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CENTER DISCUSSION PAPER NO. 244

MORE ON CHANGING INCOME DISTRIBUTION AND ECONOMIC DEVELOPMENT IN BRAZIL

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## I. Introduction

It is widely thought that income distribution worsened during the economic growth which took place in Brazil during the 1960's. This contention is hardly undocumented. At least four studies by respected development economists --- Fishlow [7], Langoni [10], Adelman and Morris [1], and Chenery et. al [4] --- present data supporting this conclusion. Fishlow, for example, reports an increase in the Gini coefficient from 0.59 to 0.63 and a rise in the income share of the richest 3.2% from 27% to 32%.

In my paper "A Re-examination of Brazilian Economic Development in the 1960's" [5], which I subsequently revised and retitled "Who Benefits from Economic Development? --- A Re-examination of Brazilian Growth in the 1960s" [6], I found that the entire income distribution shifted, benefiting every income class, the proportion of the economically active population with incomes below the poverty level (as defined by Brazilian standards) declined during the decade, those who remained poor were less poor than before in absolute terms, and the rate of growth of income among "the poor" was at least as great as the rate of growth among the non-poor. From these findings, I concluded that "the poor" did benefit from Brazilian economic growth in the 1960's.

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. On the other hand, neither did I expect to receive critical commentaries before the paper had even been published. Already, three challenges have been issued and circulated by Professors Porter [11] and Bacha [3] and Doctors Ahluwalia, Duloy, and Pyatt [2]. Each

challenge is thoughtful and imaginative. Each performs new calculations concerning the distribution of income in Brazil in 1960 and 1970. And each fundamentally disagrees with my conclusion on the distribution of the benefits of Brazilian economic growth.

In what follows, I hope to convince the reader that my point --- that the poor shared in Brazilian economic growth in the 1960's --- is robust to these criticisms and that the alternative position has not been substantiated. Nevertheless, there continues to be a great deal of severe poverty in Brazil, which my study in no way tries to condone. I hope the reader will interpret what follows in that light.

## II. What the Re-examination Does and Does Not Re-examine

Let me begin by stating what I did not try to do.

First and foremost, I should make clear that I have not challenged the legitimacy of any of the underlying figures on income distribution used by previous writers. I have used Fishlow's figures in preference to Langoni's for both a practical and a substantive reason. The practical reason is that Fishlow's study has been more widely-cited and his data more widely-used than those of Langoni, at least among English-speaking economists. The substantive reason is that Langoni excluded from consideration those persons in the economically active population with zero incomes, while Fishlow included them. For purposes of understanding the effect of growth on income distribution, I wanted to see whether growth reduced the rate of open unemployment and drew more of the population into the cash income sector.

Hence, I chose to use Fishlow's data, which allows measurement of change of the zero income population.

Second, I have not challenged any of the calculations of relative income inequality indices. I accept the reports by Fishlow, Langoni, and others that relative income inequality increased (whether measured by the Gini coefficient, income share of the richest x%, decile shares, or some other index). Where I differ with existing studies is over the appropriateness of the class of relative inequality measures as a whole for studying changing income distributions in growing economies. I return to this point below.

Third, my re-examination should not be understood as in any way attempting to justify the types of political measures adopted in Brazil toward the objective of economic recovery and growth. I have stated my position clearly in both versions of the paper and see no need to repeat it here.

Finally, I have not tried to speculate on what would have happened had Brazil followed some other type of development strategy. The task of chronicling how the income distribution might have been affected (for better or worse) had some alternative policies been pursued is best left to others more expert on the Brazilian economy than I.

Let me now say what I did try to do. The task I set for myself was to describe how income distribution did in fact change. My question was: who benefited from economic development, or more precisely, how did the income position of the poor change as compared with the non-poor? In answering this question, past studies of Brazil followed upon the Nobel-prize winning tradition of Professor Kuznets and used relative income measures.

Consequently, when those authors concluded that income distribution in Brazil worsened, theirs was a relativistic statement based on the full range of observed incomes. In contrast to the accepted tradition, I chose to use a methodology based on absolute poverty rather than relative incomes, and therein lies the essence of my re-examination and the qualitative difference between my results and those of earlier researchers. I accept their facts on changes in relative income inequality, but do not share in their interpretation, since I give greater weight in my judgment to other facts (those concerning absolute poverty) which previous studies overlooked.

