

Rural Poverty and Its Alleviation in India A Critical Scrutiny

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IN Section II of their paper [6], Kakwani-Subbarao (K-S for short hereafter) examine the 'trends in inequality and poverty' using the four time-points of National Sample Survey, namely, 1972-73 (27th round), 1973-74 (28th round), 1977-78 (32nd round) and 1983 (38th round). The survey periods covered by these rounds are given in column (2) of Table I. Their major conclusion is as follows:

In general, ... the magnitudes of reduction in poverty are higher for the period 1977-78 to 1983 than for the period 1973-74 to 1977-78. This is an interesting result because 1973-74 to 1977-78 was a period of higher growth (2.7 per cent) with increasing inequality whereas 1977-83 period was characterised by a somewhat lower per capita growth of consumption (1.7 per cent) but accompanied by substantial decrease in inequality. *The decrease in inequality was the major factor which led to a substantial reduction in poverty in the second period* (page A-5, emphasis added).

Notice that the conclusion with added emphasis asserts that inequality reduction was the major cause of substantial reduction in poverty. If it is empirically established it has far-reaching policy implications. It implies that relative inequality reducing policies would have a larger impact on the incidence of poverty than growth-promoting policies.

Because the conclusion is far-reaching in nature and because it comes from well-established authors, it is necessary to subject this paper to a critical scrutiny. In our subsequent analysis, we argue

- (1) that the methodology adopted by the authors does not establish the causal connection asserted in the conclusion quoted above;
- (2) that the authors are incorrect in inferring from two-point comparisons about the trends regarding the movements in the headcount ratio (or poverty-gap ratio) from the three discrete time-points;
- (3) that even if we interpret their conclusion in a purely descriptive sense (as opposed to the causal sense implied by the authors) it does not follow from their own empirical results;
- (4) that their descriptive conclusion would be further vitiated because of certain important data problems and inappropriate sets of price deflators used by them;
- (5) that their last section has only tenuous connection with earlier Sections II to VI;
- (6) that the authors have given several regressions results without specifying the underlying economic rationale.

METHODOLOGICAL PROBLEMS

The authors carry out two distinct and independent exercises, namely, (A) a simple decomposition exercise trying to allocate a percentage change in a poverty measure into that attributable to growth in real mean per capita total expenditure (PCTE) and the residual to changes in relative inequality (Section V) and (B) an effort to assess 'responsiveness of poverty to growth and inequality changes' (Section VI). The details of both are not adequately provided in the paper.

As regards (A), our reworking of what authors may have done indicates the following procedure adopted by them. Let H_T and H_0 be the headcount ratios (or any other poverty measure used by the authors) in the terminal and the base year, respectively. The total annual compound rate of growth (decline) of the poverty measure over period of T years is given by r where

$$H_T = H_0 (1+r)^T \quad (1)$$

Now a hypothetical headcount ratio \bar{H} has been defined which indicates the headcount ratio that would arise if only real mean PCTE were to change but the Lorenz curve were to remain unchanged. \bar{H} can be defined in two ways, namely, the Lorenz curve remains unchanged as in the base year or as in the terminal year. The authors have not explained the procedure they have used. There are also interpretational problems of which the authors seem to be unaware. This is discussed by the present authors in their paper [5]. Whichever way \bar{H} is defined their growth effect is defined by the annual growth rate r_g , where

$$H_T = \bar{H} (1+r_g)^T \quad (2)$$

The inequality effect (r_i) follows as a residual

$$r_i = r - r_g \quad (3)$$

Tables 10 to 12 (each in K-S) indicates r_g , r_i and r for two periods 1973-74 to 1977-78 and 1977-78 to 1983 for three poverty measures: headcount ratio for poor, headcount ratio for ultra-poor and poverty gap ratio.

It is important to emphasise that r_g and r_i constitute components of arithmetical decomposition purely descriptive in nature and without any explanatory content in the absence of any explicitly formulated economic model relating poverty indicator, real mean PCTE and Lorenz curve. This scheme is incapable of establishing the causal connection asserted by the authors and quoted in the beginning.

