

Consumer expenditure, Distribution and Poverty: Implications of the NSS 55th round.

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I. The Issues

For some time now, data available periodically from the National Sample Survey (NSS) have figured significantly in policy-related discussions on the effects of the economic strategies of the 1990s on the incidence of poverty, especially rural poverty. Throughout the 1990s, the NSS results on household consumption expenditure generated much interest in both academic and policy circles^[2]. These results, which many independent researchers have analysed, had led to a general consensus that rural poverty at the all-India level did *not* show any declining trend over the 1990s (see Table 1)^[3]. That this had happened despite the somewhat higher rates of aggregate GDP growth during the period became an important issue in the ongoing policy debate on the effects of the liberalising economic policies instituted by successive governments over the 1990s.

TABLE 1: Estimates of Poverty in India (Head Count Ratios)

NSS Round	Year	Rural			Urban		
		Datt	Gupta	Tendulkar-Sundaram	Datt	Gupta	Tendulkar-Sundaram
27	October 72-September 73	55.4	NA	57.2	45.7	NA	47.0
28	October 73-June 74	55.7	NA	56.2	48.0	NA	49.2
32	July 77-June 78	50.6	NA	54.5	40.5	NA	42.9
38	January -December 83	45.3	45.6	49.0	35.7	40.8	38.3
42	July 86 -June 87	38.8	NA	45.2	34.3	NA	35.4
43	July 87 -June 88	39.2	39.1	44.9	36.2	38.2	36.5
44	July 88 -June 89	39.1	NA	42.2	36.6	NA	35.1
45	July 89 -June 90	34.3	33.7	36.7	33.4	36.0	34.8
46	July 90 -June 91	36.4	35.0	37.5	32.8	35.3	35.0
47	July 91 -December 91	37.4	NA	40.1	33.2	NA	34.8
48	January -December 92	43.5	41.7	46.1	33.7	37.8	36.4
49	January 93-June 93	NA	NA	44.2	NA	NA	38.9
50	July 93 -June 94	36.7	37.3	39.7	30.5	32.4	30.9
51	July 94 -June 95	41.0	38.0	43.6	33.5	34.2	34.1
52	July 95 -June 96	37.2	38.3	40.1	28.0	30.0	28.7
53	January -December 97	35.8	38.5	38.3	30.0	33.9	31.0
54	January 98-June 98	NA	45.3	44.9	NA	34.6	31.6

Sources: (a) Ozler, Datt and Ravallion (1996), as updated by Datt (1999); (b) Gupta (1999); (c) Sundaram and Tendulkar (2000)

Those questioning the economic reform package have argued that these policies have involved neglect of rural investment and of the food security system, resulting in slow agricultural growth, reduced employment opportunities in rural areas, and high food prices. All of these are likely to be associated with persistent or even increasing rural poverty. Also, that NSS surveys show stagnation or even decline in rural non-agricultural employment, which is in conformity with their argument that the reform package increased the urban-rural divide by reducing the spill-overs that public sector effort can contribute by way of mitigating inter-sectoral and inter-regional inequalities.

By contrast, proponents of the economic policies of the 1990s have by and large held that the NSS results so far available did not allow such conclusions, and that nothing could be said conclusively about rural consumption or poverty until the next large sample results were available. The serious economists amongst these have not contested the NSS data which show that rural poverty has failed to decline, but have argued that the reforms process should not be implicated for this. They have questioned the critics' association of economic liberalisation and public expenditure cuts with the lower growth of rural real consumption implied by the NSS, and argued essentially that extraneous factors may be involved^[4].

A view often expressed in this context was that the matter of causation cannot be settled since the last large sample NSS round in 1993-94 was too close to the beginning of the reform period, and that the more recent results are from "thin samples". Because of their smaller sample size, and the associated higher variance of sample estimates, these are not reliable for analysis below the national level. Since it is generally agreed that the somewhat higher GDP growth during the 1990s has been associated with larger interstate differences in growth rates, correlation of these with poverty trends at the state level are of particular importance to test the relationship between economic growth and poverty reduction.

However, in the course of this debate, some defenders of the economic reform strategy went much further and questioned the NSS database itself. These observers found it impossible to accept that income poverty at the national level can increase during a period of rapid GDP growth and claimed that the NSS consumption expenditure estimates from the thin samples must be flawed even at the national level simply because these were out of line with estimates of GDP growth^[5]. Interestingly, this fundamentalist position, that economic growth is not only necessary for poverty reduction but also sufficient, was at this very time being questioned by the World Bank's draft "World Development Report". In the subsequent effort to stall this rethinking, a leading role was played by some influential Indian economists abroad who, among other things, questioned the Indian data in this context.

Thus, there emerged a view that the association of higher GDP growth rates with the persistence of rural poverty was not a real fact which needed to be understood and addressed, but a failure of the statistical system to capture the actual increase in consumption in rural areas. Unfortunately, this direction of attack sought to undermine the credibility of a consistent and comparable time series of estimates, which not only has an almost incomparable statistical pedigree among survey sources of economic data anywhere in the world but has also so far been accepted as reliable by economists, whatever their differences on the interpretation of the results thus thrown up.

With this background, considerable importance attached to results of the 55th Round (1999-2000) of the NSS, which is the first "large sample" since 1993-94. These results were being awaited to be examined closely, not only for an assessment of the actual material condition, consumption and employment patterns of people in the country, but also for policy implications regarding actual implementation of the economic reforms process and its effects on the welfare of the poor. But it now appears that this round, instead of providing the necessary statistical material for serious analysis of actual trends, may only add to confusion regarding the data.

There have already been a spate of newspaper reports about the results of the first two of the four sub-rounds the 55th round which have been tabulated. First reports had suggested that

these show a dramatically lower estimate of poverty than those obtained from the earlier rounds of the 1990s. Predictably, the media took the opportunity to both hail this as evidence that the reforms have reduced poverty, and, noticing the incongruity of the NSS reporting such a dramatic reduction within such a short time, to also rubbish the country's statistical system (Indian Express, September 23, 2000). Since then newspaper reports on the matter have been even more intriguing. Not only have there been claims that the data might be flawed, but also that differences exist within government. (Business Standard, October 28 and 31, 2000).

Obviously, nothing definite can be said either about the 55th Round results, or about the official position on this, until the data from all the four sub-rounds are released. But it is already evident that this NSS Round will be less relevant for analysis or policy assessment, and more interesting for another reason. This is because in the 55th Round, the National Sample Survey Organisation (NSSO) has made a major deviation from the technique it has been using so far to establish household consumption levels.

