

Who Benefits from Economic Development?: Reply

By GARY S. FIELDS*

Before the appearance of my 1977 paper in this *Review*, it was widely thought that the income distribution worsened during the economic growth which took place in Brazil during the 1960's. My paper demonstrated that the familiar data, when analyzed from an absolute perspective, could show that the poor *had* benefited from growth. I found that the entire income distribution shifted, benefiting every income class; that the proportion of the economically active population with incomes below the poverty level (as defined by Brazilian standards) declined during the decade; that those who remained poor were less poor than before in absolute terms; and that the rate of growth of income among the poor was at least as great as the rate of growth among the nonpoor. These results came as a surprise to me, and so I did not expect that my conclusion—that Brazil seemed to do better on the income distribution front than many observers had originally thought—would be received uncritically by others.

The preceding comments paint a less rosy picture. Previously used data are shown to be deficient in important respects and new evidence is offered which contradicts the old. Because of this new and more critical evidence, I myself am less certain of what actually happened. But as I shall show, this latest reexamination also confirms some of the more positive aspects of the Brazilian experience. It is fair to say that neither the most favorable nor the most unfavorable position can be sustained unambiguously.

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I

Montek Ahluwalia, John Duloy, Graham Pyatt, and T. N. Srinivasan (hereafter A-D-P-S) criticize my analysis of changing income distribution in Brazil on three counts: limitations of the data used in my study; illogic of an approximation procedure; and qualitatively dissimilar results under seemingly plausible assumptions. Let me deal with these in turn.

We would do well to remember that I used the data from Albert Fishlow's 1972 paper with no adjustment save an interpolation. I used Fishlow's data without modification for methodological reasons—I wanted to show that qualitatively different results would emerge from a different *kind* of analysis (absolute vs. relative). Fishlow's data did not permit the analysis of families nor the use of income group-specific price deflators, both of which A-D-P-S criticize in my work. Nevertheless, some at the World Bank, among them Ahluwalia (1974), have in the past regarded Fishlow's data as reliable enough to cite conclusions derived therefrom and to use the data in their own research.

Ahluwalia et al.'s second point concerns my interpolation assumptions. I assumed that all those in the income class NCr\$0–2.8 had the same income, that 75 percent of them (=2.1, the poverty line +2.8, the range of the income bracket) were "poor," and that the remaining 25 percent of them were not. Under these assumptions, I estimated the proportion poor as 35.5 percent of the population in 1970. Ahluwalia et al. regard these assumptions as illogical, and in at least one way they are right: if the poor are defined as those with incomes below NCr\$2.1, the other 25 percent in the income group 0–2.8 should properly have been considered poor as well, since by assumption,

their incomes were below the poverty line also. By referring to the poorest 35.5 percent as the poor, a misstatement arose. In footnote 6 of my 1977 paper, as quoted in A-D-P-S's introductory paragraph, I stated that the average income of the poor must have risen at an above average rate; that statement should have been expressed conditional on 35.5 percent being the proportion poor. Ahluwalia et al. accurately noted my imprecision. One implication of their critique is that the number 35.5 percent merits less weight than I gave it in the 1977 paper. This point has a bearing on the interpretation of other of their results, discussed below.

Ahluwalia et al. are quite correct on their third point: qualitatively dissimilar results *are* possible based on Fishlow's 1972 data. They have ingeniously displayed the various possibilities for the proportion poor (i.e., incomes less than NCr\$2.1) and their income share. I would prefer to state their conclusions in the following way:

1) It is logically possible, though not proven, that: There was a greater percentage poor in Brazil in 1970 than in 1960.

2) It is logically possible, though not proven, that: The average income of the poorest 35.5 percent grew at a lower rate than the incomes of the other 64.5 percent between 1960 and 1970.

3) Statements 1) and 2) cannot both be true, that is: It is true either that the percentage poor fell or the average income of the poorest 35.5 percent grew faster than the average for the nonpoor or both.

4) With certainty, we can say from Fishlow's original data that: The average income of the poorest 37.0 percent (the proportion in the two lowest income classes in 1960) grew at an above average rate.