As regards (B), Section VI in K-S provides impressive analytical expressions for 'growth elasticity' (GE), 'inequality elasticity' (IE) and 'marginal proportional rate of substitution' (MPRS) as an absolute value of the ratio of IE to GE. Again, in the absence of any explicitly formulated economic model incorporating the mechanisms and processes connecting poverty indicator, mean PCTE and relative inequality, these expressions are at best descriptive in content. The expression for GE indicates the impact of an infinitesimal increase in real mean PCTE on a given poverty measure holding constant the Lorenz curve. This is at least descriptively unambiguous in direction. The same, however, cannot be said about the so-called 'inequality elasticity'. It is well recognised (and the authors are aware) that there is no unambiguous, general one-to-one relationship between (a) the Lorenz curve and the summary indicator of Gini coefficient (which is derived from the Lorenz curve) and (b) the Gini coefficient and the poverty measure. In practice, Gini coefficient and poverty measure can change completely independently of each other! (for a diagrammatic illustrations, see [3]). In order to get around this problem—again in a purely descriptive fashion—the authors consider a very special class of uniformly proportionate outward movement in Lorenz curve. In this special case of a specific class of non-intersecting Lorenz-curves, the information contained in the entire Lorenz curve is uniquely mapped into the Gini coefficient. This particular way of defining a shift in the Lorenz curve enables the authors to derive elegant analytical expressions for IE. Strictly speaking—and this is important for interpretation—IE indicates a descriptive elasticity of any given poverty measure with respect to a uniformly proportionate infinitesimal outward movement in the Lorenz curve which, by construction, is equivalent to an infinitesimal increase in the Gini coefficient. Although it is directionally made unambiguous by a particular construction, it is again absolutely important to emphasise that 'inequality elasticity' in general is not directionally unambiguous. The numerical magnitudes of GE and IE are given in their Tables 13 and 14 for the poor and the ultra-poor at three time-points: 1973-74, 1977-78 and 1983. The magnitudes of marginal proportional rates of substitution being the absolute value of the ratio of IE to GE are given for the three selected time-points in their Table 15 where the column headings are wrongly marked.

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Three comments are relevant in this connection. First, GE and IE in their Tables 13 and 14 *cannot* be related to growth and inequality effects in their Tables 10 and 11. The latter are arithmetical decompositions between two observed discrete time-points whereas the former are derived for infinitesimal changes. Similarly, inequality effect in (A) being the component attributable to an actual change in Lorenz curve can be directionally ambiguous whereas IE in (B) is made directionally unambiguous by a particular construction.

Secondly, the authors have given the magnitudes of GE and IE which are point-elasticities evaluated presumably at observed points. These are not invariant to the points at which they are evaluated. Consequently, the authors' statements regarding GE and IE showing "a general tendency to increase over time, the increase being slower in poorer states" (p A-9), etc. are conditional upon the points of evaluation which should have been kept constant in order to detect true inter-temporal changes.

Thirdly, on the basis of the magnitudes of inequality elasticities they make two statements (1) "... if the inequality deteriorates during the course of economic growth, poverty may increase even with a faster economic growth because of increasing inequality elasticity" and (2) "both growth and inequality elasticities are considerably higher for the ultra-poor than for the poor implying that increasing inequality will hurt ultra-poor more than the poor" (p A-9). Both the statements imply *general* reduction in inequality and not a *particular* characterisation used in (B). In this case, it is possible to increase inequality *and* reduce the head-count ratio for the poor or the ultra-poor. This can be done if the fractile-groups in the middle of the distribution lose out and the beneficiaries are located at the top and the bottom end of the distribution. This is just another way of making a point made earlier, namely, that 'inequality elasticity' does *not* give an idea about the impact of *any* general change in relative inequality. In order to make sense out of these magnitudes it is necessary to model the mechanisms and processes connecting growth, relative inequality and poverty. In the absence of this, the magnitudes quoted by the authors are at best descriptive and the conclusions drawn from them misleading.

MISLEADING 'TRENDS' FROM END POINT COMPARISONS

The authors repeatedly refer to annual compound growth rates emerging from two-point comparisons as 'trends in poverty' (their Sections II and IV). Ahluwalia [1] (to whom reference has been made by the authors) established two propositions on the basis of a time-series analysis of rural poverty:

(1) The incidence of rural poverty has been wildly fluctuating without any trend over the period from 1956-57 to 1973-74.

