Should Poverty Measures Be Anchored to the National Accounts?

If one replaces average consumption from India's National Sample Surveys with private consumption per capita from the National Accounts, while retaining the survey-based distributions, then one finds a faster rate of poverty reduction in the 1990s. However, the case made for this method of measuring poverty is questionable on many counts. There do appear to be problems in the poverty data for India in the 1990s, but this step is unlikely to solve them.

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There has been a vigorous debate about how much India's poor have benefited from the robust economic growth since reforms began in the early 1990s. The claims from some quarters that the reforms increased poverty in the immediate post-reform period of the early 1990s are very difficult to support from the data. There was indeed evidence of a sharp increase in India's poverty measures in the aftermath of the mid-1991 crisis and the ensuing stabilisation programme. But only one-tenth of the increase in measured poverty in 1992 is explicable in terms of the variables one would expect to transmit the shock [Datt and Ravallion 1997]. Now there is more data for testing trends over time for the 1990s. Routine calculations from the National Sample Surveys (NSS) have so far suggested rather little change in the rural poverty rate up to 1997, though with indications of continuing progress against urban poverty [Datt 1999; Jha 2000]; Figure 1 reproduces the NSS-based estimates presented by Datt.

A number of observers have argued that there must be something drastically wrong with the numbers in Figure 1. Their main concern is that the consumption growth rate for the 1990s implied by the NSS is well below that for private consumption per person from the National Accounts (NAS). They see a 'solution': to replace average consumption in the NSS by the level of private consumption per capita given by the NAS. This has recently been proposed by Aiyar (2000) and Bhalla (2000a,b,c,d) amongst others.[1] Bhalla (2000c) and Srinivasan (2000) have recalculated India's poverty measures this way, and find a much higher rate of poverty reduction in the 1990s than suggested by Figure 1.[2]

This article tries to assess the case for anchoring India's poverty measures to the National Accounts. While the article is mainly concerned with measurement, one cannot ignore the fact that this topic has become intermeshed with the larger debate about India's economic reforms. The recent advocates of anchoring India's poverty measures to the NAS have clearly been worried that the NSS-based poverty numbers such as in Figure 1 will help fuel a backlash against economic reform in India. This concern is real, given the way the NSS poverty numbers have been interpreted by some observers; the heat of the policy debate has produced some strange arguments on all sides.

However, there is a risk of over-interpreting the policy implications of the 1990s poverty measures. It is plain that the apparent lack of progress in poverty reduction in the 1990s cannot be used to argue that economic growth typically bypasses the poor in India, since the data over a much longer period suggest otherwise [Ravallion and Datt 1996a,b 1999]. And even if it is true that the pace of poverty reduction is appreciably lower in the 1990s than the 1980s, this can hardly constitute an argument against the reforms, unless it can be established that poverty would have been lower if the reforms had not been attempted.[3] The relevant counter factual is not the rate of poverty reduction in the 1980s but the rate we would have seen in the 1990s without the reforms; it is questionable whether the pre-reform rate of economic growth was sustainable.[4]

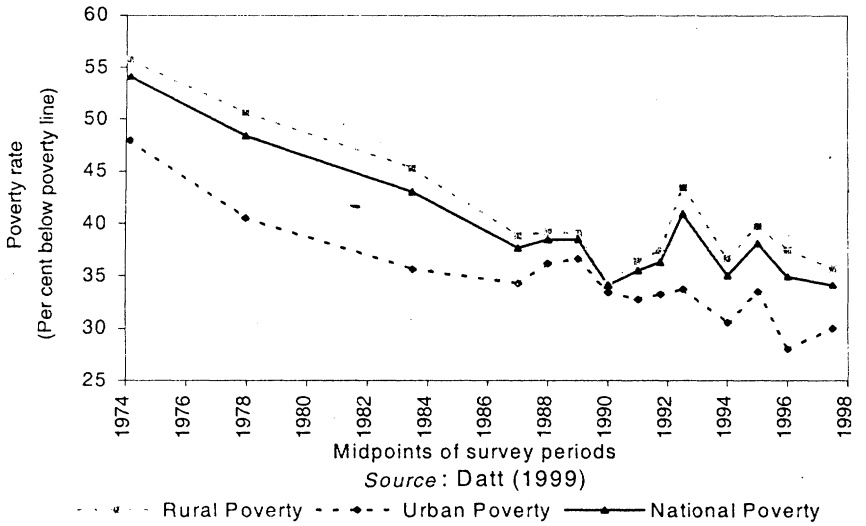
Nor is there much scope for complacency amongst the proponents of reform, even if it turns out that the pace of poverty reduction has been maintained in the 1990s. India has never been a good performer in poverty reduction by the standards of east Asia, for example. (Almost the entire region seems now to be back on track for rapid poverty reduction.) While it would be alarming if the pace of poverty reduction in India has slowed down in the 1990s, there cannot be too much cause for celebration if it hasn't.