What are we to make of these various findings? Following the logic of A-D-P-S's argument, the figure 35.5 percent poor (my estimate for 1970) should be accorded no particular significance, since it was derived under assumptions which A-D-P-S regard as inappropriate. We have all accepted 37.0 percent as the proportion poor in 1960, though even that figure is somewhat arbitrary. Suppose that 37.0 percent is used as a

reference figure for 1970. How did the incomes of the poorest 37.0 percent change over the decade? From Fishlow's 1972 data, I can only conclude that they benefited from growth at an above average rate, as the following paragraph shows.

How much above average was the growth rate of income of the poorest 37.0 percent? The answer depends on the particular assumption made concerning the shape of the income distribution within the NCr\$0–2.8 income class. Various possibilities are illustrated in my 1976 paper. There, I showed that the poorest 37.0 percent of the population received *at least* 5.4 percent of the income in 1970, which is the same as A-D-P-S's lower limit (equation (2') evaluated at $P=37.0$ percent).¹ Since the poorest 37.0 percent received a smaller share, 5.2 percent, in 1960, their share *rose* even assuming the *minimum* possible increase. And this minimum increase is based on perfect *inequality* of income distribution among those earning less than NCr\$2.8, hardly a plausible assumption.² The more equal the distribution within the 0–2.8 group, the *greater* the growth in income share of the poorest. Hence, under any consistent assumptions the average income of the poorest 37.0 percent grew at an above average rate.

In short, my main empirical conclusion—that the poor in Brazil experienced percentage income gains at least as great as those of the nonpoor—holds up to the A-D-P-S critique, if in both years we define the reference group of the poor as 37.0 percent (which is the unquestioned figure based on the 1960 census) and if we accept Fishlow's original data for both years. However, as A-D-P-S rightly point out, if we seek to interpolate the number poor and their average income, a wide range of possibilities is consistent with the published data. It is not *certain* from these data that a

¹I report 5.49 percent in my 1976 paper. Ahluwalia et al.'s constraint implies a value of 5.41 percent. The difference is due to rounding.

²Perfect inequality is where some in that income class earn NCr\$2.8 and the rest earn zero, the number of each being determined by the amount of total income which that group receives.

smaller proportion of the population fell below a constant real absolute poverty line and that the average income among those remaining poor increased, though this *may* have been the case. Less favorable outcomes are also consistent with the available data. But it remains *impossible*, given our acceptance of the original data, that everything went wrong in the sense of both a growing proportion in poverty and a decline in their average income relative to that of the rest of the population. Of course, if the original data are regarded as unsuitable for poverty analysis, anything is possible, as the other commentators seek to demonstrate.

II

Paul Beckerman and Donald Coes (hereafter B-C) have recalculated the absolute poverty estimates in my paper by replacing the implicit *GDP* price deflator which I used by the price index for São Paulo. The two cost-of-living indices are 35.32 (implicit deflator) and 38.26 (São Paulo index). The difference between these numbers does not seem great, nor does the difference between the annual rates: 42.8 and 44.0 percent. Yet, by using the São Paulo index, estimated absolute poverty in 1970 is much greater and the participation of the poor in economic growth is correspondingly reduced.

I differ with the authors' dismissal of the absolute poverty approach to income distribution change, specifically their claim that "... the method itself cannot provide meaningful quantitative statements about changes in income earned by different groups, due to its sensitivity to small changes in the measured rate of real income growth" (p. 249). They reach this conclusion based on the judgment that the differences in price index are "small."

In fact, the differences are large. Mean income went from NCr\$5.52 in 1960 to NCr\$258.1 in 1970, both expressed in current cruzeiros. With the price index I used, mean income in constant cruzeiros comes to 7.31. With the index they suggest, the mean is 6.74. By my estimates, mean income increased by 32 percent over the decade. By their estimates, the increase in mean income

was 22 percent. Thus, the overall rate of growth is reduced by *one-third* if the São Paulo price index is used rather than the implicit price deflator! This differential is hardly small. Think what it does to the whole macro-economic discussion of Brazilian growth, especially the post-1967 "miracle."

Compared with this macro-economic effect, the consequences of using a different price index seem rather small, for example, their estimate of percentage poor in 1970 as 37.3 vs. mine of 35.5 percent. Note too that by their estimates the incomes of the poor grew faster than those of the nonpoor (38 vs. 22 percent). Qualitatively, at least, my published results hold up.

The conclusions I would draw from B-C's findings differ from theirs. I would conclude that macro-economic data on real income change are themselves highly sensitive to the price index used; that estimates of change in absolute income and absolute poverty are similarly sensitive; that their results reaffirm my conclusion that the poor did share at least proportionally in the economic growth of Brazil; and that relative inequality indices being totally insensitive to all this are not very helpful.³ These are all matters of interpretation where we might disagree.

