

Adjusted Indian Poverty Estimates for 1999-2000

This paper explains a method that can be used to adjust the NSS 55th Round poverty estimates so as to make them comparable with earlier official estimates. After presenting the adjusted head-count ratios for all-India and each of the large states, for both urban and rural sectors, the author turns to some broader issues about poverty monitoring in India, including those raised by the non-comparability of estimates that is his main topic but looking further to issues of future survey design and the choice of poverty lines.

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I

Introduction: Poverty and Poverty Debates in the 1990s

What has happened to Indian poverty in the 1990s has been hotly debated. After the economic reforms in the early 1990s, there was a historically rapid rate of growth in GDP per head but, until the publication of estimates from the 55th Round of the National Sample Survey (NSS) for 1999-2000, little apparent reduction in the fraction of the population in poverty. Official poverty estimates in India are based on large household surveys of consumption carried out by the NSS approximately every fifth year. The 50th Round survey, carried out in 1993-94, produced poverty rates that were only slightly lower than the previous quinquennial survey, the 43rd, carried out in 1987-88. After 1994, there were four 'thin' survey rounds, which have smaller samples and which are not primarily designed to collect household consumption data, and these also showed little if any evidence of a reduction in poverty up to the middle of 1998. But there have always been some doubts about the reliability of these surveys; not only are they smaller, and therefore less precise, but their sample design differs from that of the quinquennial rounds, so there remain questions about their reliability as guides to the evolution of consumers' expenditure and poverty.

In consequence, the results of the 55th Round quinquennial survey of 1999-2000 were eagerly awaited. The estimates, published in February 2001, showed a marked reduction in the fractions of people in poverty. Among rural households, the fraction estimated to be in poverty fell to 27.1 per cent in 1999-2000, compared with 37.3 per cent in 1993-94, while among urban households, the fractions were 23.6

per cent in 1999-2000, compared with 32.4 per cent in 1993-94. However, because the design of the 55th Round questionnaire was different from that in earlier rounds, the comparability of these new estimates has been challenged, [see for example Sen 2000].

In this paper, I explain a method that can be used to adjust the 55th Round poverty estimates so as to make them comparable with earlier official estimates. This method is only as good as its assumptions which are plausible, but not necessarily correct. Given that the 55th Round is not comparable with earlier rounds, some assumptions are needed to make progress at all. As I shall show, my estimates suggest that much, if not all, of the official fall in poverty is real. Indeed, there are some basic facts of the 55th Round which were not compromised by the survey design, and which make it clear that there has been a substantial improvement in levels of living since the 50th Round in 1993-94.

The paper is organised as follows. Section II explains the source of the non-comparability of the 55th Round estimates. Section III explains the basis for my adjustments. Inevitably, there are some formulas, but I have tried to keep them to a minimum, and I explain what each of them means. Section IV contains the results, and includes adjusted head-count ratios for all India and each of the large states, for both urban and rural sectors. These estimates are a first cut at the issue, and should not be treated as the best estimates that are currently available. My preferred estimates of poverty and inequality in India in 1999-2000, together with comparable estimates for earlier years, are given in Deaton and Drèze (2002). Section V turns to some broader issues about poverty monitoring in India, including those raised by the incomparability that is my main topic, but looking further to issues

of future survey design, and to the choice of poverty lines.

I

Survey Design and Non-Comparability

The comparability problems came about as follows. In the 51st through 54th (thin) rounds, the NSS experimented with the recall periods over which respondents were asked to report their consumption. NSS consumption surveys have traditionally used a 30-day recall period for all goods, a decision that was based on some experiments in the early 1950s [Mahalanobis and Sen 1954]. Most statistical offices around the world use a shorter recall period for high frequency items, such as food, and longer recall period for low frequency items, such as large durable goods. The NSS experiments in the 51st through 54th Rounds compared a traditional 30-day recall questionnaire (Schedule 1) with an experimental questionnaire with three reporting periods, 7-, 30-, and 365-days, applied to different classes of goods (Schedule 2). Households were randomly assigned to one or other schedule, and it was found that, on average, the experimental 7/30/365 Schedule generated more reported total expenditures. This effect was large enough to cut estimated poverty rates by approximately a half when the experimental schedule was used in place of the traditional schedule [Visaria 2000]. Shorter reporting periods typically generated higher rates of consumption flow, so that the seven-day recall in Schedule 2 produced higher average consumption than the 30-day recall in Schedule 1, while the 365-day recall in Schedule 2 produced lower average consumption. However, the 365-day recall also has the consequence of pulling up the bottom tail of the distribution of expenditures on these infre-