The basic change is in terms of the reference periods used in questions asked on consumption. In all NSS rounds after the early 1950s, the reference period has essentially been uniform, with respondents asked about their consumption during the "past 30 days". But, after experimenting briefly with an alternative questionnaire using a "past week" reference period for food and "past 365 days" for certain other items, and having obtained higher consumption, especially for lower income households, the NSSO has changed its questionnaire. During the 55th round, questions on consumption of clothing, footwear, education, health (institutional) and durable goods were asked only for the "past 365 days", and, for food, tobacco and intoxicants, all sample households were put both the "30 day" and "one week" questions.

This appears to have been a concession by the NSSO to its critics who had alleged an increase in the underestimation of household consumption during the nineties. At least one of these critics has already hailed this change, though "belatedly only 40 years later", because, in his opinion, the 30 day recall "leads to unreasonably low estimates of actual consumption". Moreover, he has claimed that the 55th Round shows "poverty has *declined*, concomitant with economic growth" (Bhalla 2000b⁶, emphasis added), although he is aware that changes in methodology, even if these improve point estimates, make time series comparisons difficult.

He, and many others, see in this data "a reason for some celebration" because of the "joy that there are less joyless in India", and find any argument for further scrutiny to be mere carping. But, since statistics should reflect the truth, killjoys will abound among serious analysts on both sides of the policy debate if in the process the 55th Round has become non-comparable with all past rounds, and thus lost utility for the analysis that had made it so eagerly awaited. In view of this, and the criticism to which the NSS has been subjected to recently, there is a need to examine the consistency of the available NSS data, including the data so far released from the 55th Round, without this getting embroiled in the policy debate.

This is the objective of this article. In section II, the issue of the reference period used by the NSS is taken up: why the NSS has so far used the "30 day" reference period, why recently a need was felt for further methodological experiments on the matter, and what results were actually obtained from the experiments conducted by the NSSO during the 51st to 54th Rounds. The

preliminary results from the 55th Round are considered in this context, and, in the light of this, it is apprehended that *the 55th Round may be an experiment which has failed*.

It is, however, extremely important to appreciate that this is an inherent risk in all scientific endeavour, which should not reflect negatively on the NSSO. Indeed the main objective of this article is to stress the basic integrity of India's statistical system and to emphasise that prejudices, however plausible, should not be allowed to override statistical method. Since differences do exist between the NSS and alternative data, there was a strong scientific need for the experiments carried out by the NSSO during Rounds 51 to 54. The results of these experiments are important, and need to be analysed in full.

These experiments, with alternative reference periods, had shown significantly higher food consumption by the one-week recall but also larger sampling errors of these estimates. For clothing, durable goods and certain services, the 365 day recall had suggested both lower consumption and smaller sampling errors than by the 30 day recall, but also a much more equal distribution. Compared to the usual 30 day schedule, the alternative schedule had thus shown higher mean consumption and lower inequality, but with much higher sampling error of the distribution itself, especially at its lower tail.

There was thus strong evidence that the reference period used does systematically effect reported consumption and its distribution across commodities and expenditure groups. For example, that choosing a "one week" reference period increases estimates of food consumption. This had certainly warranted further experimentation. But, these experiments had not established greater reliability of estimates from the alternative schedule. It was, therefore, necessary to proceed cautiously to preserve continuity of data and draw proper inferences. Unfortunately, pressure to reflect quickly in the 55th round the higher food consumption obtained earlier in the experimental schedules appears to have led to these being administered in a manner that has contaminated the data.

A disturbing consequence of this is that the 55th Round is unlikely to provide any conclusive indication of the trend in poverty. Initial analysis of the partial data released by the NSSO from the 55th Round suggests that newspaper reports were correct in reporting that the 55th Round shows a large decline in the incidence of poverty during 1999-2000 as compared to 1993-94. However, this comparison is vitiated by differences in reference periods used. *The reference periods used in the 55th Round make its results conceptually non-comparable to all previous officially released estimates from the NSS.*

The *only* conceptually valid comparison possible is of the 55th round results from the one-week recall with those obtained from the alternative experimental schedule canvassed during Rounds 51 to 54. On this basis, *the presently available results from the 55th Round show higher poverty than during each of the previous four thin sample NSS rounds*. Since rural poverty in at least three out of these four rounds was earlier found to have been higher than during 1993-94 by using the mutually comparable 30 day recall, there is greater validity to the opposite claim: that poverty during 1999-2000 was significantly higher than in 1993-94. However, *the real problem at this stage is the threat posed by such highly contradictory conclusions to the credibility of the country's statistical system.*

The root of this problem of credibility lies in differences between NSS estimates of household consumption and those available from the National Accounts Statistics (NAS). And, particularly, in a perception that these differences had increased so markedly during the nineties that an immediate review of the NSS methodology was necessary. Section III takes up this issue. It is shown that there *are* certain persistent differences between the NSS and NAS estimates, which did diverge increasingly during the seventies and the eighties. But, contrary to the perception, *there had actually not been any further significant increase in the divergence between the NSS and NAS estimates of nominal consumption during the nineties.*

Since the NSS measures nominal consumption only, this implies that under-estimation by the NSS did not increase during the nineties and that the perception of increased divergence is largely extraneous to the NSS. This has arisen because of the use of different deflators and on account of recent revisions by the Central Statistical Organisation (CSO) to the NAS data. Moreover, it is well known that the NSS has always under-enumerated the rich, leading to lower estimates of non-food consumption than by the NAS. Any disproportionate increase in the consumption of the rich would, therefore, bias downward both the mean consumption and the inequality as measured by the NSS. This would also introduce errors in any attempt to validate alternative NSS reference periods through comparisons with the NAS. On making the necessary adjustments, it is found that estimates of food consumption by the one-week recall may actually be overestimates.

Detailed analysis of the available data thus suggests that there is as yet no statistical warrant to prefer the reference periods used in the 55th Round over the 30 day recall. First, the experimental rounds did not provide any in-sample evidence of the inferiority of the 30-day recall. Second, there is no basic inconsistency between consumption trends from earlier NSS rounds and the NAS. And, third, even point comparisons between the two sources do not necessarily imply that the 30-day recall underestimates consumption.