On one issue, B-C are in error. They state in their concluding paragraph: "The more traditional focus on relative inequality reflects the theoretically and empirically plausible assumption that the marginal utility of income is decreasing, while the absolute shares approach deliberately ignores this motive...." The second part of that statement is simply wrong. The only reason why anyone would want to calculate changes in absolute poverty is to focus in on those who presumably have the highest marginal utility of income (the poor) to the exclusion of those whose marginal utilities are assumed to be lower.

III

Albert Fishlow's comment presents new evidence claiming that poverty did not diminish in Brazil in the 1970's. My 1977

³Obviously, their Lorenz curve is the same as mine.

paper claimed that it did. What is not at issue is the reliability of the source of the basic data, since in both instances, the source is Fishlow himself (or more precisely, his rendering of Brazilian census data).⁴ What is at issue is which data are most appropriate.

Fishlow bases his claim that poverty did not diminish on a data set different from the one he used earlier. His 1972 paper examined changes in income inequality between 1960 and 1970 among *individuals* in the economically active population. His present note, on the other hand, uses data released in the interim to compare *families*. If the Brazilian data are to be believed, what they are telling us is that poverty was lessened among individuals but not among families. Thus, an assessment of the distributional performance of the Brazilian economy in the 1960's turns on the choice of recipient unit. Fishlow claims that family income comparisons are clearly superior for studying income distribution change. I disagree.

Other, more specific, points are raised in Fishlow's comment. He writes that my manner of constructing a poverty line is "both critical to the findings and quite illegitimate" (p. 250). He is wrong on both counts. Drawing the poverty line at 2.1 is not critical. The same qualitative results would have been found had the poverty line been drawn at 3.3 (the cutoff of the next higher income bracket). In the absence of reason to suppose otherwise, it is certainly legitimate and defensible to do what I did:

⁴Fishlow's tabulations go beyond the published data in at least one important way. The census publications do not report actual income shares for each income group. Fishlow published derived shares, which he obtained using a two-way procedure: in the case of the lowest income bracket, by fitting a Pareto distribution; for the other income brackets, by setting the mean equal to the midpoint. This procedure introduces particular assumptions about the distribution of income *within* the group classified as poor. We have no way of knowing whether these assumptions are or are not accurate. When I wrote my 1977 paper, I did not know that the income shares published by Fishlow were fitted, not actual, nor apparently did the other commentators in preparing their comments.

to approximate the proportion of individuals who are poor by the proportion of families which are poor. He complains that I used uncorrected data. That is legitimate when only uncorrected data are available. In his 1972 paper, Fishlow did *not* correct for income in kind or for regional price differences in making *intertemporal income distribution comparisons*, and so neither did I when I used his data. It is legitimate, I submit, to include individual zero-income recipients, since unemployment reduction is an important means of poverty alleviation.⁵ To exclude zero-income workers is, I think, a cure worse than the disease. But I must take responsibility for an unpublished arithmetic error which invalidates the statement in footnote 7 of my 1977 paper—the results *are* materially affected if zero-income workers are excluded.

Some of Fishlow's calculations are mutually inconsistent. His note argues that the correct poverty line for families is NCr\$3.3 for 1960 and an equivalent poverty line for 1970 is NCr\$125. Even if we accept these figures as equivalent, and I hesitate to, the ratio of prices is $125/3.3 = 37.9$. This implies that the 1960 mean income (NCr\$9.2) is equivalent to NCr\$348.5 when measured in 1970 prices in nominal terms. The mean income in 1970 was NCr\$401. The rate of growth of mean income is therefore $(401 - 348.5)/348.5 - 1 = 15$ percent, not 25 percent as Fishlow reports. Either the appropriate poverty line is not NCr\$125 or the rate of growth is not 25 percent. Fishlow cannot have it both ways. Which way he has it considerably affects his comparisons of income growth of the poor relative to the average. And let me record my hesitation in accepting Fishlow's calculation of changes in the Sen index based on the type of information available, especially since there is an inconsistency between the claim that the *I* component of the Sen index increased and the earlier observation that the poor's income rose albeit slightly.

⁵The census evidence cited by Fishlow showing that most of the unremunerated workers were found in agriculture, not in unemployment, is in direct conflict with the findings of P. I. Singer on which I had based my earlier judgment.