### III. What the Controversy is All About

None of the commentators on my paper has taken issue with the absolute poverty approach per se. All appear willing to define a constant real poverty line and to examine changes in the numbers of persons above or below the line and the average absolute incomes received by each. Thus, agreement seems to have been reached on the validity of the absolute poverty approach in principle.

At issue is how well I have executed the absolute poverty approach in practice. The principal objection raised in the commentaries concerns the correctness of the procedure I followed for interpolating incomes within an income class.<sup>1</sup> The problem I was faced with and had to resolve

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<sup>1</sup>I think the commentators are entirely justified in calling my data approximations into question. We should assess the suitability of data and the appropriateness of estimation procedures far more often than we do.

was the fact that none of the sources of income distribution data for Brazil (whether those of Fishlow, Langoni, Jain, or whomever) had comparable income brackets for 1960 and 1970. In other words, taking the poverty line as NCr. \$2,100 in 1960 units, and allowing for an inflation factor of 3.53, it was impossible to obtain an exact figure for the percentage of the population below NCr. \$7,413 ( $= \$2,100 \times 3.53$ ) from any published source. In the absence of the microeconomic data, I was forced to approximate the share of the population below this amount and also to estimate their income share. The interpolation procedure I adopted applied a linear approximation to the population frequency within each bracket. For example, the first positive income bracket in 1970 runs from 0 to 2.8 constant NCr.\$\$. Applying the linear approximation,  $2.1/2.8$  of the population in the 0-2.8 category was assigned to the 0-2.1 category and the remaining  $0.7/2.8$  to the next higher category.

In retrospect, this was a rather poor way to go about it. My conclusion in the original version (p. 10) was that "the incomes of 'the poor' grew at a rate double that of the 'non-poor'". In his comment [11], Professor Porter observed that my interpolation procedure assigned the average income in that category to all persons within that bracket. Since the poorer x% must have received less than the richer x%, Professor Porter concluded (rightly) that I had overstated the income gains of "the poor." Consequently, the conclusion in the original draft that the growth rate of incomes among the poor was double that of the non-poor was too strong, since the true figure was necessarily lower.

The question that naturally arose then was how great was the bias introduced by my interpolation procedure. As I shall show below, if we

accept the legitimacy of Fishlow's data, it is impossible for the alternative conclusion --- that the poor may have benefited proportionately less than the non-poor --- to be the case under any set of assumptions or interpolation procedures.

It is probably worth repeating that what we have here is a disagreement over how to best approximate the necessary figures with inadequate data, not a conceptual disagreement on how to treat the ideal data. The data problem could easily be resolved if we had either a public use sample with the microeconomic data or a special tabulation of the income distribution conformable to Fishlow's (or Langoni's) definitions. Unfortunately, we do not.

#### IV. Income Growth of the Poor and A Proof of the Impossibility of the Alternative Conclusion Using Fishlow's Data

The purpose of this section is to establish the following result:

If we accept the validity of the income distribution data presented in Fishlow [7], the poor must have received income gains at least as great as those of the non-poor. Under no set of assumptions can the alternative conclusion --- that the poor may have benefited proportionately less than the non-poor --- be sustained.

The proof goes along the following lines. I had assumed that incomes within the lowest positive income bracket were equally distributed. This is clearly one extreme assumption. The other alternative extreme assumption is that incomes within that bracket are distributed as unequally as possible. I shall show that even under this most extreme alternative assumption, the poor could not have received a smaller percentage



income increase than the non-poor.

Proof: In my work, I assumed that the poorest 75% of the economically active population in the 0 - 2.8 category received 75% of that group's income. In objecting to that assumption, Professor Porter illustrated a situation where the richest 25% in the 0 - 2.8 bracket are assumed to receive 50% of that group's income and the poorest 75% receive the other 50%. He calculated that the average earnings of the poor would rise by only 3%, while those of the non-poor would rise by 31%. This example, if valid, would destroy my earlier conclusions. Let me now prove the impossibility of Porter's example.

