuring that they complement each other.

- (iv) The complexity of the inter-relationships involved makes it essential for a research program on income distribution to encompass economy wide modelling. In this context it is important to judge such models by what they add in terms of the determinants of income distribution to an otherwise familiar structure. The Bank research program has an extensive selection of such projects ranging from the simpler input-output models [Malaysia Employment and Distribution Simulation] to the more sophisticated price endogenising variety such as the Korea model (Adelman and Robinson) and the Brazil model (Taylor and Lopes). Further work along these lines is planned as part of the Prototype Model project. The next year should provide the basis for a fairly extensive review of experience in this field.
- (v) We need to undertake detailed studies of the effect of particular policy instruments on income distribution. This is an area in which we are simply not doing enough. The problem is not however organisational. We need to develop appropriate methodologies to study the impact of particular instruments outside an economy wide framework. Inadequate attention to this problem will simply lead to a flood of tedious and mediocre research pieces. Nor is it impossible to develop such partial equilibrium methodologies. The empirical literature on foreign trade distortions provides eloquent testimony to what can be done without economy wide modelling. Why can't we have something comparable in the labour market?

Finally a word of exculpation. I have deliberately avoided any reference to sociological or socio-political dimensions of a research program on income distribution.^{1/} This is not to deny the importance of such research. It is primarily a reflection of my own ignorance in these areas and secondarily perhaps a reflection of my suspicion that at present multi-disciplinary research in these areas is less important than the independent pursuit of the same questions in different disciplines.

^{1/} This omission was the subject of substantial criticism at an earlier meeting at which a version of this paper was presented.

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ANNEX III

ABSTRACTS OF EXTERNAL RESEARCH PROPOSALS ON INCOME DISTRIBUTION
CONDUCTED IN THE DEVELOPMENT RESEARCH CENTER*

*These abstracts are taken from the Bank's Research Abstracts booklet dated October 1976. Completion dates on many of the projects have been revised since then to reflect slippage.

Ability Characteristics as Factors of Production

Ref. No. 671-33

This study will provide a better understanding of the mechanisms by which education affects labor incomes. It will test the "education as a labor market rationing device" hypothesis versus the "human capital" hypothesis.

The first stage of this project identifies those individual ability characteristics that appear statistically significant in explaining variations in labor incomes. In the second stage, the factors determining the levels of such abilities are analyzed. The separate influence of schooling and non-schooling variables on abilities will be identified. To the extent that particular aspects of school curricula affect relevant abilities, the above framework could provide an economic criterion for educational reform.

The basic data for the research is an ongoing study on "Labor Force Participation, Income, and Unemployment" of the Bombay (India) labor market (see Ref. No. 670-45, page 74). It is proposed to reinterview members of the labor force included in the sample of the Bombay labor market study to obtain data on individual scores in ability tests.

Responsibility: Development Research Center—Marcelo Selowsky, with the collaboration of Dipak Mazumdar of the Development Economics Department, and the University of Bombay.

Completion date: September 1977.

Price Intervention in Agriculture

Ref. No. 671-39

Price intervention in the agriculture sector is a common instrument of government policy in developing countries. Governments intervene in the determination of the output price as well as the price of agricultural inputs to affect the distribution of wealth in favor of either consumers, by forcing lower prices, or low-income producers, by granting subsidies. However, it is not always clear how government price intervention should be quantified in order to estimate its net distributive impact. Significant "second round" effects of such policies can be fully captured only in the context of an explicit model of intersectoral linkages in the economy.

This project will develop a partial equilibrium methodology for quantifying the different effects of a package of agricultural price interventions. The methodology will make a distinction between production effects in terms of impact on output and choice of technique, and consumption effects in terms of prices facing different consumers. These effects will be differentiated according to the income classes affected and will be appropriately weighted to obtain the net distributional impact. An important objective of the project is to compare this partial equilibrium framework with a general equilibrium approach to the same problem.

Responsibility: Development Research Center—Montek S. Ahluwalia. The research is being conducted by Professor Trent Bertrand of the Johns Hopkins University.

Completion date: January 1977.