The debate on the poverty numbers is important, but it should not deflect attention from the actions needed now to ensure that India's poor are in a good position to take advantage of the future opportunities afforded by more market-friendly policies [Ravallion 2000b]. Growth will be necessary for sustained poverty reduction in India (as elsewhere), but it is not sufficient. There are well-rehearsed reasons to question any presumption that growth-oriented reforms are all that is needed to reduce poverty. Plainly, the type of growth that is generated by economic reforms does not necessarily mean growth in the incomes of the poor. Equally plainly, while reforms can generate public and private resources for fighting poverty, they do not assure that the resources are used for that purpose.

Whichever position one takes on the reform debate, it can presumably be agreed that a credible survey instrument for monitoring consumption poverty is crucial to our knowledge about how the well-being of India's poor is evolving. The rest of this article is only concerned with the question about poverty measurement posed in the title.

Any measure of consumption poverty can be thought of as a function of average consumption and of the distribution of consumption. The following section discusses the alternative estimates of average consumption in India, while Section II

Figure 1: Incidence of Poverty in India



examines what has been happening to the distribution of consumption. Section III concludes.

I
Alternative Measures of Consumption

India's National Sample Surveys provide estimates of mean household consumption for its survey periods. India's National Accounts provide annual estimates of the private consumption component of the absorption of output and imports by the economy. The divergence between the two in the 1980s and 1990s can be seen by comparing the trend growth rates. Using the 11 NSS surveys available for this period and regressing the log of mean real consumption on time (mid-points of survey periods) the trend coefficient is 0.75 per cent per year (with a standard error of 0.19 per cent). Using instead the annual data on mean real private consumption per capita from the NAS the trend over the same period is 2.56 per cent per year (standard error of 0.14 per cent). This is a large difference. Why are the two diverging over time, and what are the implications for poverty measurement in India?

There are both methodological and conceptual differences between the two sets of numbers for 'consumption'. The two measures could hardly be more different in terms of the process that generates them. The measure of average consumption in the NSS is based on reported expenditures (cash and from own stock) in household interviews. The questions cover the consumption of some 700 items (in the 1994-95 questionnaire), spanning the whole range of commodities.

By contrast, households are essentially the 'residual claimants' in the NAs [Ruggles and Ruggles 1986]. One first estimates aggregate output for each commodity group; naturally there are coverage and measurement issues in doing so [Srinivasan 2000]. After adding imports, one then tries to account for domestic absorption by firms and governments (the increase in inventories held by firms as well as their purchases and those by the government). The remainder is then called the 'private consumption' of that commodity, and it lumps the errors in all other components together, with no obvious reason to think they cancel out.

There are also a number of conceptual differences between the two measures.[5] Indeed, the private consumption numbers in the NAS do not measure *household* consumption as such. For example, in India and most other developing countries, it is not feasible to separate the expenditures of non-profit enterprises in the economy (NGOs, charities, religious organisations, political parties and so on) from those of households in compiling the National Accounts.[6] So replacing the NSS mean with consumption per capita from the NAS when measuring poverty would imply that campaign spending by politicians trying to get elected would automatically reduce measured poverty even if none of the money goes to the poor. And every rupee spent by an NGO would reduce measured poverty, even if none of the money went to the poor.

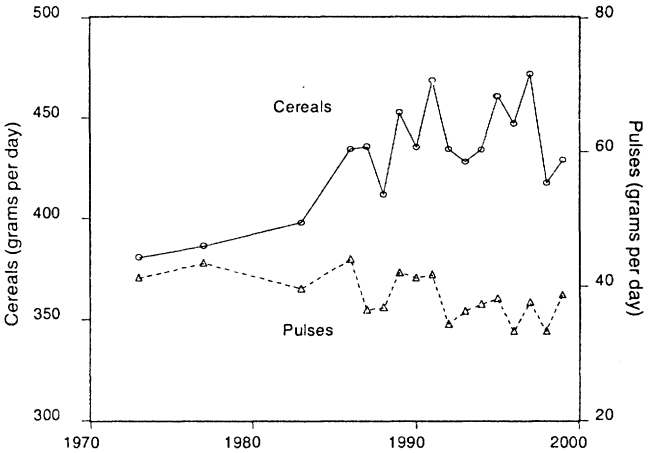
Given the differences in coverage and methods, it can hardly be surprising that the two measures of aggregate 'consumption' do not agree. What is more alarming is the extent of the difference. To eliminate the effect of using different deflators, Datt (1999) reports that *nominal* NSS consumption rose by 198 per cent between 1990-91 and 1997, while NAS consumption rose by 233 per cent over the same period. Since NAS consumption has grown more than NSS consumption, the growth rate of the difference between the two must obviously exceed that of NAS consumption. World Bank (2000) reports that NSS consumption accounted for about 66 per cent of NSS consumption in 1997, implying that it must have accounted for 74 per cent in 1990-91. So the nominal value of the difference between the two must have risen by 333 per cent between 1991 and 1997.