quently purchased items, such as durables, clothing, or hospital expenditures, and many fewer Schedule 2 than Schedule 1 households report no purchases of these items over the reporting period.

The 55th Round differed both from earlier rounds and from either of the Schedules in the experimental rounds. For the high frequency items, households were asked to report their expenditures for both recall periods. The questionnaires were printed with the list of goods down the leftmost column, with the next four columns requesting quantities and expenditures over the last seven days and over the last 30 days respectively. Such multiple reporting periods are often used in household expenditure surveys, and may well produce excellent estimates in their own right. But the results are unlikely to be comparable with those from a questionnaire in which only the 30-day questions are used. For example, when they are asked both questions, respondents are effectively being prompted to reconcile the rates of consumption across the two periods. Indeed, there is some evidence that is consistent with this sort of reconciliation. In the 51st through 54th Rounds, where different households were assigned one or other of the two schedules, the ratio of mean per capita expenditure in Schedule 2 to mean per capita expenditure in Schedule 1 lay between 1.13 and 1.18 for both urban and rural sectors in all four rounds. Yet in the 55th Round, the ratio of the two measures of per capita expenditure fell to 1.04 among rural and 1.03 among urban households. This was in spite of the fact that the low frequency items were asked only at the 365-day reporting period, which should have reduced the Schedule 1 estimates and further inflated the ratio of the Schedule 2 to Schedule 1 totals.

Although we have no way of knowing exactly what happened, one reasonable hypothesis is that the immediate juxtaposition of the two schedules prompted households to reconcile their two reports, pulling up the rate of consumption at 30-day recall above what it would have been if asked in isolation, and pulling down the rate of consumption at 7-day recall above what it would have been if asked in isolation. If so, the 30-day estimates of consumption from the 55th Round are too high compared with the 30-day estimates of consumption from earlier large rounds, particularly the 50th, and the reduction in poverty is overstated. Given the very large drop in the head-count ratios, this is a plausible story. The 7-day estimates cannot be used to repair the poverty estimates because there are no 7-day estimates from

earlier large rounds. The best that can be done is to compare the 55th with the immediately preceding thin rounds, a procedure that shows an increase in poverty in 1999-2000 compared with the period from mid-1994 through mid-1998.

II Adjusting the Poverty Estimates

This section outlines a procedure for adjusting the poverty estimates from the 55th Round to make them comparable with earlier large rounds, particularly the 50th. Because the new survey does not contain all the information that is needed to make it fully comparable, the method, like any effective procedure, rests on a number of assumptions. I shall provide some evidence to suggest that these assumptions are plausible, but they cannot be fully tested without the information that, if it existed, would obviate the need for them.

The key idea is that there are a group of goods for which the questionnaire is the same across all rounds. There are six broad categories, fuel and light, miscellaneous goods, miscellaneous services, non-institutional medical services, rent, and consumer cesses and taxes. These items have always been asked using the 30-day reporting period. The first four are important items, and expenditures on the first three are reported by virtually all households. Non-institutional medical expenditures are also important on average, with a mean that is comparable in size to expenditures on miscellaneous goods or expenditures on miscellaneous services, but they are incurred by less than half of households over a 30-day period. Taken together, expenditures on the six

broad categories account for more than 20 per cent of all expenditures, and more in urban areas. Total expenditure on these '30-day' goods is also highly correlated with total household expenditure; in the 50th Round, the correlation between the logarithm of total household per capita expenditure and the logarithm of per capita expenditure on these 30-day goods is 0.79 and 0.86 in the rural and urban sectors respectively. I can therefore use expenditures on these comparably surveyed goods to get an idea of trends in total expenditures, and therefore, of trends in poverty.