(2) The incidence of poverty is negatively associated with the real agricultural value

added per head of the rural population.

The authors take note of the second conclusion but ignore the first. The two propositions taken together imply that in the absence of a complete time-series, it would be hazardous to infer any 'trends' on the basis of two-point comparisons. This was pointed out earlier (see [9]). Moreover, if one were forced to infer trends from selected time-points, it is necessary to carefully examine year-specific effects. The authors have not done this. It may be noted that 1973-74 was a year of indifferent harvest preceded by a major drought of 1972-73. The rate of inflation during this period is known to be very high (see [2]). On the other hand, the survey for 1977-78 coincided with the agricultural year which was a local peak up to 1977-78. Similarly, the harvest for the survey period of calendar-year 1983 may be taken to consist of the rabi of 1982-83 and the kharif of 1983-84. In this case too, a local peak was attained up to that time. Consequently, the comparison between 1977-78 and 1983 may be considered to be valid from the point of view of agricultural harvest. (There are data problems with the year 1977-78 which seriously affect the authors' conclusion. These are taken up later in this note.) However, the comparison between 1973-74 and 1977-78 would be seriously affected by the base year being abnormal. There is an additional problem with the 28th round data relating to 1973-74, namely, it was a round extending over 9 months from October 1973 to June 1974. It is well-known that mean PCTE as well as its distribution

would be affected by seasonal factors for a round not covering the entire twelve month period. Consequently, the so-called 'trends' noted by the authors contain substantial element of year-specific effects and are misleading as trends. At best, they are observed point-to-point compound annual growth rates and not trend growth rates.

CONCLUSION AND EVIDENCE

Before we elaborate the data problems which vitiate the conclusion of the authors, it is necessary to take a closer look at their conclusion in the light of the information given by the authors in the set of tables in their paper.

We start with pointing out an error. The quotation mentioned in the beginning gives the growth rates of per capita consumption between 1973-74 and 1977-78 as 2.7 per cent and between 1977-78 and 1983 as 1.7 per cent. These growth rates do not match with those derived by the authors in their Table 3 (page A-4, last line). While the differences are not substantial, they are indicative of possible lack of care on the part of the authors.

We have already argued earlier that the decomposition exercise cannot establish the causal link maintained by the authors. If we take their conclusion to be *purely* descriptive, does it get established by the figures they have quoted in their paper?

A close examination of Table 10 to 12 given by the authors reveals the following points.

TABLE 1: DETAILS OF NATIONAL SAMPLE SURVEY (NSS) ROUNDS

NSS Round No	Survey Period	Abbreviation for Survey Period	Data Source
27	October 1972 to September 1973	1972-73	<i>Sarvekshana</i> , Vol II, No 3, January 1979
28	October 1973 to June 1974	1973-74	<i>Sarvekshana</i> , Vol I, No 1, July 1977
32	July 1977 to June 1978	1977-78	<i>Sarvekshana</i> , Vol IX, No 3, January 1986
38	January 1983 to December 1983	1983	<i>Sarvekshana</i> , Vol IX, No 4, April 1986

TABLE 2: COMPARISON OF MEAN PCTE USING ALTERNATIVE SET OF DEFLATORS AND K-S ESTIMATES

Sl No	NSS Round No	Survey Period	Nominal Mean PCTE (Rs Per Month)	Deflator 1973-74=100	Real Mean PCTE (Rs Per Month)	K-S Estimate of Real Mean PCTE
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	27	1972-73	44.17	82.09	53.81	53.00
2	28	1973-74	53.01	100.00	53.01	51.80
3	32	1977-78	68.89 (65.95)	118.34	58.21 (55.73)	59.10 (56.70)
4	38	1983	112.45	188.73	59.58	61.70

Notes: (1) Column (6) is worked out from columns (4) and (5).
 (2) Adjusted estimate for data problem for the 32nd round is given in bracket in line 3.
 (3) Unadjusted estimate for K-S in line 3 is taken from the earlier version of their paper.
 (4) K-S estimate in column (7) may presumably represent an aggregate for the 15 states covered by them whereas that in column (6) is at the all-India level.