It seems plausible that there has been substantial growth in spending by non-profit enterprises, though I have not seen any evidence on this. The problems of collecting data on this sector are presumably why it has not been separated from the household sector in India's NAS (or for most other developing countries). However, there can be very little doubt that other factors are at work, including non-response and/or underreporting of household consumption in the NSS.

It does not take much for the NSS to underestimate consumption. All it takes is for well-off sampled households to systematically refuse to participate in the survey, and be replaced by more compliant but less well-off ones, or for interview respondents to forget, or prefer not to reveal, items of consumption in the survey schedule. Like other high-quality surveys (by international standards), the NSS tries to avoid such problems, but that is not easy.

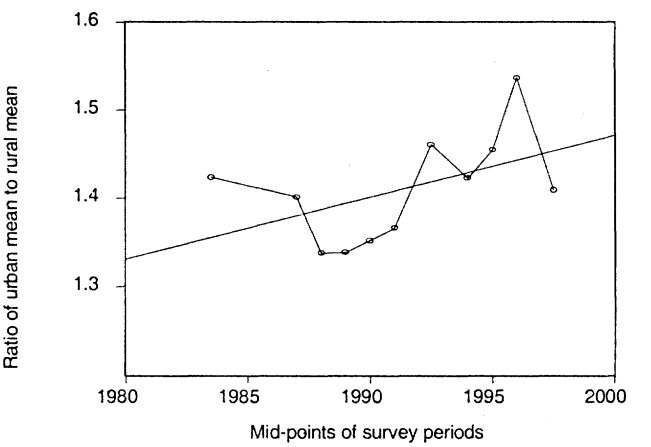
How much of the consumption growth in India is being missed by the NSS? There is no way of conclusively answering this question, but a few simple calculations are suggestive. By definition, the increase in NAS consumption in any period equals the increase in the true value of household consumption plus the increase in spending by non-profit enterprises and errors accumulated from measuring national output, imports and other components of domestic absorption. Let us assume that the growth rate in the 'non-household' component is the same as that of NAS consumption as a whole (in other words the share of total consumption accountable to the non-profit enterprises and accounting errors did not change). Furthermore, let us assume that in 1990-91 the spending by non-profits and errors accounted for 10 per cent of total (NAS) consumption (this is a wild guess, but we will see how sensitive the calculations are to getting it wrong). Then it is readily verified that the NSS must be capturing 70 per cent of the increment to the true value of household consumption, i e, 30 per cent of the gain in consumption by households between 1990-91 and 1997 has been missed. This calculation depends of course on its assumptions. If the non-household share of NAS consumption is

Figure 2: Availability of Cereals and Pulses



Source: Bhalla (2000c, Table 3).

Figure 3: Ratio of Urban Mean Consumption to Rural Mean



Source: Datt (1999).

15 per cent (5 per cent) then the share of the increment to household consumption that is being captured by the NSS is 74 per cent (66 per cent). Clearly, if the share of the non-household component of total NAS consumption is rising then the extent of underestimation by the NSS will be lower.

Validation against Other Data Sources

Data on food availability have been used by Bhalla (2000c) to argue that the NSS is underestimating the rate of poverty reduction.[7] He quotes numbers indicating a 10 per cent increase in per capita cereal 'consumption' (it is essentially production less exports) between 1983 and 1998 – years in which the official poverty rates are 'the same' (45 per cent in 1983 and 42 per cent in 1998). This leads him to infer that "...something is drastically wrong in the NSS surveys' [Bhalla 2000c: 9].

Is this right? Figure 2 plots the data for both pulses and cereals. It can be seen that almost all of the increase in per capita cereal availability that Bhalla points to was in the 1980s; there has been no trend increase since then. Indeed, the trend for the 1990s in cereal availability per capita is negative, though not significantly different from zero. (The regression coefficient on time for the 1990s is -0.88 grams per day, with a standard error of 2.12.) The NSS-based poverty measures also show a decrease in the 1980s; that is not at issue. However, these data cannot be used to support a claim that the more modest reduction in poverty for the 1990s evident in Figure 1 is inconsistent with the trends in the per capita availability of food staples.