Analytics of Change in Rural Communities

Ref. No. 671-17

The World Bank has significantly increased its lending program for rural development projects. These projects provide a direct and quick way of raising the standard of living of the mass of rural poor (both in terms of personal incomes and access to basic services). However, if continued rounds of improvements to the standard of life in rural areas are to be realized on a continuing basis, then rural development projects must contribute toward the initiation of a self-sustaining process of change. The design of projects to achieve self-sustaining change in rural areas raises important questions about the role of agriculture and, more generally, of rural areas in national economic growth. It also demands a more comprehensive understanding of the economic structure of rural areas than that which currently exists, and requires the development of suitable planning and appraisal techniques to take account of the regional focus of rural projects.

In an effort toward helping solve these problems, this research project involves a detailed case study of the Muda River Irrigation Project in Malaysia, utilizing the extensive field surveys conducted by the Cooperative Program of the World Bank and the Food and Agriculture Organization (FAO). The study has two main components:

1. Formal models of household and regional economic change are developed to clarify the structure of the local economy. These models encompass household behavior with respect to production, consumption, and investment decisions; the functioning of local factor markets for land, labor, and capital; and the interactions in the regional economy between agriculture and other economic activities, and among output, the distribution of incomes, and final demands. They make it possible to identify the points of growth and leakage in the regional economy, and provide a rigorous framework for exploring regional development strategies with maximum growth impact.
2. By linking the regional model to a national macroeconomic model, some of the important questions concerning the tradeoffs between national economic growth and regional rural development strategies will be explored. This type of analysis should also shed light on feasible time paths and strategies for alleviating rural poverty within the constraints imposed by national income generation and population growth.

Responsibility: Development Research Center—Clive L.G. Bell and Peter B.R. Hazell.

Completion date: June 1978.

Indirect Estimation of the Size Distribution of Income

Ref. No. 671-41

A major problem in empirical work on income distribution is the lack of reliable estimates of the size distribution of income. The traditional approach to estimating this distribution is the use of large-scale income surveys, which are typically very expensive since they must try to cover the whole country.

This project attempts to develop a more concise methodology for estimating the distribution of income among various socioeconomic groups at different income levels. The basic approach is to estimate the implicit distribution of income given the total levels of consumption of different commodities and the consumption parameters of different groups. The distribution of income or consumption among groups can then be estimated given (1) information on the consumer budget of each group in terms of consumption shares of various commodities, and (2) the total consumption of these commodities. Consumption shares for the groups are obtained through small, relatively inexpensive surveys and totals of consumption are generally available from national accounts.

Responsibility: Development Research Center—Montek S. Ahluwalia.
The research is being conducted by Professor Richard Eckaus of the Massachusetts Institute of Technology.

Completion date: March 1977.

Income Distribution in Thailand

Ref. No. 671-36

Devising policies and programs that contribute to the elimination of poverty requires a knowledge of the forces affecting the distribution of household income within a country. This study seeks to identify these forces by applying techniques of statistical inference and economic analysis to a nationwide survey in the early 1970s of some 14,000 households in Thailand. Specifically, it aims to verify whether income distribution in Thailand conforms with three important economic postulates:

1. The earning power of individuals is determined by the human capital embodied in them.
2. Labor force participation rates are affected by earnings incentives and opportunity costs.
3. The relationship between a household's income and its level of living varies over the life-cycle in a predictable way.

These hypotheses are modeled at the microeconomic level. The human capital earnings function is extended to incorporate the behavior of two important groups of non-wage-earning labor force participants: the self-employed and unpaid family workers. This function, estimated separately for men and women, can be used to construct "potential" household age-income profiles on the basis of the characteristics of household members. The earning power of individuals and of their households is then used to explain the relationship between labor force participation and household income. The entire model is viewed as a three-equation system which explains the distribution of household income as the joint effect of earnings and labor force participation.

From preliminary results, low-income households appear to have a disproportionate number of people who do not have much formal education and have few working adult members.

Responsibility: Development Research Center—Carmel Ullman Chiswick.

Completion date: December 1977.

Social Accounts and Development Models

Ref. No. 671-27

Macroeconomic analysis of development has been restricted mainly to two-gap models of resource needs and input-output models of industrial structure. This is an inadequate framework for studying policies concerned with problems of employment generation, income redistribution, and eradication of poverty. The project will attempt to provide the basis for a new generation of macroeconomic models that will permit analysis of the tradeoffs between alternative policy goals such as growth and redistribution by (1) reviewing the data requirements of such models, and (2) specifying the type of model required. Hence, the research is concerned simultaneously with the determinants of inequality and its measurement; the definition, quantification, and causes of poverty; and the simultaneous determination of both prices and quantities in a model framework.