Nonetheless, with poverty having failed to decline by NSS data during the nineties despite the somewhat higher GDP growth, scepticism will continue unless it is established that there was an increase in inequality. Validation of NSS data is, therefore, not complete without an identification of the sources of such increased inequality. This is done in Section IV, where it is shown that *inequality increased both through the impact of higher relative food prices and through changes in the distribution of nominal consumption between the rural and urban areas and within each of these areas.*

An important aspect of these distributional changes relates to the rural-urban differential. Even those who insist that there has not been any increase in the inequality of incomes within each of these sectors, and argue the primacy of income growth for poverty reduction, would agree that rural poverty is unlikely to have declined if rural per capita incomes have stagnated. A calculation of rural incomes based on the National Accounts Statistics is, therefore, presented. Again recent revisions in the NAS cause some complication, but the results of this analysis are quite conclusive in indicating that *there was a very significant deceleration of the growth of per capita incomes in rural areas during the nineties, because the rapid growth of the non-agricultural incomes did not spill over to rural areas.*

However, it is important to keep separate the trends evident in the data currently available from the issue of data collection and the credibility of the NSS. While there is enough consistency

between the existing NSS and NAS estimates to reject the charge that somehow the former has become less relevant recently, it is certainly the case that the NSS has always missed out on some part of the consumption that is actually being carried out by the residents of the country. It is, therefore, quite possible that all currently available estimates of poverty are overestimates, although it remains extremely unlikely that the trends so far available for the nineties are in any way erroneous.

As in any survey-based method, there are two possible sources of error in the NSS data. There could be non-sampling biases in recall and response, which might be reduced by choosing a different reference period or by some other change in the survey methodology. And, there is almost definitely a sampling bias with respect to the underreporting of consumption by richer households. These certainly require further investigation, and there is clearly a need for systematic work through experimental sampling techniques to try and minimise such biases. But for such experimentation to be valid, these must be conducted in a manner that allows for comparability of the data over time and is completely transparent, with separate data from different survey methods made available separately. Further, in order to preserve the integrity of the statistical system, such experiments must be analysed statistically by statisticians and comparable estimates generated, without interference from economists, policy makers and others who have their own priors, and perhaps even biases, to defend.

This is particularly important because the utility of NSS consumer expenditure surveys is wider than its use in the measurement of poverty. Although this is an important and politically sensitive use, the NSS is also the main data-source for estimates of consumer demand, which are required for various policy purposes. Since the reference periods used for these surveys do appear to affect estimates of consumption and its distribution across expenditure classes, it is necessary to ensure that choice in this matter does not distort the input required for policy. This is an especially relevant priority today, when the economy is facing a situation of large excess stocks of several food items despite relatively low increase in their output. An assessment of why current demand is low, and what can be expected in the future, is possible only if the data available allows for comparability over time and is not distorted because of changes in survey methodology.

II. The Choice of Reference Period

Since the 1950s, NSS consumption surveys have been using a uniform reference period of one month, and spreading interviews evenly over months to iron out the problem of seasonal variations. However, in recent years the NSSO has revived the issue of whether a one-week reference period is more suitable for determining food consumption than the one-month reference period currently used.

This is not a new issue. Indeed, this question has been of concern to the NSS since the very inception of the surveys in the early 1950s. In fact, in the formative years of the NSS, considerable attention was paid to the length of reference period suitable for ascertaining the correct level of consumption of different items of goods and services. A special report on the suitability of reference period was brought out covering the period April 1951 to March 1954.

Most interestingly, a special investigation into this very issue was carried out during March-April 1952 under the guidance of P.C. Mahalanobis, based on 1254 households of 76 villages of West Bengal. The households were divided into two groups. For one group, consumption details were procured by actual weighing of food items (clean rice, pulses, sugar and salt) by field staff. For the other group, data collection was by questioning, and here again the group was divided equally between those for whom the questions pertained to a reference period of one-week and those for whom the reference period was one-month.

The results were revealing. It was found that the two sets of data obtained by questioning differed quite sharply, with the consumption estimates obtained on the basis of the one-week reference period being higher than those obtained on the basis of one-month recall. It was also found that the one-month recall generated information that corresponded much more closely to the data on the basis of actual weighing of food items (Mahalanobis and Sen, 1954⁷). This led to the conclusion that the one-month reference period was better suited for the purpose of estimating food consumption through the survey methodology in India, although even then a one-week reference period was standard in budget surveys in the West.

Since then, the National Sample Survey has consistently used the one-month reference period for food items, although both the one-month and one-year reference period have been used for some non-food items. In the five quinquennial surveys of household consumption expenditure between 1972-73 and 1993-94, information for clothing, footwear and durable goods was collected from each sample household for two reference periods - "last 30 days" and "last 365 days". In the 50th round, "educational" and "institutional medical" expenses were also added to the list of items for which data were collected by these two reference periods. Nonetheless, in order to maintain comparability, the final results for all these past quinquennial rounds, as published in the relevant NSS reports and used by researchers, was only by the "30 day" recall.

However, during the 1990s, the question of the most suitable reference period for food consumption has resurfaced. This is for many reasons, but essentially because past NSS rounds have been throwing up certain puzzles regarding food consumption, which have yet to be resolved. First, the NSS estimates of cereals consumption have shown lower growth than the official estimates of cereals production, and, consequently, from having exceeded the official cereals availability figures till the late 1980s have since fallen below. Second, although the relatively slow growth of

cereal consumption in the NSS has been attributed to a shift in food consumption patterns towards other food, the NSS itself has consistently estimated lower consumption of most non-cereals food than the NAS. Third, although the food consumption data from the NSS imply a very high incidence of nutritional inadequacy when converted to nutrient terms, this contrasts markedly with the very high percentage of respondents who report that they have had two square meals in subjective questions to this effect asked by the NSS itself since 1983.

In an effort to test whether some of these differences are due to recall, the NSS had in its recent thin samples experimented with alternative schedules administered to independent sub-samples during the course of the same survey. This was done for the 51st Round (1994-95), the 52nd Round (1995-96), the 53rd Round (1997) and the 54th Round (Jan-Jun 1998). In all of these Rounds, one half of the sample (Type 1) had a reference period of 30 days for all items. For the other half (Type 2) the reference periods were as follows: one week for all food, pan, tobacco and intoxicants; one month for fuel and light and miscellaneous goods and services; and one year for clothing, footwear and durable goods as well as education and institutional medical expenses. But, since the Type 2 schedule was not comparable to earlier NSS surveys, the results by this schedule were not tabulated in the NSS Reports on Consumer Expenditure for the relevant rounds. All available analyses of consumer expenditure and of poverty during the nineties are based on the Type 1 schedule.

However, in a separate report, the NSSO has recently released the comparative results on consumer expenditure, and its distribution, as obtained from the Type 1 and Type 2 schedules canvassed during the 51st, 52nd, 53rd and 54th Rounds (NSSO, 2000a⁸). It is, therefore, possible to examine the effect of choosing one reference period over another. These results based on the alternative schedules are extremely interesting. It emerges that the "one week" recall gives much higher estimates of overall food consumption, *exactly as Mahalanobis had found in the early NSS surveys and confirmed through pilot investigation in West Bengal villages in 1952*⁹¹.