An alternative source of household survey data for India is the Market Information Survey of Households (MISH) done by the National Council of Applied Economic Research (NCAER). This survey does not include food consumption, so one cannot create consumption-poverty measures comparable to those from the NSS, though MISH does ask about spending on selected non-food items. The survey also has a short income question, in which the respondent is asked to indicate to which of five income classes her family belongs [Natarajan 1998]. Naturally this is a difficult question to answer, and it is far from clear that the answers would be consistent or accurate, given the ambiguity in what 'income' means (not least in rural areas of a developing country) and the influence of subjective factors and respondent knowledge of family income. MISH has a large sample though (about 3,00,000 households). Estimating income accurately is considered problematic even in surveys that identify a very wide range of income components using hundreds of quantitative questions rather than just one (essentially qualitative) question.[8]

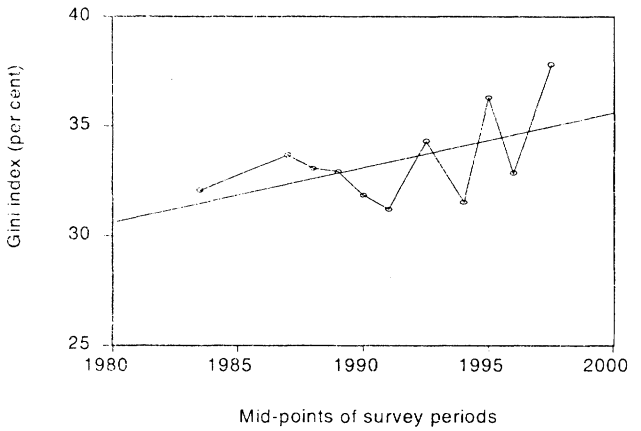
Lal, Natarajan and Mohan (2000) compare a series of poverty measures from MISH using a poverty line that gives the same poverty rate as the Planning Commission's 1987-88 estimate based on the NSS.[9] Using the same deflators as the Planning Commission, Lal et al, report a decline in the poverty rate from 39 per cent in 1987-88 to 26 per cent in 1997-98 – clearly a steeper decline than indicated in Figure 1. (They confirm the tendency for urban poverty incidence to fall faster than rural poverty.)

Table: National Gini Index and Decile Shares of Consumption in India in 1980s and 1990s

NSS Survey Period	Gini Index (Per Cent)	Decile by Share of Total Spending (Per Cent)									
		Decile 1 (Poorest)	2	3	4	5	6	7	8	9	Decile 10 (Richest)
January 1983-December 1983	32.06	3.69	4.81	5.78	6.72	7.69	8.73	9.99	11.67	14.48	26.44
July 1986-June 1987	33.68	3.58	4.65	5.59	6.53	7.48	8.54	9.81	11.52	14.4	27.9
July 1987-June 1988	33.08	3.83	4.8	5.69	6.55	7.47	8.49	9.7	11.36	14.18	27.93
July 1988-June 1989	32.93	3.87	4.79	5.67	6.54	7.45	8.48	9.74	11.46	14.36	27.64
July 1989-June 1990	31.84	3.83	5.01	5.87	6.7	7.61	8.64	9.87	11.53	14.2	26.74
July 1990-June 1991	31.21	3.91	4.94	5.89	6.79	7.72	8.74	9.97	11.62	14.38	26.04
January 1992-December 1992	34.31	3.63	4.73	5.56	6.4	7.3	8.36	9.63	11.38	14.24	28.77
July 1993-June 1994	31.52	3.96	4.94	5.84	6.72	7.64	8.66	9.89	11.56	14.36	26.43
July 1994-June 1995	36.32	3.28	4.24	5.17	6.13	7.14	8.31	9.73	11.69	15.04	29.27
July 1995-June 1996	32.86	3.88	4.98	5.79	6.57	7.43	8.41	9.62	11.25	13.97	28.1
January 1997-December 1997	37.83	3.54	4.52	5.39	6.2	7.03	7.92	8.97	10.36	12.6	33.47

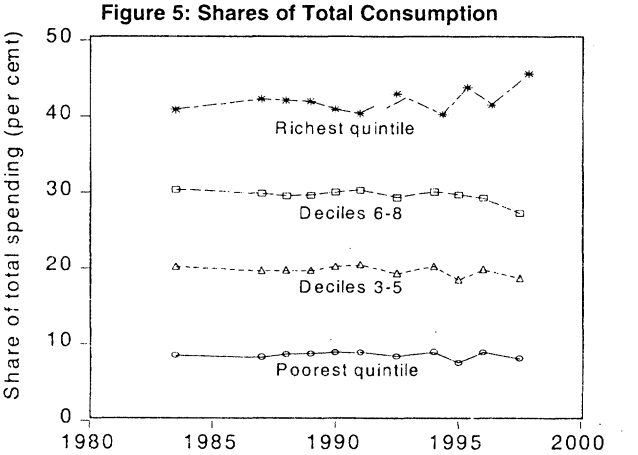
Note: Author's estimates from NSS data. Deciles are constructed to have an equal number of people in each (as close as possible), and the ranking variable is household expenditure per person. Interpolation and Gini index based on parameterised Lorenz curves (based on general elliptical or incomplete Beta functions, whichever fitted best.)

Figure 4: National Gini Index of Consumption Inequality



Source: Table.

Figure 5: Shares of Total Consumption



Source: Table.