The study identifies the data needs on the basis of the UN system of standardized national accounts, which presents annual data in a social accounting matrix (SAM) framework. Since the UN system imposes a heavy burden on the statistical offices of developing countries, the research starts with specific country studies on the feasibility of obtaining basic data in modified versions of the UN format. At the same time, some extensions of the standardized UN system are required, since it does not encompass questions of income distribution, employment, basic needs, and poverty, but rather treats them separately. Also, systems of classification (e.g., the level of disaggregation) and the reconciliation of data from various sources are studied, to make the system more relevant for planning.

The development of data requirements is paralleled by research on model structures within the social accounting matrix framework. The model being developed in this project generalizes the linear (input-output) models by allowing for price substitution. By utilizing CES functions extensively, the basic model provides a general and flexible framework that includes a wide range of existing models as special cases. A major feature is the specification of two-way causal links between income distribution and the structure of production, both of which are endogenous.

The modeling phase of the project will be followed by work on the relative importance of alternative policies and the specification of relationships of particular objectives. As an extension of the initial research goals, the overall model and its data framework may be simplified for use in practical applications in a variety of countries.

Responsibility: Development Research Center—F. Graham Pyatt and Montek S. Ahluwalia, with the assistance of Alan Roe, Jeffrey Round, and Suresh D. Tendulkar (consultants). Messrs. Round and Roe have been associated with work in Sri Lanka and Swaziland on earlier projects financed by the World Employment Programme, International Labour Office (ILO), Geneva, and the Overseas Development Administration, London. Messrs. Round and Tendulkar have been similarly associated with work in Malaysia.

Completion date: December 1977.

Evaluation of Asian Data on Income Distribution

Ref. No. 671-08

This project was designed to parallel the research project on the "Evaluation of Latin American Data on Income Distribution" (see Ref. No. 670-83, page 8), and is part of the work program aimed at improving the data base in this area. The basic research objective is to undertake an analysis of patterns of income distribution for each country in the Asia region.

After an extensive effort at identifying suitable data sets, access was obtained to income and expenditure surveys in nine countries—Republic of China (Taiwan), Hong Kong, India (for the States of Gujarat and Maharashtra only), Malaysia, Nepal, Philippines, Singapore, Sri Lanka, and Thailand. These surveys will be the basis for an analysis of the patterns of income distribution and the nature of poverty. The study will provide profiles of the socioeconomic characteristics of different income groups that are broadly comparable across countries. Particular attention will be paid to the nature of rural-urban differences in income distribution and the relationship between income and various labor force characteristics. The project will also provide an assessment of the quality and comparability of available data.

Responsibility: Development Research Center—Montek S. Ahluwalia. This project is being undertaken jointly by the World Bank's Development Research Center and the Economic and Social Commission for Asia and the Pacific (ESCAP). The principal researcher is Pravin Visaria.

Completion date: December 1977.

Distributive Impact of Public Expenditures

Ref. No. 670-96

It is increasingly apparent that the benefits of economic development have been unevenly distributed. Public expenditure, which represents from 10 percent to 25 percent of national income in developing countries, has a substantial impact on redistribution, whether or not it is consciously used for that purpose. Yet, information on the distributive effects of public expenditures is meager. Investigation of such effects has been restricted to analyses using statistics compiled for other purposes. Two major defects of such studies have been the frequent need for restrictive assumptions and the highly aggregated nature of the results.

The general objective of the project is to assess the impact of public expenditures on the distribution of income in Colombia and Malaysia. Specifically, the study first identified major public expenditures whose benefits can be clearly traced to individual households; described the beneficiaries of public services by income distribution, ethnic group, geographical location, and other factors; and explored the factors determining household demand for certain government services.

After the relevant public programs were identified, estimates of unit costs were developed. Sample surveys were then conducted to study how services are distributed, and to explore demand relationships for the various public outputs by socioeconomic characteristics of the households sampled. Combining the cost and survey data permitted estimates of government spending per household.