Sources: (1) Various published NSS Reports for column (4)
 (2) Minhas et al [7] for column (5)
 (3) Kakwani-Subbarao (K-S) Table 2 for column (7).

Whether one takes period I (1973-74 to 1977-78) or period II (1977-78 to 1983) distinguished by K-S, the growth effect due to a rise in real mean PCTE was in general larger in numerical magnitude than the inequality effect for all the poverty measures. Cases involving *dominant and favourable* inequality effect are exceptions rather than a rule and we may note them. During period I we observe an interesting case of Haryana for the poor and Gujarat for the ultra-poor where favourable inequality effect dominated over growth effect. During period I where the authors claim a major impact of reduction in inequality on poverty, the inequality effect is indeed generally favourable *but* it is numerically dominant only in the case of Punjab for headcount ratio for the poor (Table 10) with Orissa (and possibly Gujarat) also coming into the set when we consider the headcount ratio for the ultra-poor (Table 11) or the poverty-gap ratio (Table 12). Consequently, the conclusion drawn by the authors, namely, "the decrease in inequality was the major factor which led to a substantial reduction in poverty in the second period" does not follow even descriptively from a careful examination of Tables 10 to 12 given by the authors themselves.

It is, however, true that the component in decomposition attributable to changes in

Lorenz curve, was mostly adverse during period I and mostly favourable during period II but that cannot sustain the conclusion of the authors. What can be inferred is that growth and inequality changes were mostly mutually reinforcing in period II and mutually offsetting in period I. This negates their position. Even this conclusion is subject to the data problems to which we turn now.

DATA PROBLEMS

There are two major data problems with the K-S paper which would vitiate even the descriptive conclusions drawn by them. The first problem relates to the choice of the price deflator and the other to the unusual nature of the survey data for 1977-78.

First, we comment on the choice of the deflator. The authors presumably use the same deflator, namely, consumer price index for agricultural labourers (CPIAL) for adjusting both the poverty line and the mean PCTE for state-specific price changes over the time-points of the four National Sample Surveys on consumer expenditure.² Earlier researchers have indeed used CPIAL for adjusting the poverty line. However, Minhas et al [7] have worked out state-specific consumer price indices separately for the total rural population (CPITR) and Minhas and Jain [8] for the middle range of the rural population (CPIMR) where poverty line may be expected to lie. Appropriate weighting diagrams and averages of village-specific price quotations have been used in the computation of CPITR and CPIMR. CPITR provides the conceptually appropriate deflator for mean PCTE and CPIMR for the poverty line.

The second data problem relates to the 32nd round of NSS for 1977-78—the year which plays an absolutely crucial role in the K-S conclusions. This problem has been discussed in detail at the all-India level elsewhere (see [4]). It has been pointed out that the 32nd round, compared to the 27th (1972-73) and 38th (1983) reports unusually high proportion of total expenditure spent on durables and an equally unusually low proportion spent on food at the top end of the PCTE-scale in the open-ended class-interval. This phenomenon is much sharper for the rural than for the urban population. This brings out an unusual nature of the published results of the 32nd round. This would affect (a) mean PCTE for the open ended class interval; (b) mean PCTE for the entire population (thereby affecting the growth effect in the decomposition); and (c) the Gini coefficient (which would affect the inequality effect in the decomposition). It may be noted that the nominal Gini coefficients for the rural and the urban population turn out to be about equal for 1977-78 whereas for all the other rounds, the Gini coefficient for the rural population has always been distinctly lower than that for the urban population. A detailed discussion

of the possible adjustments has also been presented in the paper [4]. K-S are aware of this problem but they claim the problem to be serious only for Maharashtra and Rajasthan and have made some adjustments the details of which are nowhere available in their paper.