Total expenditure on food was about 30 per cent higher according to the Type 2 schedule than from the Type 1 schedule, with the difference ranging from about 14 per cent in the case of cereals and milk to almost 75 per cent in the case of spices. These differences between the one-month and one-week responses were found to be fairly stable across each of the experimental rounds, but, interestingly, the differences were found to be systematically larger for richer households (Table 2). *The one-week response thus implies greater inequality in food expenditure and also higher income elasticity of food than by the one-month recall.*

Equally interesting are the results for the goods and services canvassed by the 365-day recall in the Type 2 schedule. In the case of these, with the exception of clothing, the results by the Type 2 schedule generally indicate much lower mean consumption than by the 30 day recall used in the Type 1 schedule. However, even for these goods and services, including clothing, the consumption reported by the bottom half of the population turns out to be much higher by the Type 2 schedule than by the Type 1 schedule and, simultaneously, the consumption reported by the richest households is much lower. While the top quartile reports 40 to 50 per cent less consumption of these goods by the 365 day recall than by the one month recall, the poor report a consumption which is more than double by the Type 2 schedule than by the Type 1. As a result, *the 365 day recall indicates a much lower income elasticity of demand for clothing, durables and the*

expenditure on education and health than hitherto estimated with the 30 day recall, and also a much more equal distribution of the consumption of these items across household groups.

TABLE 2: Percentage difference between consumption estimates from Type 2 and Type 1 schedules in Experimental Rounds (All India Rural)

	Round	Quartile 1	Quartile 2	Quartile 3	Quartile 4	All
Items canvassed by 7 day recall in schedule Type 2	51	23	28	31	37	31
	52	22	26	29	44	33
	53	23	25	26	34	28
Items canvassed by 365 days recall in schedule Type 2	51	176	77	31	-54	-26
	52	214	101	22	-47	-16
	53	145	71	16	-46	-22
Items canvassed by 30 days recall in schedule Type 2	51	-2	-2	-1	-11	-6
	52	-4	-3	-2	2	-1
	53	-2	-2	-5	-5	-4
All items	51	24	25	25	3	15
	52	24	25	22	11	18
	53	23	23	19	3	13

Source: Computed from NSSO Report No. 447

The net consequence of these differences, which are substantial and systematic in the four experimental rounds, is that total consumer expenditure is reported higher according to the Type 2 schedule, and the distribution of this expenditure across households is also reported more equal. For the bottom half of the population, total consumer expenditure by the Type 2 schedule is about 25 per cent higher than by the Type 1 schedule, with about 75 per cent of the difference due to food and 25 per cent due to the items canvassed by the 365 day recall. As a result, the proportion of population below any expenditure level is always higher by the Type 1 schedule than by Type 2, with this difference very large at the lower expenditure ranges. Poverty estimates obtained from the Type 2 schedules are, therefore, much lower than by the one-month recall used so far.

Pravin Visaria (2000a¹⁰) has compared the poverty estimates as obtained from these two consumer expenditure schedules canvassed during Rounds 51 to 54. He finds, for example, that although about 38 per cent of the rural population was found to have consumption below the poverty line in 1995-96 (52nd round) by the Type 1 schedule, this percentage was only around 19 per cent by the Type 2 schedule. The corresponding percentages for urban areas were 30 and 15. Similar large differences are obtained for the 51st, 53rd and 54th rounds which all show almost half the poverty incidence by the Type 2 schedule as compared to the Type 1 schedule (Table 3).

In the large sample 55th Round of 1999-2000, this experiment has been carried one step further. In this round, estimates for clothing, footwear, durable goods and expenditure incurred on education and on health (institutional) were obtained *only* with a 365 day recall. For, food and intoxicants, every sample household was canvassed by both the one week and the one month reference periods. The idea obviously was to prepare for a transition to the Type 2 schedule. This would not only make the NSS methodology similar to that followed in budget surveys carried out in developed countries, it would also increase the NSS estimate of food and total consumption. These would then be closer to the alternative estimates obtained from the NAS, reducing the discrepancy that currently exists between nutritional adequacy as calculated from the NSS consumption data and the results that the NSS has obtained from subjective questions regarding the incidence of hunger.

TABLE 3: Poverty Estimates from Type 1 and Type 2 Schedules of Rounds 51 to 54, and by the 30-day and 7-day recalls in sub-rounds 1&2 of the 55th Round

Round	Year	Rural		Urban	
		Type 1	Type 2	Type 1	Type 2
51	July 94-June 95	41.2	22.8	35.5	18.3
52	July 95-June 96	37.6	19.1	29.9	15.2
53	Jan -Dec 97	35.9	20.7	32.3	17.8
54	Jan - Jun 98	42.6	23.6	32.9	20.0
55*	Jul - Dec 99	27.6	24.8	25.2	23.4

Source: Visaria (2000b): “Polemics on Poverty”, *Business Standard*, October 30th, 2000. For the 55th Round, the result from the 30 day recall for food is shown under Type 1, although this schedule in Rounds 51 to 54 did not use the 365 day recall, and is not strictly comparable. That from the 7 day recall is under Type 2.

Visaria, who is Chairman of the NSSO Governing Council, has expressed preference for the one-week recall for food in precisely these terms. He recognises that poverty estimates by this recall are not comparable with earlier estimates, and in fact argues that there is a case to revise upwards the poverty line to make these rather low estimates “more realistic and relevant”. He, however, stresses the prior that “the incidence of stark hunger in the country may be much less than generally believed”. And writes that: “The overstatement of the level of poverty has thus quite likely been a consequence of a long reference period adopted for the collection of the data on food consumption by our people. To confirm this hypothesis, the quinquennial survey for 1999-2000, due to be completed by June 30, has collected data from each household according to the two reference periods of a week and the 30 days preceding the date of survey. Its findings will perhaps help to clinch the issue” (Visaria op. cit.).

However, in the process, not enough thought appears to have been given to the comparability of the 55th Round results with those from earlier rounds. First, to the extent that the 55th Round has canvassed certain items by only the 365 day recall, this makes estimates for these items non-comparable with estimates from all previous rounds, and, as discussed above, also has serious implications for the distribution so obtained. Second, since the two schedules canvassed in Rounds 51 to 54 had given varying results for food consumption, incorporating them so that all the households respond by both the one week and one month reference periods was obviously problematic since it could bias the results of either or both.