Rather than estimating per capita expenditure as a first stage and then going on to estimate poverty, I use a more direct procedure. Denote the logarithm of household total expenditure per head by x , and the logarithm of total expenditure per head on 30-day goods by m . The logarithm of the poverty line is written z , and everything is measured in constant price rupees. If the head-count ratio is denoted by P , I can write $P = F(z)$, ... (1) where $F(\cdot)$ is the cumulative distribution function of the logarithm of per capita expenditure (pce). $F(z)$ is simply the fraction of people who live in households with a logarithm of pce less than the logarithm of the poverty line, or just the fraction of people who live in households with pce less than the poverty line.

We are interested in using the amount of m to predict the level of poverty. Consider then the probability of being poor conditional on spending m on 30-day goods, $F(z|m)$. I can rewrite equation (1) as

$$P = \int_0^{\infty} F(z|m)g(m)dm = E_m [F(z|m)] \dots (2)$$

where $g(m)$ is the density function of the

Table 1: Headcount Poverty Ratios
(Rural India Per Cent)

	50th Round		55th Round		55th Round	
	Official	Estimate	Official	Estimate	Adjusted	St Error
Andhra Pradesh	15.9	15.9	11.1	10.5	14.9	0.64
Assam	45.0	45.2	40.0	40.3	44.1	2.05
Bihar	58.2	58.0	44.3	44.0	49.2	1.18
Gujarat	22.2	22.2	13.2	12.4	15.4	1.17
Haryana	28.0	28.3	8.3	7.4	12.7	1.06
Himachal Pradesh	30.3	30.4	7.9	7.5	18.9	1.32
Karnataka	29.9	30.1	17.4	19.8	25.7	1.59
Kerala	25.8	25.4	9.4	9.4	12.6	0.67
Madhya Pradesh	40.8	40.7	37.1	37.3	36.4	1.47
Maharashtra	37.9	37.9	23.7	23.2	29.2	1.43
Orissa	49.7	49.8	48.0	47.8	47.3	1.75
Punjab	12.0	11.7	6.4	6.0	5.9	0.43
Rajasthan	26.5	26.4	13.7	13.5	19.6	1.59
Tamil Nadu	32.5	33.0	20.6	20.0	19.9	1.02
Uttar Pradesh	42.3	42.3	31.2	31.1	33.7	0.86
West Bengal	40.8	41.2	31.9	31.7	37.1	1.50
All-India	37.3	37.2	27.1	27.0	30.2	0.43

Notes: The 'official' estimates for the 50th and 55th Round are those published in the Planning Commission's press releases. The 'estimate' in columns 2 and 4 are my calculations from the unit record data. These differ from the official numbers because the latter are extrapolated from published tables rather than directly calculated from the data. The last two columns show the adjusted poverty estimates using the procedures detailed in the text, together with standard errors calculated from 100 replications of a bootstrap that takes into account the cluster structure of the data, but ignores stratification.

logarithm of expenditure on 30-day goods m . Equation (2) invites us to consider what is the probability of being poor overall, given expenditure on 30-day goods. The head-count ratio for the population as a whole is the average of this probability over everyone.

Equation (2) cannot be evaluated using data from the 55th Round any more than can equation (1). However, if there are grounds to suppose that the probability of being poor conditional on m , $F(z|m)$, is constant over time, and if the density of m , $g(m)$, is the same in the 55th Round as it would have been with a traditional schedule, then we can use the actual marginal distribution of m from the 55th Round together with the conditional head-count function $F(z|m)$ from an earlier round to compute corrected head-count estimates. In particular, I use the 50th Round to compute the head-count conditional on m and estimate the 55th Round poverty rate according to

$$P_{55} = \int_0^{\infty} F_{50}(z|m)g_{55}(m)dm = \hat{E}_{m55} [F_{50}(z|m)], \quad \dots(3)$$

where the 'hats' denote estimates, and the subscripts denote the relevant NSS rounds. According to (3), we use the probabilities of being poor given expenditure on 30-day goods, estimated from the 50th Round, and combine them with the distribution of expenditures on 30-day goods from the 55th Round, expenditures that were collected in a comparable way in the 50th and 55th Rounds. Put differently, we can observe directly expenditures on 30-day goods in the 55th Round. These tell us something about poverty in that round. Exactly what can be calculated by using each household's 30-day expenditures to calculate its probability of being poor, given the relationship between being poor and 30-day expenditures from the 50th Round, and then averaging over all households to get the estimated poverty count.