If we make adjustments for the conceptually more appropriate sets of price deflators and for the data problems of the 32nd round, K-S conclusion does not hold at the all-India level. We may start by noting that K-S adjustment and our adjustment for mean PCTE at the all-India level is not very different in percentage terms. Adjusted estimate of K-S is 95.94 per cent of the unadjusted one whereas it is 95.74 per cent in our case (Table 2). We may also note that K-S estimate of mean PCTE at the all-India level is presumably aggregated from the corresponding state-level estimates. Since K-S have not provided the information regarding the state-level population estimates, we could not work out the corresponding estimate using the state-level CPITR which are available (see [7]). Our estimates at the all-India level are directly taken from the table published for the all-India rural population. However, this difference would not affect the growth rates which form the basis of K-S conclusion.

Table 3 shows that based on the unadjusted estimates for the 32nd round, there was considerable slowing down in the rate of growth of real mean PCTE whether one uses K-S estimates or those based on alternative set of deflators. However, unadjusted K-S estimates are on the higher side: After adjustment for data problem for 1977-78, the picture drastically changes. Using alternative set of deflators (line 3 of Table 3) there is a slight increase in the rate of growth of real mean PCTE from period I to period II whereas K-S get the opposite result (line 4 of Table 3). Since the alternative set of deflators is conceptually more appropriate and empirically more sound, the conclusions based on them ought to be more acceptable than K-S estimates. In other words, growth effect would be somewhat stronger during the second period than during the first. Since the Gini coefficient also stands artificially inflated because of the data problem for 1977-78, the reduction in Gini coefficient in period II would also appear sharp although that should not be taken to be a reasonable representation of what actually happened. Consequently, the very edifice of K-S conclusion quoted at the beginning appears to be shaky at the all-India level.

Another empirical inconsistency in the K-S paper may be pointed out at this stage. The growth rates of per capita consumption quoted in their Table 3 are at constant prices whereas the growth rates of Gini Index and Theil's Measure quoted in their Table 6 are at current prices. That inequality indices computed at current and constant prices can and do differ, has already been established (see [4]) at the all-India level.

As regards the data problem for 1977-78,

TABLE 3: AVERAGE ANNUAL COMPOUND GROWTH RATES OF REAL MEAN PCTE: K-S ESTIMATES AND THESE BASED ON ALTERNATIVE DEFLATORS

Sl No for 1977-78	Nature of Estimate	1973-74 to 1977-78 Period I	1977-78 to 1983 Period II
(1)	Unadjusted based on alternative deflators	2.39	0.50
(2)	Unadjusted based on K-S	3.40	0.80
(3)	Adjusted based on alternative deflators	0.89	1.18
(4)	Adjusted based on K-S	2.30	1.50

Source: Table II, column (6) for lines 1 and 3. Table II column (7) for lines 2 and 4.

TABLE 4: SHARE OF MAHARASHTRA AND RAJASTHAN IN CERTAIN RELEVANT ALL-INDIA RURAL MAGNITUDES

	(Per Cent)
(1) Share in all-India rural population	13.26
(2) Share in the all-India rural population located in open ended class-interval	7.11
(3) Share in the all-India total expenditure (rural) in open ended class-interval	31.75
(4) Share in the all-India expenditure (rural) on durables in open ended class-interval	53.07

K-S (p A-6) recognise it as being acute in the case of Rajasthan and Maharashtra and claim to have "adjusted the numbers to conform broadly to the temporal changes in the same state in the years immediately preceding and following 1977-78". The inequality estimates shown in their Tables 5 and 6 reflect these adjustments. The details of these adjustments are not provided by K-S. We tried to assess the impact of these adjustments by comparing the earlier (October 1989) version of the K-S paper with the revised version (February 1990) which has appeared in *EPW*. The comparison of Tables 10 to 12 in the two versions providing decomposition results reveals a curious situation. The rate of growth of real PCTE before adjustment for Maharashtra and Rajasthan were 6.2 per cent and 10.1 per cent for period I (Table 3, October 1989 version). After adjustment these growth rates become -1.0 per cent and 3.4 per cent respectively (Table 3, February 1990 version). This should have resulted in an *adverse* growth effect in Maharashtra and a reduction in growth effect for Rajasthan in the authors' decomposition exercises. However, Tables 10 to 12 are *identical* in both the versions for period I with respect to Maharashtra and Rajasthan. The growth effect continues to be favourable for Maharashtra despite a reduction in real mean PCTE over the period indicated in Table 3 of K-S. In other words, the authors have not carried over the effects of data adjustments for 1977-78 in their decomposition exercises for period I. This makes for a wrong and misleading comparison between the two periods for Maharashtra and Rajasthan.