Responsibility: Development Economics Department and Development Research Center—Jacob Meerman and Marcelo Selowsky. The research on Malaysia is being undertaken in collaboration with Professor Lai ah Hoon (University of Malaysia), Peter Heller (University of Michigan), and the Eastern Market Assessment Survey Company. The research in Colombia is being undertaken in collaboration with the Colombian Data Company.

Completion date: December 1976.

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Employment and Income Distribution in Malaysia

Ref. No. 670-94

This project studies the related problems of income distribution and employment in Malaysia. It consists of two components: a study of patterns of income distribution in Malaysia in 1970, and the construction of an input-output type of planning model which attempts to project the employment implications of growth.

The study of income distribution is based on the 1970 Post Enumeration Survey conducted by the Department of Statistics of the Government of Malaysia. It examines the contribution of such factors as rural-urban location, sector of employment, employment status, occupation, education, and race to the overall degree and pattern of income inequality. The survey data have also been used to identify the socioeconomic characteristics of different poverty groups.

Work on the input-output planning model was guided largely by the objective of providing a quantitative framework for the analysis of employment prospects over the Third Malaysia Plan (1976-80), and the project was implemented in close collaboration with the Economic Planning Unit in Malaysia. The model is of the familiar comparative statics type which generates detailed sectoral output and employment projections for growth rates in final demands as determined from a macro forecasting model. It is used to examine feasible rates of "employment-restructuring," i.e., progressively relocating the relatively impoverished, dominantly Malay labor force from agriculture to the nontraditional economic sectors. It is also used to estimate investment requirements of projected output extension. The data base developed in the course of this project is being expanded in order to construct a more advanced planning model for Malaysia which will generate the distribution of income among socioeconomic groups (see Ref. No. 671-27, page 15).

Responsibility: Development Research Center—Montek S. Ahluwalia. The income distribution study was undertaken by S. Anand (formerly on the staff of the Development Research Center and currently Fellow of St. Catherine's College, Oxford). The work on the planning model was undertaken by Mr. Ahluwalia and Suresh D. Tendulkar.

Completion date: February 1977.

Urban Income Distribution in Latin America

Ref. No. 670-85

The broad objectives of this project are similar to those of the other projects in the World Bank's program of data-oriented research on income distribution (see Ref. No. 670-83, page 8, and Ref. No. 671-08, page 13): the study of patterns of urban income distribution with special emphasis on problems of identifying poverty groups.

Three household income-expenditure surveys (for Bogotá and Medellín, Colombia, and Lima, Peru) have been selected for analysis from data for 18 Latin American cities collected between 1966 and 1972 by member institutes of the Estudios Conjuntos sobre Integración Económica Latinoamericana (ECIEL). This project will document the major features of the distribution of income in these cities, including the relation between inequality and various socioeconomic characteristics of individuals and families. The project attempts to develop appropriate criteria for identifying poverty groups and compares the empirical results obtained when alternative criteria, such as income, consumption, and estimated permanent income, are used. The success of alternative or second-best criteria in identifying poverty groups originally defined in terms of some first-best criterion will also be examined.

Responsibility: Development Research Center—Montek S. Ahluwalia. The research is being conducted by Philip Musgrove of the Brookings Institution and Robert Ferber of the University of Illinois, in collaboration with the participating institutes of ECIEL.

Completion date: March 1977.

Growth, Employment, and Size Distribution of Income

Ref. No. 670-84

Extensive study of national data has given rise to the hypothesis that the size distribution of income appears to worsen as development proceeds. However, evidence from specific countries, such as the Republic of China (Taiwan), leads to some questioning of this hypothesis. Even if a negative historical relationship exists between growth rates of Gross National Product (GNP) and trends in the distribution of income, the issue still remains whether this relationship is inevitable or whether an effective development policy can reduce the conflict between growth and size distribution of income.

This study analyzes the relationship between economic growth, governmental policies and income distribution in Taiwan. The underlying assumption is that in a mixed economy, the long-run trend in the distribution of income is determined by forces reflecting factor endowments, production conditions, and technology, which are modified by government intervention. The study attempts to isolate the factors responsible for the observed changes in income inequality in Taiwan from 1964-72. The technique adopted for this purpose is a decomposition of the Gini coefficient into the contribution of factor income shares and the degree of concentration of factor incomes. Changes in the Gini coefficient are, therefore, attributable to changes in these components. The impact of government policy on the different sectors of the economy is examined in order to draw generalizations about the relationship between government policy and the processes of growth and distribution.