The problem is that, since all households were questioned on food consumption by both types of recall, there would have been a pressure for consistency between answers to the one-week and the 30-day reference periods on the part of both respondents and investigators. It is very likely that when a household is questioned using both the one-week and one-month recall, the answers will be tested by simple multiplication of the one-week reply for the monthly response as well. Hence, although the 55th Round gives results on food consumption by both the reference periods, these can no longer be seen as independent. Both are likely to differ from earlier rounds depending upon the exact conflation of the reference periods.

Now that some results are available from the first half of the 55th Round (Table 4), and preliminary estimates of poverty can be made on this basis, it is worth considering these to get an idea of the problems involved^[11]. Using the distribution obtained from answers to food consumption by the 30 day recall in the 55th Round, the incidence of poverty during July-December 1999 are

27.4 and 25.2 per cent respectively for Rural and Urban India^[12]. *By the one-week recall*, the same poverty lines give estimates of 24.4 and 23.4 per cent respectively. Visaria (2000b) reports very similar results (Table 3).

TABLE 4: Distribution of Consumer Expenditure in the 55th Round (sub-samples 1 &2) All- India July – December 1999

RURAL					URBAN				
Expenditure Class	30 day recall for food etc		7 day recall for food etc		Expenditure Class	30 day recall for food etc		7 day recall for food etc	
	% persons	Av. MPCE	% persons	Av. MPCE		% persons	Av. MPCE	% persons	Av. MPCE
0 – 220	4.8	186	4.3	185	0 – 290	4.5	242	4.1	242
220 – 250	4.5	237	3.7	237	290 – 330	4.2	310	3.3	311
250 – 290	9.0	271	7.9	272	330 – 405	9.4	369	9.1	368
290 – 330	9.6	311	8.9	311	405 – 480	10.0	443	9.7	443
330 – 370	10.2	351	9.8	351	480 – 550	9.9	516	9.1	514
370 – 410	10.2	390	9.7	390	550 – 630	9.1	591	9.6	589
410 – 460	10.4	435	10.5	434	630 – 735	9.9	682	10.6	681
460 – 515	9.9	487	10.6	487	735 – 855	9.7	792	9.7	792
515 – 605	10.9	556	11.8	557	855 – 1040	10.2	943	10.3	940
605 – 765	10.3	676	11.4	677	1040 – 1315	9.4	1167	10.1	1167
765 – 945	5.3	843	5.8	842	1315 – 1535	4.5	1422	4.6	1418
945 +	5.0	1331	5.7	1324	1535 +	9.1	2371	9.9	2352
All	100.0	484	100.0	502	All	100.0	839	100.0	860

Source: NSSO (2000b), Report No. 453

It is these figures which have been compared to the much higher figures reported in Table 1 for the poverty incidence found from the 50th Round in 1993-94 to conclude that actual poverty has declined. However, since all available results from the 50th round are based on answers by the 30-day recall only, two assumptions are necessary for such a conclusion to be valid. First, that dropping the 30-day question for clothing etc., and relying only on the 365-day recall, has made no difference. Secondly, that the presence of the one-week questions for food in the 55th round questionnaire has not altered responses to the 30-day questions. In other words, the comparability of the 55th Round to earlier rounds requires assuming that its results using the 30-day recall for food are equivalent to those by the Type 1 schedule used in Rounds 51 to 54.

However, under this assumption, a comparison of results obtained from the “one week” and “30 day” recalls in the 55th round also constitutes a test of the crucial issue of whether the presence of questions by both reference periods in this round has contaminated responses. This is so, since with the one-week recall for food the 55th Round reference periods are exactly the same as those in the Type 2 schedule of Rounds 51 to 54. If there was no contamination, the poverty incidence calculated from the one week recall would have been much lower than by the ‘30 day’ recall, exactly as found on comparing the Type 1 and Type 2 schedules canvassed experimentally during Rounds 51 to 54. On the basis of the results of those experiments, a 27.4 per cent rural poverty incidence by the Type 1 schedule should imply an incidence of only 12 to 14 per cent by the Type 2 schedule independently canvassed; while for urban areas a poverty incidence of 25.2 per cent by the Type 1 schedule should similarly correspond to only 10 to 13 per cent by the Type 2 schedule^[13].

Had the poverty incidence by the “one week” recall in the 55th round been at these low benchmark levels derived from the earlier experiments, this round could have been deemed a success in terms of what it had set out to do. The differences in consumption estimates by the two recalls observed in the “thin sample” Rounds 51 to 54 would then have been maintained into the 55th Round large sample, and seen to be invariant to whether these were canvassed independently or together. This would have clinched the issue that there is a systematic bias on account of recall. Provided, further, that there was some independent basis to argue that estimates of food consumption by the one week reference period were closer to the truth, a strong case could be made to adopt this reference period for future surveys, while adjusting downwards the poverty estimates of all past years. Such adjustment to past data would have been far from easy. And, much of the very large body of research carried out in the past using NSS data in a number of areas, for example in demand analysis, would be rendered useless without any possibility of further time-series work on these topics for several years. Nonetheless, the decision to opt for the one-week reference period would then have had some vindication.

But, given the very small difference between poverty estimates actually found by the two recalls, the 55th Round must be judged to be a failed experiment, on precisely these grounds. Although the 55th Round does show a significant *increase* in real mean consumption compared to Rounds 51 to 53 by the 30 day recall in both rural and urban areas, this is found to *decline* when comparisons are made using data by the 7 day recall. In the earlier experimental rounds, the one week recall had consistently shown about 30 per cent higher consumption of food, beverages, pan, tobacco and intoxicants than by the 30 day recall. But the 55th Round shows a difference of only about 5 to 6 per cent (Table 5). Thus, either the presence of the one-week question has biased upward the one-month estimates, or the presence of the one-month question has biased down the one-week replies, or, as is most likely, answers to both sets of questions have been influenced by the presence of the other. There is, therefore, strong evidence that there was contamination across responses by different reference periods and that, as a consequence, the 55th Round results, even by the one month recall, are not comparable with the 50th and earlier rounds.