Lal et al attribute their higher rate of poverty reduction in the 1990s to underestimation of consumption in the NSS, although they do not offer much insight into why this might be happening. Supportive evidence for their conclusion that the NSS is underestimating consumption is found in their comparisons of the MISH estimates of expenditures on selected non-food goods. For example, they show that the MISH estimates for expenditures on television sets, tape recorders, electric fans and bicycles are considerably higher for 1993-94 than indicated by the NSS. And the MISH estimates accord closely with production data for that year.

Proponents of anchoring the poverty measures to the National Accounts are clearly not wrong in arguing that there is underestimation of consumption in the NSS. It remains unclear why the problem appears to have become more severe in the 1990s.

Consumption underestimation in the aggregate is not in contention here, though it needs to be better understood and steps taken to correct it; that will require effort by the National Sample Survey Organisation. The real issue here is not whether the NSS is underestimating aggregate consumption, but what is happening to distribution. The next section turns to this issue.

II
Measured Inequality in NSS

It has been argued by Aiyar (2000), Bhalla (2000a,c) and others that inequality has been stable over time in India. This has led them to conclude that poverty must have fallen, given the consumption growth indicated by the NAS. As Bhalla (2000c) puts it: "Economic growth has occurred, distribution of per capita consumption has not changed...By definition, therefore, the absolute levels of income of the poor have increased, and therefore, the poverty level must have declined, and declined by an enormous amount." There are two key assumptions in this argument: Firstly that distribution is not changing, as indicated by the NSS, and secondly that the NSS is getting the distribution right. Let us look at these assumptions in turn.

Datt (1999) provides estimates of the Gini index of household expenditure per person from the NSS for the 1973-97 for both urban and rural areas. The numbers indicate fluctuations in inequality, but no significant trend in either rural or urban areas. Datt does not estimate the aggregate (national) Gini index, however. And it is constancy of the *national* distribution that is being assumed by the advocates of anchoring poverty measurement in India to the NAS.

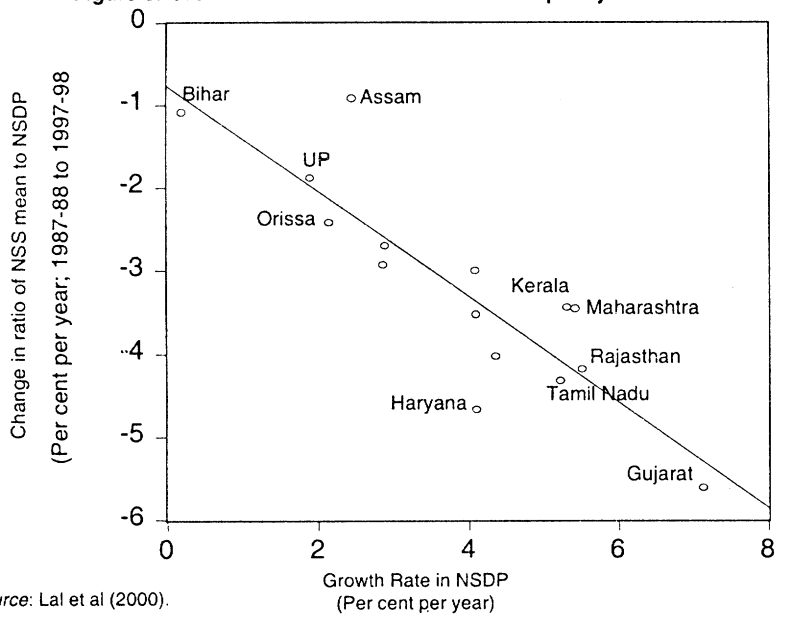
There is evidence of rising consumption inequality *between* urban and rural areas in the 1990s. Figure 3 plots the ratio of the means using all the (full-year) surveys for the 1980s and 1990s. While there is no significant trend for the period as a whole, the data from 1988 onwards indicate a positive trend; the regression coefficient on time is 0.0156 and is significant at the 4 per cent level.[10] (Throughout this article, all significance tests are based on White standard errors corrected for heteroskedasticity). Naturally this would put upward pressure on overall national inequality in the 1990s, given that mean urban consumption is on average about 40 per cent higher than rural consumption.

Is inequality rising nationally? The Table gives my estimates of the national Gini index.[11] Figure 4 plots the index against the mid-points of survey rounds. There are large fluctuations around a rising trend. If one calculates the simple average of the national Gini indicies for the first three survey rounds of the series one gets 32.94 per cent. For the last three rounds, the figure is 35.67 per cent. The annualised difference is 0.27 percentage points per year. This is quite close to the estimated regression coefficient of the Gini index on the mid-points of NSS rounds; the regression coefficient is 0.25, though this is only significant at the 11 per cent level. However, there is strong (negative) serial dependence in the residuals; when the Gini index is above trend in one survey round, it tends to be below trend in the next. With a correction for the serial dependence in the residuals the trend coefficient rises to 0.29, and becomes significant at the 1 per cent level.[12] If this trend continues, the Gini index will rise from about 36 per cent in 1995-97 to 39 per cent in 2005-7.