Moreover, the authors make adjustments for the data problems only for Maharashtra and Rajasthan leaving all other states untouched.

Our adjustment at the all-India level for the data problem in the 32nd round brings down the level of per capita total expenditure by a little over 4 per cent (see [4]). Table 4 gives the relative share of Maharashtra and Rajasthan in relation to all-India (rural) for certain relevant magnitudes. The most favourable magnitude for the authors relates to the share in all-India expenditure on durables in the open ended class-interval (line 4). Even here, it should be clear that 47 per cent of adjustment is left out by the authors and this would seriously affect the estimates of the authors for other major states considered by them. In other words, for other states as well, the authors' calculation may *overstate* the growth effect for period I and consequently *understate* the growth effect for period II. This would vitiate the descriptive content of their conclusion quoted in the beginning.

With the foregoing serious data problems affecting the authors' conclusions there is not much point in undertaking the impact of appropriate state-specific price deflators on their calculations. This would amount to reworking their paper in full which is not feasible.

DECOMPOSITION AND POVERTY ALLEVIATION POLICIES AND PROGRAMMES

The authors' Section VII discusses poverty alleviation policies and programmes for the period following 1983, the end-point of their decomposition. If the authors wanted to evaluate "the effectiveness of these strategies... at the state level", they should have confined themselves to the policies and programmes for the year 1983 and relate them to their decomposition. All that the authors end up doing is to study how sensitive the inter-state allocations on poverty alleviation programmes have been to (a) the inter-state distribution of the poor and (b) within-state incidence of poverty. This is something which could have been done independently of the earlier sections of their paper. Consequently, their Section VII appears as an unconnected appendage to their earlier sections.

REGRESSIONS WITHOUT SPECIFICATIONS

The authors quote several inter-state regressions in their Sections III and IV without any discussion of the specification of the underlying relationship. It should be obvious to anybody with bare knowledge of econometrics that regression relationships do not establish causal connections, rather the causal connections have to be established on the basis of a priori economic reasoning and a careful specification of the underlying model.

Thus the authors regress state-specific growth rate of per capita consumption on (presumably) base year level of state-specific per capita consumption in their Section III. What they use, in effect, is only the degree of correlation in their discussion. Similarly, they regress growth rate of headcount ratio (for the poor/ultra-poor) on the level of (presumably) base year headcount ratio. They claim this to be a 'relationship'. They also regress growth rate of Gini index on growth rate of per capita consumption to conclude that "faster growth rate in consumption tends to introduce greater inequality" (p A-6).

Nowhere is any discussion to justify the 'relationship' on a priori economic reasoning. If the authors wanted to study the inter-state association between the variables without reference to causal connection, they could have confined themselves to the discussion of correlation coefficient only. There was no need to quote regression equations with standard errors and all.

CONCLUSION

We have argued in this note that the important conclusion in K-S Paper with far-reaching policy-implications cannot be taken to be established in view of the serious weaknesses in their arguments, inadequate treatment of data problems and insufficient analysis. The authors appear to have been

in undue haste in putting their paper in print. This is unfortunate in view of the previous reputation of the authors and social importance of the theme handled by them.

Notes

- 1 This should also be clear from the examination of authors' Table 6, 10 and 11. For example, for Bihar Gini coefficient declined at the rate of 0.2 per cent per annum between 1977-78 and 1983 (Table 6) but this had zero impact on the headcount ratio measure for the poor (Table 10) and *adverse* impact on the headcount ratio for the ultra-poor (Table 11). Similarly, a *rise* in the Gini coefficient for Tamil Nadu over the same period is associated with a *favourable* impact on the headcount ratio for the poor or the ultra-poor.
- 2 Even here, the authors do not appear to have used CPIAL covering the exact survey periods of various NSS rounds. They have possibly used the average for the financial or the agricultural year.

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