Responsibility: Development Research Center—Montek S. Ahluwalia. The researchers are John C.H. Fei, Gustav Ranis, and Gary S. Fields of the Economic Growth Center, Yale University, in collaboration with Kuo Wan-Yong (Shirley) of the Economic Planning Council in Taiwan.

Completion date: March 1977.

Reports

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Evaluation of Latin American Data on Income Distribution

Ref. No. 670-83

A major limitation of empirical work on income distribution and the relationship between inequality, poverty, and development is the poor quality of available information. This project is part of a broad program of work aimed at improving the data base for the systematic study of distributional problems (see also Ref. No. 670-85, page 10, and Ref. No. 671-08, page 13).

The objectives of this project are to identify, for each country in Latin America, a recent data set suitable for the analysis of patterns of income distribution and to use these data to present a detailed picture of the different aspects of income distribution. Initially, 31 household surveys in 13 countries were identified as suitable for analysis. Access was obtained to 22 surveys for 11 countries (Argentina, Brazil, Chile, Colombia, Costa Rica, Ecuador, Honduras, Panama, Peru, Uruguay, and Venezuela). Multiple tabulations of the data have been prepared, accompanied by statements of their basic characteristics. The final report of the project will present a detailed picture of different aspects of income distribution, including the overall degree of inequality as conventionally measured by Lorenz curves or various indices of inequality (e.g., the Gini coefficient, the Theil index, etc.), and the socioeconomic characteristics of different income groups. An attempt will then be made to examine the validity of various hypotheses about causes of income inequality by examining correlates of inequality in selected surveys.

The final report will also provide an assessment of the quality of available data in Latin America as well as an evaluation of the degree of comparability in income distribution data across countries.

Responsibility: Development Research Center—Montek S. Ahluwalia. The project is being undertaken jointly by the World Bank's Development Research Center and the Economic Commission for Latin America (ECLA). The principal researcher is Oscar Altimir (formerly a World Bank consultant and now Chief of the ECLA Statistics Division).

Completion date: June 1977.

Short-Run and Long-Run Influences upon Income Distribution

Ref. No. 670-06

This project examines the factors affecting the size distribution of income in the context of an economy-wide general equilibrium model. The model is used to examine the impact of alternative development strategies upon the size distribution of income in the economy as a whole as well as the income levels of different socioeconomic groups.

A multisector, dynamic general equilibrium model has been constructed and calibrated with data from the Republic of Korea. The model specifies market-clearing behavior in both output and factor markets. The factor market equilibrium generates factor incomes that are paid to different socioeconomic groups. The incomes thus generated are fed into the consumer demand system so that a simultaneous solution is achieved for prices and quantities of outputs and factors. Factor endowments are allowed to change over time, with investment being determined by entrepreneurial expectations. The model attempts to capture a number of key characteristics of developing countries. Production in each sector is distinguished according to three different sizes of firms with different production functions and different degrees of access to credit.

An important result of this study is that the size distribution of income, and its basic path over time, are highly stable and difficult to change by policy intervention. By contrast, the relative position of various socioeconomic groups is very sensitive to economic policy choices. Thus, while the relative degrees of poverty and wealth as a whole appear difficult to change, the composition of the poor and the rich can be altered substantially. The study argues that, because of interactive effects within the economy, the most effective policy packages to alleviate poverty involve balanced, multitargeted, broadly based programs. Only when a sufficient number of different interventions are applied simultaneously does, in effect, a change in development occur.

Given preconditions concerning the initial distribution of land, physical capital, and human capital, the study identifies development strategies that appear to alleviate poverty and improve distribution, provided land tenure arrangements are not too inequitable and policy actions are undertaken to maintain the agricultural terms of trade. The choice between an import-substitution strategy, which skews the distribution and impoverishes the rural sector, and an export-oriented development strategy based on labor-intensive exports, has a major and lasting effect on reducing poverty. Further simulation exercises with the model reveal that broad policy packages (termed "market socialist" and "reform capitalist" in the study and including nationalization and large social development programs) are also effective. Piecemeal antipoverty schemes proved ineffective or even detrimental.