The Gini index is a summary statistic. A rising Gini index could happen in any number of ways. The table presents estimates of the shares of spending in India by deciles of the population (so that there is an equal number of people in each decile), ranked by household expenditure per person. Figure 5 helps interpret these numbers by plotting how the shares of various composite expenditure groups have evolved in the 1980s and 1990s. I divided the population into four groups as described in the figure.

One finds that the share of the poorest 20 per cent has remained quite flat over time, though dipping noticeably in 1994-95. The trend (regression coefficient on time) is not significantly different from zero over the whole period. The pattern is quite similar for the next poorest group (deciles 3-5), for whom we also see fluctuations, though around a slight downward trend in the 1990s. Over the whole period, the trend in the share for deciles 3-5 is –0.091 percentage points per year, and this is significant at the 1 per cent level after

Figure 6: Growth Rates in the NSS-NAS Discrepancy and Income



Source: Lal et al (2000).

the correction for serial dependence in the residuals.[13]

The gainers have clearly been the richest quintile; for them the trend over the period as a whole is 0.227 percentage points per year, which is significant at the 8 per cent level (there is no serial correlation in this case). Just focusing on the seven observations for the 1990s, the trend coefficient for the upper quintile rises sharply to 0.620, which is significant at the 1 per cent level (3 per cent if one does not correct for serial correlation in the residuals).

From the point of view of the national poverty rate, one should focus on deciles four and five in the table (corresponding to poverty rates in the region 30-50 per cent). For this segment of the distribution, the trend decline is -0.063 and significant at the 1 per cent level after correcting for the serial correlation in residuals. If one focuses just on the surveys spanning the 1990s, the trend coefficient doubles, to -0.135 (significant at the 2 per cent level).

The above calculations are at best suggestive. There are a number of problems in the data; for example, geographic differences in how prices are changing have been ignored, as have differences in how changes in relative prices might have affected different expenditure groups. However, the results in the table do not offer much support for the first assumption made by proponents of anchoring poverty measurement to the NAS – that distribution is not changing. Indeed, there are signs in the NSS data of rising inequality during the 1990s. Of particular note is that the critical segment of the distribution from the point of view of the poverty rate have been seeing a statistically significant trend decline in its share of total spending, and markedly so in the 1990s.

These signs of rising inequality are not confirmed by the MISH-based estimates of the income Gini index reported by Lal et al (2000). Similarly to the NSS-based estimates reported by Datt (1999), there is no sign of rising inequality within either urban or rural areas in the MISH-based estimates. (Surprisingly, the MISH estimates indicate higher inequality in rural areas than urban areas – in marked contrast to the NSS and urban-rural inequality comparisons in most developing countries.) However, Lal et al, do not find evidence of rising income inequality nationally. They do not report mean incomes for urban and rural areas, though from their results it seems unlikely that their data would give the signs of a rising urban, mean relative to the rural mean, found in the NSS data for the 1990s using the same deflators (Figure 2).

Further research is needed to better understand the differences between the MISH and NSS data for the 1990s. Unlike the NSS data, the MISH data do not appear to have been the subject of independent research (outside NCAER). So the strengths and weaknesses of the data are not as well known as for the NSS. The fact that the income data are from a single question, with just five class intervals, cannot leave one very confident about the results. The measure of inequality one eventually extracts from such data will depend (in a largely unknown way) on how respondents interpret what 'income' means, how the boundaries of the class intervals are chosen, and the way the responses are aggregated when processing the data. It may well be more reasonable to interpret this as a measure of subjective economic welfare than as a measure of 'income'.

Distribution-Neutral Errors in NSS?

Proponents of replacing the NSS mean with average consumption from the NAS are assuming that the NSS underestimates consumption by a constant proportion across all levels. Thus it is argued that inequality in the NSS is measured correctly; all we need to do is correct the mean.

Notice that this distribution-neutrality assumption must be imposed geographically, such as between states and between urban and rural areas. For example, consider the standard urban-rural poverty profile by state. To reproduce this poverty profile under the proposed method of anchoring poverty measures to the NAS one would have to scale up both the urban and rural means in all states by the same number, given by the ratio of the NAS consumption mean to the NSS mean nationally. For consistent aggregation, the proportionate under-reporting in the NSS must be assumed to be the same everywhere.

A number of aspects of this distribution-neutrality assumption are worrying. It is agreed that mean consumption from the NSS has been growing at a lower rate over time than consumption per capita from the NAS. If one believes that the NAS consumption numbers accurately measure mean household consumption (though this is questionable, as I have already argued), then the assumption of a constant rate of underestimation clearly does not hold over time. Proponents of anchoring poverty measures to the NAS argue that on the one hand the rate of underestimation is roughly constant between people at one date but on the other hand it has risen over time with growth in mean consumption from the NAS.