The following data for 1970 are derived from Fishlow's original data, reproduced in Table 1 of "A Re-examination...":

<u>Undeclared</u> <u>Income</u> <u>Bracket</u>	<u>Mean Income</u> <u>in Bracket,</u> <u>Undeclared</u>	<u>Deflated</u> <u>Income</u> <u>Bracket</u>	<u>Mean Income</u> <u>in Bracket,</u> <u>Deflated</u>
None	$(0/11.7\%) = 0$	None	0
1 - 100	$(8.0\%/31.7\%) \times 258.1 = 65.04$	0 - 2.83	1.84
101 - 150	$(6.2\%/12.8\%) \times 258.1 = 124.92$	2.83 - 4.25	3.54
151 - 200	$(10.6\%/15.6\%) \times 258.1 = 175.25$	4.25 - 5.66	4.96

Mean incomes within brackets are calculated as the income share of that group as a fraction of population share multiplied by mean income.

The first two columns are deflated by 35.32 to be comparable with 1960 data. (See footnote 1, p. 6, of "A Re-examination..." or footnote 2, p. 7 of "Who Benefits...") If the richest 25% of the 0 - 2.83 group were to receive 50% of the income, their income total would equal  $50\% \times 1.84 \times 31.7\% P = 29.2\% P$ . Since total income is  $7.31 P$ , their share is  $(29.2\%P)/7.31 P = 3.99\%$ . If  $7.925\%$  ( $=25\% \times 31.7\%$ ) of the population

receives 3.99% of the income and the mean is 7.31, the mean of the richest 25% of the 0 - 2.83 group is then  $\frac{7.31 (3.99\%)}{7.925\%} = 3.68$ , which exceeds the upper limit of their category (2.83). Thus, it is impossible for the richest 1/4 to receive as much as 1/2 the income in their bracket, as in Professor Porter's example.

How much can they possibly receive? Let  $\alpha$  equal the maximum share of the 0 - 2.83's income which could be received by the richest 25% within that category. The richest 25%'s income is given by  $\alpha(1.84)(31.7\%P) = 2.83 (1/4 \times 31.7\%P)$ . Solving, we find  $\alpha = .3845$ . Thus, the richest 25% within that bracket could receive at most 38.45% of that bracket's income, and the remaining 75% of the people would receive 61.55% of the income. The poorer 75% in that bracket would then have a mean income of

$$\frac{.6155 (.5832P)}{75\% (31.7\% P)} = 1.51.$$

From these data, we may compute the mean income of the poorest 35.5% of the population, under the assumption that the richest 25% of those in the 0 - 2.83 bracket earn at the maximum of the bracket. We find

$$\bar{y}_p^{70 \text{ Min}} = \frac{23.775\%}{35.475\%} \times 1.51 = 1.01.$$

The superscript Min denotes that this is the minimum possible value of  $\bar{y}_p^{70}$  under the maintained assumptions. Turning now to the non-poor,

for  $\bar{y}_n^{70 \text{ Max}}$ , we have

$$35.475\% P^{70} (1.01) + 64.525\% P^{70} \bar{y}_n^{70} = 7.31 P^{70},$$

from which we obtain  $\bar{y}_n^{70 \text{ Max}} = 10.77$ .

For 1960,  $\bar{y}_p^{60} = .78$  and  $\bar{y}_n^{60} = 8.30$ . Hence,

$$\frac{\bar{y}_p^{70} - \bar{y}_p^{60}}{\bar{y}_p^{60}} = \frac{1.01 - .78}{.78} = 30\%,$$

and

$$\frac{\bar{y}_n^{70} - \bar{y}_n^{60}}{\bar{y}_n^{60}} = \frac{10.77 - 8.30}{8.30} = 30\%,$$

and the percentage increases are exactly equal.

From these calculations, we find that it is impossible for the poor to have received a smaller percentage income increase than the non-poor, given that the richest 25% in the 0 - 2.83 range are assumed to receive the maximum allowable income in that bracket.