The new evidence presented in Fishlow's comment offers us a choice—on which income recipient unit (families or individuals) is most appropriate for purposes of inter-temporal analysis, on whether a concern with income or expenditure distribution is of more interest, on the legitimacy or illegitimacy of uncorrected income distribution data. I believe I might be forgiven if in 1975 (when the first draft of my paper was written) or in 1976 (when the final draft was accepted for publication), I accepted Fishlow's earlier decisions on these questions, especially when it is recognized that I was trying to show that his own data, analyzed with a different *type* of measure, would suggest a quite different interpretation. I would conclude that Fishlow has *not* presented a "single and compelling" piece of evidence to the effect that the poor in Brazil did not share in economic growth. Maybe they did, maybe they didn't, but his results do not sustain an unambiguous conclusion either way.

IV

My 1977 paper had two purposes, one methodological and one empirical. The methodological goal was to apply in the case of a less-developed country a largely overlooked class of absolute measures which gauge directly the extent to which the poor gain from economic development. At the time I wrote the paper (1975), absolute poverty measures had seldom been used in a *dynamic* context in studies of LDC's (i.e., to measure who benefits how much from economic development within a country, though Fishlow had effectively introduced these measures to construct static poverty profiles and Ahluwalia had used these measures in a *cross section* of countries). I hoped to show that those who wish to give predominant weight to countries' progress toward alleviating economic misery might find these absolute measures (changes in proportion of income units which are poor and changes in average income among the poor) more convenient than the more familiar measures of relative inequality (changes in Lorenz curves, Gini coefficients,

income shares of particular percentile groups, etc.) for gauging the beneficiaries of growth.

The empirical goal was to reexamine the specific case of Brazilian growth in the 1960's. Toward that end, my results established that the same income distribution data, when analyzed from an absolute rather than from a relative perspective, yielded a qualitatively distinct and decidedly more positive picture of who benefited.

I believe the methodological objective has been largely satisfied. Though my paper provoked much discussion, pro and con, I am unaware that anyone on either side has rejected *in principle* the call for applying absolute tools to the study of income distribution change, at least in conjunction with relative inequality measures if not as a replacement for them, though Beckerman and Coes reject absolute measures *in practice*. We are not likely to witness a return to the debates of the mid-1970's over whether the participation of the poor in economic growth is better measured by the Gini coefficient rather than by a Theil index, Kuznets ratio, Atkinson index, Pareto coefficient, *log* variance, or what have you. To the contrary, the development community is now groping toward the most appropriate way of measuring the alleviation of absolute poverty. This concern is reflected in such current phrases as "redistribution with growth," "meeting basic needs," "new directions in development assistance," "progress and commitment for the poor majority," "trickle down," and "distributional weights."

The empirical issue remains unsettled. In the last two or three years, the absolute approach has been applied to the study of distribution and development in other countries besides Brazil. The existing literature is surveyed and new evidence presented in my forthcoming book.

Scholars of integrity welcome the opportunity to subject earlier ideas to tests on new and better data. The evidence presented in the three comments, though new, is not necessarily better. But it is disturbing. The additional evidence shows recent Brazilian economic history in a less favorable light

than I portrayed it before, though in my judgment, less dismal than Fishlow's comment suggests. This can only raise new doubts on the extent to which the poor shared in Brazilian growth.

The several comments have also raised important questions about the suitability of published income distribution data for absolute poverty analysis. Since the appearance of my 1977 paper, and indeed in response to it, the Brazilian data base has come under close scrutiny and some technical limitations brought to attention. In addition to the fine points about the specifics of census reporting procedures, and tabulations derived therefrom (see fn. 4), doubts about the quality of the underlying data are also raised by the very observation that, given a nearly constant demographic structure, the family and individual distributions produce such divergent results.

Attention is rarely given to technical matters such as these; too often, we simply accept whatever data are available. But as the new evidence indicates, these technical issues are paramount in coming to even a qualitative judgment on distributional aspects of Brazilian development.⁶

Notwithstanding our differences, I expect that the several commentators would join me in two final observations: given the

available resources, much more could have been done than was done to alleviate economic misery in Brazil; and much more can be done in the future if the political will is there.

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⁶It might be noted that technical considerations were also prominent in earlier debates on changing income distribution in the course of the economic development of India.