Nor is the distribution-neutrality assumption easy to reconcile with the finding reported in Lal et al (2000) of a negative correlation across states in the growth rate of net state domestic product (NSDP) and the change in the ratio of NSS consumption to NSDP, both measured over the period 1987-88 to 1997-98. Figure 6 plots the two growth rates. It is clear that the rising discrepancy that has emerged between the NAS and the NSS has been associated with higher growth. One sees much less sign of this rising divergence in the low growth states such as Bihar, Uttar Pradesh, Orissa and Assam. The high growth states in the 1990s (such as Gujarat, Mararashtra, Rajasthan and Tamil Nadu) appear to account for the bulk of the overall NSS-NAS discrepancy.

Figure 6 is hardly what one would expect to find if the NSS-NAS divergence is distribution-neutral. Regressing the percentage change in the ratio of NSS consumption to NSDP across states on the growth rate in NSDP one obtains a regression coefficient of -0.636 (with a standard error of 0.071), implying that the elasticity of NSS consumption to NSDP is 0.364, which is significantly positive, but also significantly less than the value one would expect with distribution-neutrality.

One can question the internal consistency of assuming that consumption underestimation has increased with growth yet the rate of underestimation is no higher for the rich than the poor. Indeed, one might expect the underlying conditions that (apparently) lead to greater underestimation of consumption as its mean grows to also entail higher underreporting by the rich than the people at any one date. The more consistent interpretation would seem to be that the rate of underestimation (error as a share of consumption) rises as consumption rises, implying that inequality is underestimated.

None of this denies that the NSS is missing a share of the gains to household consumption in the 1990s. It is generally thought that conventional household surveys are not very accurate in measuring the incomes and consumptions of the relatively rich. Unsurprisingly, this is not something about which there is much evidence. However, we can be reasonably confident that there is non-compliance (people refusing to participate in the survey) and probably some underreporting amongst those who do participate. This is unlikely to be confined solely to the rich – it is probably found at all levels of living. On a priori grounds it does not seem plausible, however, that this problem would be just as great for the poor as the non-poor. Some of the poor might underreport or refuse to participate, but by and large they would not have any reason to do so, and there may well be just as much overestimation of their incomes and expenditures. And the poor tend to consume things that are less difficult to measure than the things rich people consume.

With more rapid growth, we may well see a rise in non-compliance and underreporting, and hence a larger drift between the survey mean and the consumption component of the NAS. However, it is by no means obvious that this will be distribution neutral. Indeed, it is quite possible that the bulk of the problem is above the poverty line. Participating in a survey such as the NSS takes time, and compliance amongst the non-poor can be expected to drop off when the opportunity cost of their participation is rising. It is no doubt tempting for the interviewer to switch to a less well off but more cooperative household. Those who still participate might also be disinclined to report their new purchases of luxury goods to the interviewer from the government's statistics office. They do not want to flaunt their newfound wealth.

III
Conclusions

There are a number of flaws in the arguments that have been made for anchoring poverty measurement to the National Accounts. Given the way consumption is measured in the NAS, one can hardly be confident that it gives an accurate measure of either the level of average household consumption or its rate of growth. The difference between the NAS and NSS consumption numbers reflects in part measurement errors in the former and the fact that the spending of the (apparently growing) non-profit sector cannot be separated from household consumption when accounting for domestic absorption of measured output in the NAS.

The assumptions made about distribution by advocates of NAS anchoring are also questionable. For one thing – in marked contrast to the claims made by advocates of this change in methods of measuring poverty – there is compelling evidence of rising consumption inequality in the NSS data for 1983-97. This is driven in part by a rising share of consumption going to the richest quintile, though not at the expense of the share going to the poorest quintile. The crucial segment of the distribution from the point of view of the poverty rate in India (for poverty rates in the range 30-50 per cent) has seen a statistically significant trend decline in its share of total spending over the 1980s and 1990s. Many people seem to have misread the signals on distribution because they have looked at inequality within rural areas, and within urban areas, ignoring what has been happening *between* the two sectors; however, the intersector disparity in mean consumption has been rising significantly in the 1990s.

Nonetheless, if one assumes that the NAS gives us a reliable estimate of the growth rate of household consumption, and that the NSS gets distribution right, then there has clearly not been sufficient increase in consumption inequality in India in the 1990s to eliminate the gains to the poor from economic growth. But it must be acknowledged that those are pretty big assumptions about the nature of the errors in the data. While it is entirely possible that the NSS is underestimating consumption growth, there is no obvious reason for assuming that such a bias is distribution neutral. That is not easily reconciled with the very strong correlation across states between the rate of increase in the discrepancy between NSS and state level income per capita and the rate of growth in the latter. At an aggregate level, the claimed underestimation of consumption in the NSS would appear to rise as income increases. So it is quite possible – and certainly no less plausible than the distribution-neutrality assumption – that the underestimation of consumption growth is largely for the non-poor. The bulk of the bias is then in measured inequality not poverty. This is a conjecture; naturally this is something about which data are scarce. However, the key point is that there is no basis for assuming that the errors in the NSS data are distribution-neutral.