The research phase of this project is almost complete. A final report has been prepared and will be published shortly. Some additional experiments with the model are currently under way.

Responsibility: Development Research Center—Montek S. Ahluwalia.
The principal researchers are Professors Irma Adelman (University of Maryland) and Sherman Robinson (Princeton University).

Completion date: December 1976.

Reports

Adelman, Irma, and Robinson, Sherman. *Income Distribution Policy in Developing Countries*. Stanford University Press (forthcoming).

LIST OF DEVELOPMENT RESEARCH CENTER RESEARCH OUTPUT
ON INCOME DISTRIBUTION SUBMITTED FOR RAPIDE

This list of research output is grouped according to the major areas for research demarcated in the research strategy followed. The list includes papers at very different stages of completion. In general only those papers that are at the stage of drafts for outside circulation have been included. However, in some cases, papers that are at a preliminary first draft stage have been included to provide a flavour of ongoing research. Items underlined are of book or monograph length.

1. GENERAL OVERVIEW

- (1) H.B. Chenery, et. al., Redistribution with Growth: An Approach to Policy, Oxford University Press, 1974.
- (2) T.N. Srinivasan, "Development Policies and Levels of Living of the Poor: Some Issues", Summary Report on Bellagio Workshop on Analysis of Distributional Issues in Development Planning, May 1977 (mimeo).

2. EMPIRICALLY ORIENTED RESEARCH

(a) Cross Country Studies of Income Distribution

- (1) S. Jain, Size Distribution of Income: Compilation of Data, Johns Hopkins University Press, 1975.
- (2) M.S. Ahluwalia, "Inequality, Poverty and Development", Journal of Development Economics, 3, 1976, World Bank Reprint Series 36.
- (3) O. Altimir and S. Pinera, "Decomposition Analysis of the Inequality of Earnings in Latin American Countries", August 1977 (mimeo).
- (4) O. Altimir, "Income Distribution Estimates from Household Surveys and Population Censuses in Latin America: An Assessment of Reliability", November 1976 (mimeo).
- (5) P. Musgrove and R. Ferber, Finding the Poor. On the Identification of Poverty Households in Urban Latin America: A Study of Bogota, Medellin and Lima, August 1976 (mimeo).

(b) Country Specific Studies

- (1) J.C.H. Fei, G. Ranis, S.W.Y. Kuo, Equity with Growth: The Taiwan Case, 1977 (mimeo).
- (2) M. Selowsky, The Distribution of Public Services Across Income Groups: A Case Study of Colombia, May 1977 (mimeo)

- (3) S. Anand, The Size Distribution of Income in Malaysia, Nov. 1977, (mimeo)
- (4) M.S. Ahluwalia, "Rural Poverty and Agricultural Growth in India", June 1977.
- (5) M.S. Ahluwalia, F. Lysy, G. Pyatt, J. Round, J. Nolan, and Tendulkar, "A Social Accounting Matrix for Malaysia", November 1976, (mimeo).

(c) Problems of Measurement

- (1) N. Kakwani, Income Distribution Methods and Applications, February 1977 (mimeo).
- (2) R. Eckaus, "Report on an Indirect Approach to Measuring the Size Distribution of Income", March 1977 (mimeo).
- (3) 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).
- (4) G. Pyatt, "Distribution of Income and Wealth: On International Comparisons of Inequality", American Economic Assn., February 1977, Vol. 67, No. 1.
- (5) G. Pyatt, "On the Interpretation and Disaggregation of Gini Coefficients", The Economic Journal 86 (June 1976), World Bank Reprint Series No. 38.
- (6) C. U. Chiswick, "On Estimating Earnings Functions for LDCs", Journal of Development Economics 3 (1976), World Bank Reprint No. 44.

3. QUANTITATIVE MODELLING OF DISTRIBUTIONAL PROCESSES

(a) Economy Wide Models

- (1) 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 Run, Stanford University Press, November 1977 (joint publication Stanford University and Oxford University, U.K.).
- (2) E. Bacha, L. Taylor, F. Lysy, Models of Growth and Distribution in Brazil, 1977, (mimeo).
- (3) M.S. Ahluwalia and S. Tendulkar, "Annex V. Input/Output Analysis of Growth 1970-1980", 1976, (mimeo).
- (4) M.S. Ahluwalia, F. Lysy and G. Pyatt, "A Price Endogenous Model of Malaysia: Some Static Experiments", May 1977, (very preliminary draft) (mimeo).