Of course, there is nothing sacred about the selection of 25% as the upper income group. More generally, we may ask: if we assume the greatest possible inequality within the 0 - 2.83 bracket, could the poorest 37.0% (which corresponds to the percentage below the poverty line in 1960) possibly receive less income in 1970 than in 1960? It is easily shown that the answer is negative. Let us define  $\theta$  as the maximum share of the 0 - 2.83's income which could be received by the richest  $\theta$  percent. Then,  $1.84\theta = 2.83\theta$ , or  $\frac{\theta}{\theta} = 1.54$ . The most unequal possible income distribution is when  $\theta \rightarrow 1$ , and therefore  $\theta \rightarrow .649$ ; the remaining 35.1% of the persons in that bracket would receive nearly zero incomes. Thus, with the greatest possible inequality within the 0 - 2.83 category, the 1970 income distribution would be

<u>Income</u>	<u>% of Population</u>
0	11.7%
0	$35.1\% \times 31.7\% = 11.1\%$
2.83	$64.9\% \times 31.7\% = 20.6\%$
> 2.83	56.6%

The minimum value of the income received by the poorest 37.0% equals  $2.83 \times (37.0\% - 22.8\%) P$ , and their minimum share is this figure divided by total income (7.31 P). This minimum share is 5.49%. Hence, under the most extreme assumption, the income share of the poorest 37.0% definitely did not fall in Brazil between 1960 and 1970, and appears to have risen by 0.3%. Since real national income per capita rose by 32%, the real incomes of the poorest 37.0% rose by at least 40%. ( $(\frac{5.49}{5.2} \times 1.32) - 1$ ). Furthermore, the more equal the distribution of income within the 0 - 2.83 bracket, the greater the percentage income gain of the very poorest during the economic growth of Brazil in the 1960's. Thus, it could not be that the incomes of the poor grew more slowly than those of the non-poor.

Q. E. D.

#### V. On the Inconsistency Between Actual Income Distribution Data and Fitted Data and the Conclusions Therefrom.

In their comments [2] Ahluwalia, Duloy, and Pyatt (hereafter A-D-P) purported to show that even the softened conclusion --- that the poor had income gains at least as great as the non-poor --- was incorrect. Their arguments follow along two lines:

(1) A critique of the particular interpolation assumptions I made, and (2) the results of an analysis based on fitted Lorenz curves. I have already considered their first point in the last section. This section

concerns their second point.

A-D-P make use of data calculated by Shail Jain [8]. These data pertain not to the actual income distribution but rather to a fitted distribution, based on a procedure for estimating Lorenz curves from grouped data suggested by Kakwani and Podder [9]. A-D-P find ". . . radically different estimates of the growth of income of the poor. [A-D-P's] estimate implies that the mean income of the poor rose from 77.63 in 1960 to 82.67 in 1970 --- an increase of 6.5 percent compared to an increase in mean income of 32 percent. Against this, [Fields'] estimate implies an increase in mean income of the poor of 62 percent!"

After a careful review of the A-D-P calculations, I am convinced that the data fitted by Jain do in fact imply the conclusions A-D-P have drawn. Let us recall, however, that the Jain data used by A-D-P are estimates. I will now show that the estimated data systematically understate the actual growth in mean income among the poor.

(i) As compared with the actual income distribution, A-D-P overstate the income of the poor in 1960.

Proof: In 1960, the poor (defined as those with incomes less than Cr\$ 2.1) actually received 5.2% of total income. In fact, 37.0% of the population had incomes below that level. Next, A-D-P estimate that the poorest 38% received 5.75% of the income. This means that the 38th percentile is estimated to have received 0.55% of the income. However, the next 14.4% of the population received 7.0% of the income, or less than 0.5% per percentile. Thus, the income of the poor in 1960 was overstated.

(ii) As compared with the actual income distribution, A-D-P understate the income of the poor in 1970.

Proof: In 1970, the poorest 43.4% of the population received 8.0% of the income. A-D-P estimate that 6.0% of the income was received by the poorest 40%. Hence, the 3.4% of the population between 40.0% and 43.4% are estimated to have received 2.0% of the income, or approximately 0.65% per percentile. Since that income bracket included 12.8% of the population who received 6.2% of the income, this estimate implies that the remaining 9.4% of the people at the upper end of that income bracket must have received 4.2% of the income, which gives an average of about 0.45% of the income per percentile. Thus, according to their estimates, the poorer people in the 101-150 bracket are estimated to have received incomes which are 50% higher than the incomes received by the richer persons in that bracket, which is clearly impossible.