TABLE 5: Some Comparisons with results of the 55th Round, sub-samples 1 and 2

		ROUND	50th	51st	52nd	53rd	55 th
Real per capita mean consumption at 1993-94 prices	Urban	Type 1	458	464	502	480	524
		Type 2		549	568	548	537
	Rural	Type 1	281	277	280	294	303
		Type 2		316	329	331	314
Ratio of the one week to one month estimate of consumption of food etc.	Urban		1.35	1.32	1.33	1.05	
	Rural		1.31	1.33	1.28	1.06	
Share of Bottom 40 per cent of population in total consumption	Urban	Type 1	19.9	18.8	18.8	19.2	19.8
		Type 2		21.0	21.3	21.4	19.9
	Rural	Type 1	23.1	22.7	23.4	22.3	24.2
		Type 2		24.7	25.0	24.2	24.1
Share of Top 20 per cent of population in total consumption	Urban	Type 1	42.8	45.4	45.8	44.5	42.2
		Type 2		40.2	40.1	40.2	42.1
	Rural	Type 1	38.4	39.9	38.1	39.7	36.6
		Type 2		35.3	35.0	35.5	36.3

Source: Computed from NSSO (2000b): Report No. 453

Similarly, while the distribution by the Type 1 schedule had consistently exhibited greater inequality than by the Type 2 schedule in the earlier experimental rounds, this is not observed on comparing the 55th Round distributions using the one week and 30 day recalls. In this case, there is some basis for assessing the direction of bias, and, interestingly, this is different in the rural and urban areas. In both areas, the 55th Round results by the two recalls are very close to each other, and intermediate between the results by the different schedules in Rounds 51 to 54. In urban areas the 55th Round distribution appears closer to the Type 1 schedule of the experimental rounds and similar to that in the 50th Round. For rural India, on the other hand, the evidence seems to suggest quite strongly that the 55th Round distribution is closer to that by the Type 2 schedules of Rounds 51 to 54, whereas the 50th Round distribution was closer to that by the Type 1 schedule. Consequently, the consumption share of the bottom 40 per cent of the rural population by the one-month recall of the 55th round would be overestimated by at least 5 per cent.

Most importantly, on the basis of the only comparable results from the 55th round, there is a strong *case* for the claim that *poverty was higher during July-December 1999 than during 1993-94*. This is by the results from the one week recall for food etc., using which the 55th Round reference periods are exactly the same as in the Type 2 schedules used in Rounds 51 to 54. By these identical reference periods, the poverty estimates from the 55th Round are higher than all the estimates obtained from NSS Rounds 51 to 54. Moreover, poverty during at least three out of four of these thin sample rounds was higher than during the 50th Round by the mutually exactly comparable 30-day recall^[14].

However, although more valid conceptually than the claim of reduced poverty, any claim of increased poverty during the 55th Round must also be inconclusive since there is the possibility of the one week recall being contaminated by the presence of the 30 day question. The essential problem is that of the comparability of the 55th Round with past rounds. The real matter of concern is that these contradictory indications by the two recall periods, and the strong evidence of contamination of both by the presence of the other, may push future discussion of trends in poverty into a statistical minefield which can only erode the credibility of the NSS. Moreover, since the question of what has happened to poverty is important not only to social scientists but to politicians as well, an unseemly debate could be set off with different camps arguing not just about how exactly the data should be interpreted but also about the motives behind the sudden change in methodology.

On interpretation, there are bound to be differences regarding by how much the 7-day questions have distorted the one-month response, and vice-versa. But these will be impossible to settle with the 55th Round data. And much ink may be spilt on issues, which may not really be germane, such as the way in which the questionnaire was designed or the order in which the questions were asked. Indeed, Bhalla and Visaria have already raised the issue of the order in which questions by the one week and one month recall were asked. Apparently, the NSSO had decided to ask the one-month question first, but this order got reversed in some cases because of late issue of instructions.

This might not, however, be of much importance since the pressure for consistency across questions in a survey can arise irrespective of the order in which questions are asked and can lead respondents to revise their earlier answers. In any case, *the "one week" and "one month" questions were solicited in the 55th Round questionnaire for each commodity in two separate columns on the same page, with the 7-day response to be entered in the left column*. Given

that English and most Indian languages are read from left to right, it is possible and even likely that the one-week question was asked first even if investigators were instructed otherwise. Unless it is asserted that the NSSO has so much authority that instructions from Delhi always override instincts of individuals in the field, there is no real basis to evaluate whether contamination was less from the one-week to the one-month recall, or the other way around.

Notwithstanding this, it is very important not to question past decisions taken by the NSSO Governing Council on operational matters concerning the actual conduct of surveys. It should be assumed that these were taken after full consideration of the statistical issues involved and implemented accordingly, subject of course to the usual administrative constraints and pressures. Reopening these will not improve the data but might embroil the NSSO in unnecessary controversy on matters which are best left internal⁵.

Things would, of course, be much worse if this becomes a political issue. There are then likely to be accusations that reference periods were changed in order to fudge results. And, in turn, there may be pressure to explain the contamination now confirmed by arguing either that *no inferences are possible from the thin samples*, or that the 55th Round results show that *the choice of reference period does not matter*. This would be untrue, and totally unscientific, negating not only the value of previous experiments but also the rationale for the change in reference period. Whatever the immediate outcome of such uninformed political intervention, this would in the longer run risk destroying an institution which is not only of extreme national importance but also one among the few government organisations in India which currently enjoys almost unparalleled world repute as regards both its competence and its integrity.

The only way out of this difficult and potentially dangerous situation for the credibility of the country's statistical system is to be honest and transparent. It should be obvious to all serious analysts that changes in the reference periods used in the 55th Round were taken in good faith as part of a larger experiment, but that this has turned out to be a failure. For this reason, the credibility of the NSSO can be salvaged if the experimental nature of the 55th Round is stressed, and all data available from it is released for independent research. However, another Consumer Expenditure Survey using a large sample will need to be conducted as soon as possible to give results which are comparable with previous rounds, while incorporating whatever valid lessons might have been learnt in this and the experimental surveys.

In this context, of the design of future NSS rounds, certain other results from the experiments conducted during Rounds 51 to 54 should be noted. First, estimates of consumer expenditure obtained from the Type 2 schedule were found to be sensitive to the positioning of the survey date within a month, with higher consumption reported in the first week of the month. There is thus an additional source of sampling error in the one-week consumption estimates. To avoid this, samples would need to be drawn uniformly over weeks, and not just uniformly over months. To achieve this, without sacrificing statistical reliability at the state and lower levels, *a substantial increase in sample size would be required, involving a corresponding large increase in survey cost*. Moreover, this also raises the question of how to interpret the resulting distribution of consumption across households, since selecting samples uniformly over weeks might reduce the bias in the consumption estimates but at the same time introduce an additional bias in the distribution so obtained.