(b) Multi-sector Regional Models of Distributional Processes

- (1) C. Bell, S. Devarajan, P. Hazell, R. Slade, "A Social Accounts Analysis of the Structure of the Muda Regional Economy", November 1976, (mimeo).
- (2) C. Bell, P. Hazell and R. Slade, "Autonomous Growth and Project Impact in the Muda Regional Economy: 1967-1972", September 1977 (mimeo).

(c) Aggregative Models of Distributional Processes

- (1) L. Taylor and F. J. Lysy, "Vanishing Short-Run Income Redistributions: Keynesian Clues about Model Surprises", Bellagio Workshop on Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).
- (2) C. Bell, "A Simple Dualistic Economy in a Comparative Statics Setting", Bellagio Workshop on the Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).
- (3) C. Lluch, "A Model of Employment and Income Distribution", Bellagio Workshop on the Analysis of Distributional Issues in Development Planning, April 1977, (mimeo), revised July 1977.

4. OTHER AREAS OF ONGOING AND FUTURE RESEARCH

(a) Food Nutrition and Other Human Capital Aspects

- (1) S. Reutlinger and M. Selowsky, Malnutrition and Poverty: Magnitude and Policy Options, World Bank Staff Occasional Paper No. 23, 1976.
- (2) S. Reutlinger and M. Selowsky, "Policies to Increase Calorie Consumption in Children: Cost Effectiveness Comparisons", Notes for the Bellagio Workshop on the Economics of Nutrition Oriented Food Policies and Programs, August 25-28, 1977, (mimeo).
- (3) M. Selowsky, "A Note on Preschool-Age Investment in Human Capital in Developing Countries", Economic Development and Cultural Change 24 (July 1976), World Bank Reprint Series No. 32.
- (4) S. Pinera and M. Selowsky, "The Economic Cost of the 'Internal' Brain Drain: Its Magnitude in Developing Countries", World Bank Staff Working Paper No. 243, September 1976.

(b) Labour Markets

- (1) M. Selowsky and S. Pinera, "Unemployment, Labor Market Segmentation, the Opportunity Cost of Labor and the Social Returns to Education", World Bank Staff Working Paper No. 233, June 1976, (forthcoming in the Quarterly Journal of Economics).
- (2) R. McCabe, "Education, Administered Wage Rates and the Size Distribution of Income in Urban Zaire", November 1976, (mimeo).
- (3) N. Stern, "On Labour Markets in Less Developed Countries", March 1977, (mimeo).
- (4) G. Pyatt, "Labor Markets and the Efficiency of Labor", Bellagio Workshop on Analysis of Distributional Issues in Development Planning, April 1977, (mimeo).

(c) Sharecropping

- (1) C. Bell and P. Zusman, "A Bargaining Theoretic Approach to Cropsharing Contracts", World Bank Reprint Series No. 45, (American Economic Review 66 (September 1976)).
- (2) C. Bell, "Some Tests of Alternative Theories of Sharecropping Using Evidence from Northeast India", December 1976, (mimeo).
- (3) P. Bardhan, "Variations in the Extent and Forms of Agricultural Tenancy: An Analysis of Indian Data Across Regions and Over Time", December 1976, (mimeo).
- (4) C. Bell and P. Zusman, "Sharecropping Equilibria with Diverse Tenants", June 1977, (mimeo), (forthcoming in the Economie Appliquee).
- (5) C. Bell, "Production Conditions, Innovation and the Choice of Lease in Agriculture", (forthcoming in Sankhya: The Indian Journal of Statistics, 1976, Vol. 28, Series C, Pt. 4).

(d) Price Policy in Agriculture

- (1) J. Mellor, "Agricultural Price Policy and Income Distribution in Low Income Nations", World Bank Staff Working Paper No. 214, September 1975.
- (2) T. Bertrand, "Market Interferences and Income Distribution: A Methodology for Studying the Agricultural Sector in Less Developed Economies", August 1977, (mimeo).