The procedure here is very different from another method that is sometimes used. This alternative uses expenditure on 30-day goods and on all goods in the 50th Round to calculate the value of 30-day expenditures that correspond to the overall poverty line, effectively a 30-day expenditure poverty line. It then uses this 30-day poverty line in the 55th Round to calculate the fraction of people whose 30-day expenditure is below this cutoff, and uses that as an estimate of poverty. The problem with this method is that there are some people whose 30-day expenditure is low, but who will not be poor overall, and some who are poor

overall, but not poor in 30-day expenditures. These households are recognised in (1), (2) and (3), but missed in the alternative. Indeed, it is easy to use (2) and (3) to show that the alternative method will be correct only when 30-day poverty is a *perfect* predictor of overall poverty.

Note too that the procedure deals simultaneously with the changes from 30-days to 7-days, and the change from 30-days to 365-days. We make no use of the 7-day or 365-day expenditures from the 55th Round, relying only on the 30-day expenditures, which are comparable across both.

What assumptions are required for (3) to work, and why might they be valid? The most plausible assumption is that the density of m is the same in the 55th Round as it was actually conducted as it would have been had the 55th Round been run in the traditional way. Remember that the questionnaire for the 30-day goods is identical to earlier questionnaires, so that the issue is whether changes elsewhere in the questionnaire altered the responses to the parts that remained the same. This is certainly possible, although there is no reason to think so. There is also relevant evidence from the thin rounds, which can be used to compare the distributions of 30-day goods in Schedule 1 and in Schedule 2, where the questions on all the other goods were different. Tarozzi (2001) runs these tests and is unable to reject the hypothesis that the distributions of reported expenditure on 30-day goods are the same in the two Schedules.

The second assumption is about the stability from the 50th to the 55th Round of the function $F(z|m)$ and its validity depends, among other things, on the stability

of the Engel curve relating the logarithm of expenditures per capita on 30-day goods to the logarithm of total household expenditure per capita. If this Engel curve is stable over time, and the distribution of households around the Engel curve does not change, then the fraction of people who are poor at any given level of m will be constant. Note that it is not required that expenditure on 30-day goods be a fixed ratio of total expenditure, only that the relationship between them remain stable. To see how this works, and to see potential problems, suppose that the Engel curve can be written

$$m = \phi(x) + u \quad \dots(4)$$

where $\phi(x)$ is monotone increasing in x and the cdf of u , which is independent of x , can be written $H(u)$. Then we have

$$F(z|m) = \Pr(x \leq z|m) = \Pr[u \geq m - \phi(z)|m] = 1 - H[m - \phi(z)]. \quad \dots(5)$$

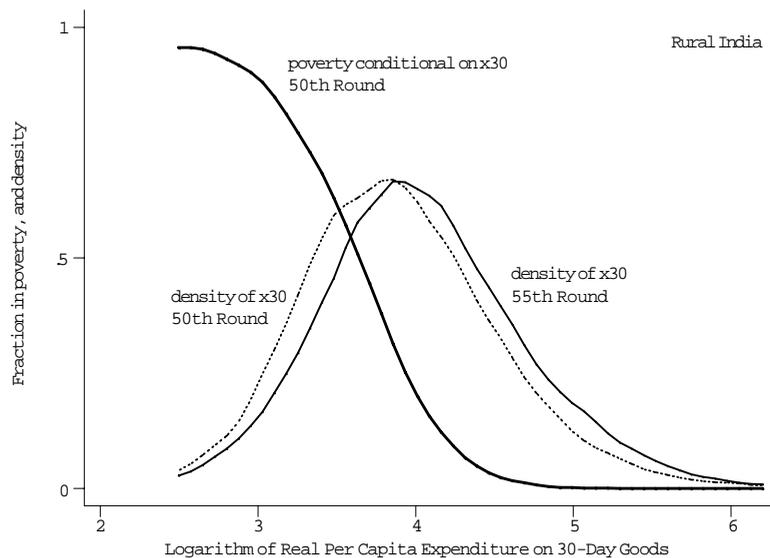
By (5), the regression of poverty on m , $F(z|m)$, will be constant over time if the Engel curve remains fixed, and if the distribution of u remains constant. The equation also highlights a potential source of difficulty. If the Engel curve depends on other variables, perhaps most obviously on relative prices, and if these variables shift, the poverty regression will also shift, and the estimates will likely be biased. If such variables are identifiable, and if the data are available, they can be used to condition the distribution of x along with m in equations (2) and (3). Note finally that, while it is useful to consider the Engel curve when justifying the procedure, the estimation does not work with the Engel curve nor its inverse, the projection of total