(iii) From (i) and (ii), it follows that the data used for the A-D-P calculations understate the growth of income of the poor in Brazil between 1960 and 1970. Q.E.D.

The difficulties encountered with these data derived from the fitted Lorenz curve call into question the estimating procedure itself. As mentioned above, A-D-P's estimates are based on figures calculated by Jain [8] using the procedure of Kakwani and Podder [9], hereafter referred to as K-P. Without going through K-P's methodology, the precision of the values calculated by Jain is subject to doubt for the following reasons:

(i) K-P present four methods for estimating Lorenz curves. Their simplest method (Method I) utilizes ordinary least squares (OLS). It is apparent from the Lorenz curve itself that the assumption of homoscedasticity in OLS is not likely to be fulfilled, which would render the OLS estimates inefficient, although still consistent. Yet, this inefficient procedure

(with correspondingly larger errors in estimation) is the procedure used by Jain and accepted by A-D-P, which might explain part of the inconsistency between the actual and income fitted distributions.

(ii) A second difficulty is that the Jain computations are based on decile groupings. Yet, their accuracy is called into question by K-P's observation of income groups from 11 to 20 improves the accuracy of the technique." This, of course, pinpoints the whole issue that what goes on within certain deciles (the third and fourth) is critical to the calculations.

(iii) Most importantly, even the most efficient of the K-P procedures (Method IV) does not do a very good job of predicting the incomes at the extreme lower end of the distribution, which is the concern in Brazil. Based on 11 groups, K-P estimate the share of the poorest 5% in Australia as .5857% of total income, when in fact they received .767%, a percentage differential of some 20%. Thus, despite the goodness of fit (as measured by  $R^2$ s in the neighborhood of 0.99) for the Jain regressions for all 81 countries, the particular prediction associated with the value  $\text{pop.} = 5\%$  is in error by a rather large amount. Might predictions of the income shares at the lower ends in Brazil not also be in error by similar orders of magnitude?

In summary, the claim by A-D-P that the growth of incomes in Brazil among the poor was less than among the non-poor is based on data fitted by a procedure which we have reason to believe is not very precise and which furthermore has been shown to be inconsistent with actual data on at least two counts. Since small errors in the overall fit might well make large differences in the particular values with which we are concerned, and since the fitted data differ considerably from the actual data in a systematic

fashion supportive of the A-D-P result, it does not appear that the A-D-P claim has been decisively established nor my conclusion convincingly refuted.

#### VI. On Comparing the Poor and the Non-Poor.

A somewhat different sort of critique is that of Professor Bacha [3]. Bacha accepts my calculations concerning the growth in average incomes among the poor as compared with the non-poor, but disagrees with my interpretation. His critique consists of two main points which I shall try to refute below. He writes:

(1) "Dividing up the economically active population in three equivalent income intervals, we characterize the process of income concentration in Brazil during the sixties by the facts that the proportion of the poorest category increased from 62 to 71 percent of the labor force; the share of the labor force in the intermediate interval. . . decreased from 32 to 21 percent between 1960 and 1970; finally, the share of the labor force in the upper income interval went up from 6 to 8 percent in the same period."

(2) "Swollen in numbers, from 62 to 71 percent of the working force, the lowest income earners also increased their share in total income from 22 to 27 percent. Proportionally, the increase in numbers was less than the gains in income; hence the average income of the "poor" went up by more than the overall average. But this. . . is a mere statistical artifact."

Beginning with Professor Bacha's first point, the key statement is the term "equivalent income intervals." Bacha alleges that the income interval "less than 2.1 1960 Cr.\$" is equivalent to the bracket "less than 99 1970 Cr.\$."