Second, and more important, interpenetrating sub-samples were drawn independently by each type of schedule during Rounds 52 and 53. These sub-sample differences enabled computation, separately from the Type 1 and Type 2 schedules, of the Relative Standard Errors (RSE) for most estimates obtained, thus allowing some conclusions regarding the relative sampling variability of estimates across schedules. These show: (a) that the RSE of the mean consumer expenditure estimates was higher in the case of almost every commodity in almost every state by the one week recall than by the 30 day recall, indicating that consumption estimates from the one week recall were statistically less reliable *in general* than corresponding estimates by the 30 day recall, although in the case of many food items, notably cereals, the RSE of mean consumption was low by both reference periods; (b) that the RSE of mean consumption of almost every commodity for which a 365 day recall was used, was lower by this longer reference period than by the 30 day recall; and (c) that the RSE of estimates of the proportion of households and persons falling in every expenditure class below the poverty line was found to be much higher by the Type 2 schedule than by the Type 1 schedule, indicating much lower statistical stability of the distribution obtained from the Type 2 schedule, at least at the lower tail (Table 6). This third point is perhaps the most important from the point of view of the reliability of poverty estimates.

TABLE 6: Relative Standard Errors of Estimated Distribution of Persons by MPCE Class

RURAL Expenditure Class	52nd Round		53rd Round		URBAN Expenditure Class	52nd Round		53rd Round	
	Type 1	Type 2	Type 1	Type 2		Type 1	Type 2	Type 1	Type 2
	0 – 120	12.9	30.6	28.5		39.4	0-160	14.3	23.6
120-140	12.4	21.3	18.8	28.7	160-190	9.5	20.5	11.1	21.3
140-165	7.3	16.7	17.2	18.5	190-230	7.2	15.7	7.4	16.1
165-190	5.5	9.6	9.1	14.7	230-265	6.5	10.5	6.7	11.9
190-210	6.0	8.5	9.7	14.1	265-310	4.4	7.2	5.2	9.8
210-235	4.8	6.8	8.2	11.2	310-355	4.5	6.4	5.1	11.5
235-265	4.1	5.0	6.9	9.8	355-410	4.4	5.5	4.8	6.3
265-300	3.6	4.7	6.5	6.1	410-490	3.7	4.4	4.7	5.9
300-355	3.4	3.8	5.4	5.9	490-605	3.3	3.2	3.8	4.1
355-455	3.7	3.0	4.1	3.9	605-825	3.6	3.0	4.3	3.5
455-560	5.2	4.1	5.3	6.1	825-1055	4.4	4.5	6.1	5.3
560+	4.9	4.2	5.0	5.4	1055+	4.4	4.9	6.4	5.5

Source: NSSO (2000a)

These in-survey diagnostics of the statistical reliability of samples obtained by different recalls had, therefore, indicated much greater stability of the distribution obtained from the Type 1 schedule used so far, and had also indicated lower errors with a longer reference period. Indeed, the NSSO (2000a) acknowledges the second point transparently, and cites this as a reason to chose the 365 day recall for clothing etc. But in the case of food and intoxicants, a different criterion was followed. After noting, correctly, that “the substantial and systematic differences between the week and month based estimates indicate that one or both methods are not depicting the real life situation”, it went on to make the judgement that “because the RSEs arising from schedule type 1 and schedule type 2 do not differ very substantially for these item groups (food, pan, tobacco and intoxicants), the RSE criterion does not really clinch the issue in favour of schedule type 1, and the question of bias becomes all-important.”

However, precisely because the matter of bias cannot be settled without an independent procedure to arrive at the “true” estimate, Mahalanobis and his associates had considered it necessary, almost fifty years ago, to include through the methodology of independent and interpenetrating samples a test by physical weighing to provide the benchmark for judging bias in survey estimates simultaneously obtained by different reference periods. Although the NSSO does not report any such independent benchmark in the surveys recently carried out, it does note that the earlier survey by Mahalanobis had also obtained higher consumption by the “one week” recall, and that *“the present survey(s) can be taken as confirming the findings of the earlier survey because the data appear to follow the same pattern”*. A similar benchmark survey is an obvious priority for the NSSO.

Nonetheless, since no in-survey benchmark was available, the NSSO compared the consumption estimates from the two schedule types for food, beverages and tobacco taken together with the corresponding estimate of consumption from the NAS. At this aggregate level, the consumption of food, beverages and tobacco taken together was found to be 20 to 25 per cent lower than the NAS estimate by the one month recall, whereas the estimates by one week recall ranged from 6 per cent higher in 1995-96 to 2 per cent lower in 1997 as compared to NAS estimates for those years. It is on this basis that the NSSO concluded that “further methodological survey on this important subject would be advisable”, leading to the choice of reference periods used in the 55th Round.

It should, therefore, be evident that, quite apart from the very important issue of comparability with past NSS rounds raised thereby, *the inclusion of the “one week” questions in the 55th Round, and the effective changeover to the Type 2 schedule with its less reliable distribution, had no in-survey statistical warrant from the experiments conducted during Rounds 51 to 54*. The only justification for the one week recall was that this gave an estimate for aggregate expenditure on food, beverages and tobacco which was closer to that obtained from the CSO’s National Account Statistics than the corresponding estimate by the one month recall. However, even here, there is some inconsistency. The decision to adopt the 365-day recall for a number of items was taken despite earlier rounds indicating that this was likely to increase the even larger gap that exists between the NAS and NSS estimates for these items, and lead to a reduction in the measured inequality in NSS consumption data. These systematic differences between different recalls are important and deserved further analysis but, instead, the issue of the choice of reference period appears to have been linked inextricably to another old issue: that of comparisons of the NSS with the NAS data.

III. Comparing NSS and NAS estimates of Consumer Expenditure

With the change in reference periods made essentially because previous experimental rounds had indicated that a one-week reference period might give estimates of food consumption closer to those estimated in the National Accounts, the 55th Round has reopened an old question in an entirely new way. Differences between NSS and NAS consumption estimates have been the subject of much academic and official assessment ever since the 1950s. The system of official statistics has in the past incorporated results of such research by sometimes revising NAS estimates to correspond to the NSS. For a time poverty estimates were also anchored to the NAS by using a hybrid of NAS mean consumption and the NSS distribution. But never before has the NSS survey methodology been changed as a result.

The most recent official assessment of differences between the NSS and NAS estimates of consumption expenditure was in 1993, by the Expert Group on Estimation of Proportion and Number of Poor which had been constituted to review the official methodology for poverty estimation. At that time, official poverty measures were anchored to the NAS consumption estimates, by applying the NSS distribution to the NAS mean consumption. This practice was stopped because of the Expert Group's recommendation that poverty ratios be calculated exclusively from the NSS consumption data without any adjustment for the discrepancy between these and the NAS.