Table 2: Headcount Poverty Ratios
(Urban India, Per Cent)

	50th Round		55th Round		55th Round	
	Official	Estimate	Official	Estimate	Adjusted	St Error
Andhra Pradesh	38.3	38.8	26.6	27.2	27.7	1.45
Assam	7.7	7.9	7.5	7.5	8.3	1.23
Bihar	34.5	34.8	32.9	33.5	33.8	1.77
Gujarat	27.9	28.3	15.6	14.8	16.0	1.69
Haryana	16.4	16.5	10.0	10.0	9.5	1.45
Himachal Pradesh	9.2	9.3	4.6	4.6	4.5	0.68
Karnataka	40.1	39.9	25.3	24.6	25.5	1.96
Kerala	24.6	24.3	20.3	19.8	18.7	1.03
Madhya Pradesh	48.4	48.1	38.4	38.5	37.9	1.63
Maharashtra	35.2	35.0	26.8	26.7	28.1	1.49
Orissa	41.6	40.6	42.8	43.5	41.4	3.80
Punjab	11.4	10.9	5.8	5.5	6.3	0.42
Rajasthan	30.5	31.0	19.9	19.4	22.8	2.23
Tamil Nadu	39.8	39.9	22.1	22.5	24.4	1.21
Uttar Pradesh	35.4	35.1	30.9	30.8	30.4	1.64
West Bengal	22.4	22.9	14.9	14.7	19.5	1.24
Delhi	16.0	16.1	9.4	9.2	6.5	0.68
All-India	32.4	32.6	23.6	23.5	24.7	0.41

Notes: The 'official' estimates for the 50th and 55th Rounds are those published in the Planning Commission's press releases. The 'estimate' in columns 2 and 4 are my calculations from the unit record data. These differ from the official numbers because the latter are extrapolated from published tables rather than directly calculated from the data. The last two columns show the adjusted poverty estimates using the procedures detailed in the text, together with standard errors calculated from 100 replications of a bootstrap that takes into account the cluster structure of the data, but ignores stratification.

Figure: Distribution of Expenditures on 30-Day Goods in the 50th and 55th Rounds, and Probability of Being Poor Conditional on 30-Day Expenditures, Estimated from 50th Round



expenditure on 30-day expenditure, but directly with the regression of poverty on 30-day expenditures through equation (3).

Once again, without the missing data from the 55th Round, there is no way of checking the validity of the assumption. But once again robustness can be checked by calculating the regression functions $F(z|m)$ from the thin rounds – albeit less precisely than for the 50th Round – and the results inserted into (3) in place of the estimates from the 50th Round. I have done some experiments along these lines for the All India estimates shown below, and while there are some changes, they are well within the range of uncertainty given the standard errors.

IV

Empirical Methods, Results and Discussion

I calculate the adjusted poverty estimates by direct application of equation (3). Using data from the 50th Round first, I calculate a dummy variable for each household indicating whether its members are poor or not, according to whether its household per capita expenditure is below the official poverty line for the 50th Round. When the calculations are for All-India, I use the All-India poverty line; when they are for a specific state, I use that states specific poverty line. All calculations are done separately for urban and rural sectors. I then use a locally weighted regression procedure, to locally regress the poverty dummy on the logarithm of 30-day expenditures. I do this at each point on a 50-point grid. These regressions are weighted

by the NSS-supplied household inflation factors multiplied by household size so that everything is effectively done at the individual, not household, level.