In truth, we do not know yet why India's post-reform economic growth has not shown up more in the consumption numbers from National Sample Survey data. Careful data work will be needed to help figure out why. Replicating past research on the discrepancies in mean consumption between the two sources for India would be a good starting point (see the papers on this topic in the 1974 Srinivasan and Bardhan volume, *Poverty and Income Distribution in India*). There has also been research comparing these two sources of consumption data for the US, where similar discrepancies are found between the National Accounts and survey-based estimates [Triplett 1997; Slesnick 1998]. A comparable breakdown by components could be revealing (though this requires care in matching the different categories used in the two data sources; see Branch (1994), on US data). Comparisons with other survey data are also useful; although the NCAER's MISH is a much lighter survey instrument (notably in how it measures household incomes) and has not been subjected to the same close scrutiny by researchers as the NSS, it is notable that MISH does not appear to confirm the stagnation of poverty rates in the 1990s that the NSS-based estimates suggest [Lal et al 2000].

There is no cause for complacency about existing survey-based poverty and inequality measures for India. The concerns about underestimation of consumption by the NSS are real. Legitimate concerns exist about various aspects of NSS survey design [such as the recall periods used for consumption; see Visaria 2000]. Concerns have also been raised about the deflators routinely used to update the poverty lines at least until the mid-1990s [Deaton and Tarozzi 1999]. With greater use of the micro-data from the NSS by researchers and policy-makers we can expect useful

feedback on how the survey might be improved. Careful research on both the NSS and NAS data might also suggest ways of combining the two data sources and of reducing errors in both. However, mechanically replacing average consumption from the survey data with consumption per capita from the National Accounts will not provide more credible poverty measures for India. EPW

Notes

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1 Note that two of these references to Bhalla are unpublished papers, and traditionally one does not comment on unpublished papers. However, Bhallas published newspaper articles (including Bhalla, 2000 a, d) and public presentations (such as that summarised in http://www.icrier.res.in/public/ashoka10dec.html) have drawn heavily on these unpublished papers. And these days, an unpublished paper can be seen just as much, and have as much influence, as a published one. Furthermore, the fact that almost all of his own past papers that he refers to in Bhalla (2000 b, c) are unpublished (going back 20 years) suggests that it may be a long time before published versions of Bhalla (2000 b, c) appear.

2 India's Planning Commission also used this method for a short period, though it was abandoned after the deliberations of its Expert Group [Planning Commission 1993].

3 Critical discussions of the impacts of reform on poverty in India include Sen (1996) and Ghosh (2000). Impacts through the relative price of food have been a prominent theme in this critique; for further discussion see Ravallion (2000a).

4 For further discussion see Joshi and Little (1996). Also see the discussions in Jha (2000) and Srinivasan (2000).

5 This has been well recognised in the early scholarly literature for India. See for example, Srinivasan et al (1974). For detailed discussions of the conceptual and practical differences see Ruggles and Ruggles (1986), Triplett (1997) and Slesnick (1998). Also see the comments in Attansio (1999).

6 While the 1993 System of National Accounts recommends separating out the expenditures of non-profit organisations, the data necessary to do so have not permitted this for India [Kulshreshthra 1998]. This appears to be a common problem.

7 Bhalla's (2000c) calculations of what growth rates in total consumer spending would be consistent with the data on growth rates in quantities consumed, given assumed demand elasticities, are naturally sensitive to those assumptions; it is readily verified that one can get quite different results under different assumptions.

8 On estimating household incomes see the discussion in Deaton (1997).

9 It is not clear how the authors estimated the income poverty rate from their data on which of the five income classes the respondent indicated. There is more than one way that can be done, and the choice can have non-negligible impact on the estimates.

10 There is also evidence of serial dependence in the residuals around the regression line in Figure 3. If one corrects for this (I used a first-order MA process) the positive trend after 1988 becomes 0.0175 and is significant at the 1 per cent level.

11 The national Gini index cannot be calculated from the urban and rural Gini indices (such as Given in Datt 1999). Instead one has to calculate the national Lorenz curve from the primary data on urban and rural distributions of consumption, after aggregating the distributions consistently (so that the same expenditure level is treated the same way). The Gini index is then calculated from the national Lorenz curve, by numerical integration of the fitted Lorenz curves. This is how the numbers in the table were obtained.

12 To correct for the serial dependence, I used a moving average process of order one for the error term; a nested test of an MA(1) correction against an AR(1) showed that the former was far preferable. The MA(1) coefficient was -0.92 with a standard error of 0.061. This implies a first-order serial correlation coefficient of -0.50. This correction ignores the uneven spacing of the surveys.

13 Without this correction the trend is about the same but is only significant at the 6 per cent level.

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