To arrive at this conclusion, he used a formula developed in the early work of Langoni:

$$U_i^{1970} = U_i^{1960} (1+n)(1+r),$$

where  $U_i^{1970}$  is the upper limit of the  $i$ 'th income interval,  $n$  is the rate of inflation between 1960 and 1970, and  $r$  is the real rate of growth of per capita gross domestic product between these same years. Indeed, with  $(1+n) = 35.32$  and  $r=32\%$ , the value 99 is obtained. However, since it is blown up by  $(1+r)$ , this figure can hardly be said to define incomes which are equivalent to one another or to define a constant real poverty line.<sup>1</sup> Thus, Bacha's conclusion that the proportion of the poorest category increased from 62 to 71 percent of the labor force" is not based on a constant poverty line and consequently should not be given serious consideration.

We can, however, recalculate equivalent 1970 income intervals by dividing each value in Bacha's Table 1, Column 8, by  $1+r = 1.32$ , or equivalently, multiplying the values in Column 1 by 35.32. Interestingly, we find that the fourth income bracket terminates at 158 Cr.\$, which conveniently permits comparison with the economically active population in the first three categories. According to this calculation, in 1970, no more than 55.3% of the economically active population had incomes below 155 Cr.\$. (This figure corresponds to 4.5 Cr.\$ in 1960.) The

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<sup>1</sup>It is instructive to note that Langoni performed this calculation only in the preliminary version of his book. Is it possible that the reason he dropped it was that  $U_i^{1960}(1+n)(1+r)$  represents greater purchasing power than  $U_i^{1970}$ ?

percentage below that figure in 1960 was 62.1%. Combining these with the results presented in my paper, we have:

Percentage of economically active population below 2.1 Cr.\$, 1960:	37.0%
Percentage of economically active population below 2.1 Cr.\$, 1970:	35.5%
Percentage of economically active population below 4.5 Cr.\$, 1960:	62.1%
Percentage of economically active population below 4.5 Cr.\$, 1970:	55.3%

Professor Bacha's assertion that the low income group got larger is thus refuted regardless of the definition used.

Turning now to Bacha's second point, I would argue that the fact that the average income of the poor went up is not a statistical artifact, as he claims. He presents a simple economy with three persons a, b and c with incomes at time t such that  $x_{at} > x_{bt} > x_{ct}$ , where x is a constant poverty line. Suppose that at time  $v \neq t$ , a positive income transfer from person b to person a has taken place, so that  $x_{av} > x_{bv} > x_{cv}$ . Professor Bacha notes that although income distribution worsens in this example due to the regressive transfer, average incomes rise in both income classes. Thus, an increase in average incomes among the poor could be a statistical artifact concealing a deteriorating income distribution. For this reason, Professor Bacha dismisses as irrelevant both my finding and his finding that the average income of the poor went up by more than the overall average.

I would argue that the finding is not irrelevant for the very simple reason that in Professor Bacha's example, the size of the low income group gets larger, while in the actual data, the low income group becomes

smaller. I would challenge him to produce an artifactual example with the low income group getting smaller and their average incomes rising, yet the overall income position of the poor deteriorating absolutely. Since I cannot think how such a situation could come about, I would maintain my earlier position that the increase in income share of the lower income group does reflect an economically-meaningful improvement.

#### VII. Concluding Points

A-D-P, in their note, raised the possibility of examining changes in the distribution of income amongst families rather than individuals. Fishlow's data did not permit the analysis of families. In light of the fact that they and their colleagues at the World Bank have in the past accepted the legitimacy of Fishlow's conclusions without question, I didn't think it was necessary to justify the use of Fishlow's data now.

Professor Bacha, I trust, will find that the contributions of Brazilians and Brazilianists have been suitably accounted for in "Who Receives the Benefits. . ." [6]. His speculation about the impact of changes in the distribution of non-cash income merits serious analysis when and if the data permit, and I too await the results of such a study.

Professor Porter devoted much of his comment to the more general point that the Gini coefficient may not be an adequate measure of distributional change. His objections are valid ones. But more fundamentally, the question is more than just "one more measure showing something else." At issue is how to translate the general concerns which many of us share for the distributional consequences of growth into suitable form. Perhaps the major lesson from the debate over Brazil is that economic science does not yet provide a satisfactory answer. My critics would undoubtedly agree.

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