Turning to the 55th Round, I first deflate 30-day expenditures by the consumer price index implicit in the appropriate official poverty line. For example, the official poverty line for rural Bihar was 212.16 rupees in the 50th Round and 333.07 rupees in the 55th Round, so that the deflator for rural Bihar is 333.07 divided by 212.16. While it might be preferable to work with a price index for the 30-day goods, the real issue is the stability of the Engel curve with respect to changes in relative prices, which my preliminary calculations suggested was not too much of a problem. After deflation, I compute a kernel density estimate for the logarithm of deflated 30-day expenditures, using the same grid points as in the 50th Round regression. The corrected poverty estimate is then a weighted average of the 50th Round regression predictions using the estimated density as weights.

Before looking at the estimates, it is useful to consider the Figure. The Figure shows, for All-India rural, the two estimated densities for the logarithm of real per capita expenditure on 30-day goods in the 50th and 55th Rounds. These estimates, weighted and averaged over the population, give us our estimates of the poverty head-count ratio. The important point to note is the extent to which the density of 30-day goods has moved to the right from the 50th to the 55th Round. At all levels of the distribution, among the poor, the middle, and the best-off, there are more 30-day expenditures in 1999-2000 than

there were in 1993-94. It is this fact that drives the reduction in estimated poverty, and that makes it so unlikely that there has been no improvement in living standards. Expenditures on 30-day goods are translated into poverty estimates using the probability of being poor given expenditures on 30-day goods. This is shown as the solid line in the figure, falling from left to right. Among those with very low expenditures on 30-day goods, the probability of being poor is nearly one, but falls steadily the more 30-day expenditures there are. Again, these are probabilities; I am not assuming that there is any deterministic relationship between being poor and the amount of 30-day expenditures. The poverty head-count ratios reported below are calculated by weighting the density of 30-day purchases, in Figure 1 the dotted line, by the probability of being poor conditional on 30-day expenditure, the solid declining line.

Table 1 shows the results for rural India, and Table 2 for urban India. I have shown my own recalculation of the official head-count ratios along with the official estimates themselves. The two sets of estimates are slightly different, presumably because the official numbers are not calculated from the unit record data, but from interpolation using published tabulations of the size distribution of per capita expenditure. I present both sets of numbers because the adjusted estimates are calculated from the unit record data, and to demonstrate that the difference between the adjusted and official estimates does not come from my inability to reproduce the official counts. The penultimate column of the tables shows the adjusted head-count ratios, and the last column shows bootstrapped standard errors. These should be thought of as standard errors around the true poverty rates, not around the estimates that would have been produced by the 55th Round had it been run along traditional lines. Because I have allowed for the cluster structure of the data, but not the stratification, they are likely somewhat too large. Once again, note that these estimates have been updated for other factors and fully recalculated in Deaton and Drèze (2002).

The adjusted rural poverty estimates are somewhat higher than the official 30-day estimates. For all-India, the official estimate of 27.1 per cent is replaced by 30.2 per cent. Instead of there being a drop in rural poverty since 1993-94 of 10.2 percentage points, the adjusted figures show a reduction of only 7.0 percentage points, so that a little more than two-thirds of the official reduction appears to be real. Most of the states show a similar pattern, though

in the cases of Madhya Pradesh, Orissa, Punjab, and Tamil Nadu, the adjusted estimates are lower than the official numbers. In the urban sector, Table 2 shows adjusted urban estimates that are typically very close to the official estimates. For All India urban, the official estimate of 23.6 per cent is raised only to 24.7 per cent, so that I estimate that 7.9 percentage points of the official reduction of 9.1 percentage points is real. Across the states, some of the adjusted figures are lower and some higher than the official figures. Notable changes are in Rajasthan and West Bengal where the adjusted poverty counts are considerably higher than the official ones.

In summary, the calculations suggest that the official poverty counts based on the 30-day questionnaire are not seriously misleading, though in the rural sector, it appears that only around two-thirds of the officially measured decline in poverty is real. The other third is an artefact, presumably induced by changes in the survey instrument between the 50th and 55th Rounds.

The corrected figures raise a number of questions of their own. First, if the changed survey design has its effects through the way that respondents react to the questionnaire, it is unclear why the effects should be different from one state to another, and in particular, between rural and urban households. Perhaps the difference has something to do with other changes, for example in the way that respondents were asked about home-produced foods, or in the uniform adoption of a 365-day questionnaire for the low-frequency items. Both of these changes surely altered reported expenditures on durables and on home-grown food, and would have done so differently depending on whether or not the respondent was engaged in agriculture and on his or her level of living. Agriculture is more important in the countryside, and durables are more important in towns. Second, the results are very different from the prior expectations of many researchers, including my own. That reported 30-day expenditure would be pulled up by the presence of questions about 7-day expenditures seems entirely plausible, yet the results in the tables suggest that most of the distortion was to the seven day reports, not to the 30-day reports. There is one other shred of evidence relevant to this. The NSS has recently repeated the 7-day versus 30-day experiments that were done by Mahalanobis in the 1950s. The preliminary results appear to suggest that the 30-day estimates are more reliable than the 7-day estimates, which appear to be overstated. If so, it is possible that the 30-day

responses are generally reliable, and that 7-day recall is less accurate, and more prone to being changed by changes in questionnaire design.

V

Poverty Monitoring in India

It is worth stepping back from the results, and considering the broader implications for poverty monitoring in India. That it is necessary to adjust the official estimates at all is unfortunate, although the NSS is surely to be congratulated for its willingness to conduct experiments in the interests of long-run improvements in data collection and poverty estimation. That said, it is most desirable that survey practice and design be stabilised soon so that, when the next large round is done, we will be able to have poverty counts that are compatible with at least some earlier estimates, and that do not need to be adjusted by statistical techniques. Exactly what the questionnaire should look like is still not clear. Many people have assumed that, because Schedule 2 produced more expenditures, it is therefore better, but that is not necessarily true. Recent NSS experiments, reported elsewhere in this volume, suggest that, at least for some goods, the 30-day reporting periods may be more accurate, as Mahalanobis and Sen originally found. So it may be better to go back to something like the traditional Schedule, with its uniform 30-day reporting period. The choice between 30 days and 365 days for low-frequency items is a particularly difficult one. The reduction in mean that accompanies the move to 365 days is likely to come from people's inability to remember purchases made 10 or 12 months ago. On the other hand, the longer recall period allows more people to report something, and raises reported expenditures the bottom tail of the per capita expenditure distribution. Compared with the earlier practice, this change tends to reduce measured poverty below what it would have been with the original, 30-day, questionnaire. Indeed, as argued by Sundaram and Tendulkar (2001), the change from 30 days to 365 days for the low-frequency items may by itself be responsible for much of the understatement of poverty (relative to earlier methods) in the official estimates for the 55th Round.

It should also be noted that the choice of recall period, important though it has been in the current discussions, is far from the only, or even most important, issue in poverty counts. Issues of comparability between NSS and NAS estimates are now

being seriously addressed, and it appears that earlier claims that NSS estimates were wild underestimates were themselves greatly exaggerated. Nevertheless, given the importance of the poverty estimates in India, not to mention the worldwide interest in the relationship between growth and poverty reductions, it would be useful to maintain a close dialogue between the producers of the two kinds of data.

Finally, poverty counts depend only in part on the survey data. They also depend on the poverty lines that are used as cut-offs. I have argued elsewhere that the current set of poverty lines used by the Planning Commission are not defensible and ought to be changed, [see Deaton and Tarozzi 2000] and my companion paper in this issue. The distribution over states and sectors of the current poverty lines makes very little sense, and there are better alternatives available. [27]

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[I am grateful to Jean Drèze, Bo Honoré, Nick Stern, and Elie Tamer for helpful discussions during the preparation of this paper. I should particularly like to acknowledge discussions with and help from Alessandro Tarozzi whose paper, Tarozzi (2001), develops an alternative methodology and covers many important issues not dealt with here.]

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