In this context, the Expert Group had noted that "The NAS estimate of private consumption is derived as a residual by deducting from estimated production of the various goods and services (adjusted for foreign trade), the estimated use for capital formation and public consumption. Apart from the lack of reliable direct data on production for a sizeable segment of the economy, the adjustments for uses other than private consumption are based on scanty data, often of the distant past, and subjective judgements". It went on to state that "NSS data are of course not free from errors, biases, comparability over time and other problems. The nature of these have been widely debated and there is a sustained effort to refine and improve the survey design and procedure. Even as these efforts continue - as of course they must - the NSS remains the best available source of assessing poverty incidence and the characteristics of the poor across space and time." In this, the Expert Group had put special emphasis on the fact that the NSS surveys "are carefully organised and use uniform concepts" (Planning Commission, 1993).

In view of this rather categorical assessment, the NSSO's recent decision to change the reference periods in the 55th Round, and to thus disturb the uniformity of NSS concepts, appears somewhat strange. This would at least have been understandable had the in-survey diagnostics from the experiments in Rounds 51 to 54 indicated much greater statistical reliability of the distribution obtained by the Type 2 schedule. But not only was there no such statistical warrant, the decision to include the one week recall appears to have been taken purely on the consideration that this would bring the NSS estimates of food consumption closer to those from the NAS. Since the official view so far has been that there is no strong basis to prefer the NAS estimates over those from the NSS, this is perhaps an indication of the extent to which the NSSO felt pressured from arguments that the NSS was increasingly underestimating consumption during the nineties, and thus overestimating poverty.

Ravallion (2000c) has drawn attention to a recent upsurge in criticism of the NSS consumption data by those who “have clearly been worried that the NSS-based poverty numbers will help fuel a backlash against economic reform in India”. He has reviewed their argument to re-anchor poverty measures to the NAS consumption estimates, and notes: (a) that the alternative estimates from the NAS are not free from error and, unlike the NSS, includes “non-household” consumption which might be increasing; (b) that some of the claims of increasing underestimation by the NSS, e.g. in case of cereals, are weak; (c) that the NSS misses out some of the rich, and hence the difference between the two consumption estimates could enlarge without this biasing poverty measures if an increasing proportion of measured NAS consumption is consumed by the rich; and (d) that the NSS surveys do indicate much faster increase in the consumption of the top quintile so that this is a distinct possibility. His basic conclusions are that there is no *a priori* reason to prefer the NAS mean consumption, and no basis to adjust poverty measures downwards on the assumption that errors (if any) in the NSS data are distribution-neutral. In response to the recent criticism, this is essentially an endorsement of the position taken in 1993 by the official Expert Group, but, curiously, Ravallion does not examine whether the gap between the NAS and NSS estimates of consumption has actually increased in recent years, accepting somewhat uncritically that it has.

However, the question of whether this gap between the NAS and the NSS has increased during the nineties is of rather crucial importance. This is because recent criticism of the NSS has at its starting point the observation that although GDP growth has increased somewhat during the nineties, and is reflected in the growth of real per capita private consumption expenditure from the NAS, measures of real per capita mean consumption obtained from the NSS show much lower growth during the nineties than during the seventies and eighties, particularly in rural India. This observation, which recent critics of the NSS have raised *ad nauseam* as evidence that something terrible must have gone wrong with the NSS during the nineties, appears to have convinced many who had earlier been in agreement with the Expert Group to swing around to the view that there was a need to reconsider the matter. This added weight to the opinion which appears to have led the NSSO to consider anchoring its estimates of food consumption to the NAS, paying less attention to the purely statistical in-sample results of its own experiments.

But did the gap between the NSS and NAS consumption estimates actually increase by so much after the Expert Group’s widely accepted observations to warrant this outcome? An important matter which seems to have been overlooked in this context is that the NSS measures only nominal consumption on the date of the survey and that estimates of real consumption are derived later by users of this data by deflating it with price indices which they chose to be most suitable for their purpose. Since the most important use of NSS data is to calculate poverty estimates, the deflators most commonly applied to NSS data are base weighted price indices constructed for population groups close to the poverty line, and these cannot be the same as the current weighted price indices for total consumption implicit in the NAS estimates. For this reason, a correct assessment of whether there was increasing divergence between the NAS and the NSS requires comparison, not of the derived measures of real consumption, but of the direct estimates of nominal consumption from the two sources.

On making this proper comparison, the striking result is that *there is no evidence of any large widening of the gap between the NAS and NSS estimates of nominal consumption during the 1990s*. The ratio of the NSS consumption expenditure to the corresponding estimate from the NAS (with 1980-81 as base) did fall from 0.82 to 0.69 between 1977-78 and 1990-91. But, during the subsequent years for which this NAS series is available, this ratio has remained more or less constant, varying in the range 0.68 to 0.72 (Table 7). Matters have been confused somewhat because the CSO has after 1998 shifted to a new series of national income with base year 1993-94, and estimates from 1997-98 onwards are available only on this basis. This has involved a revision of the NAS nominal consumption for 1993-94 by almost 16 per cent over the corresponding estimate according to the NAS series with 1980-81 as base, reducing the NSS to NAS ratio from 0.69 to 0.60 for that year. Some analysts failed to notice this break in the NAS series, and, by mistakenly attributing the resulting fall in the ratio to increased NSS underestimation, added fuel to the fire from the critics of the NSS¹⁶.

TABLE 7: Comparison of NSS and NAS Nominal Consumption

NSS Round	Year	NSS per capita monthly consumption (Rs/month)			Annual per capita consumption (current prices)			Ratio of NSS nominal consumption to	
		Rural	Urban	Total	NSS	NAS 80-81	NAS 93-94	NAS 80-81	NAS 93-94
27	Oct 72-Sep 73	44.17	63.33	48.11	585	744	907	0.79	0.65
32	Jul 77-Jun 78	68.89	96.15	74.89	911	1109	1352	0.82	0.67
38	Jan 83-Dec 83	112.45	164.03	124.72	1517	1967	2330	0.77	0.65
42	Jul 86-Jun 87	140.93	222.65	161.24	1962	2657	3092	0.74	0.63
43	Jul 87-Jun 88	158.10	249.93	181.19	2204	2938	3406	0.75	0.65
44	Jul 88-Jun 89	175.10	266.85	198.45	2414	3296	3818	0.73	0.63
45	Jul 89-Jun 90	189.46	298.00	217.37	2645	3639	4244	0.73	0.62
46	Jul 90-Jun 91	202.12	326.75	234.49	2853	4108	4764	0.69	0.60
48	Jan 92-Dec 92	247.21	398.95	287.24	3495	4871	5612	0.72	0.62
50	Jul 93-Jun 94	281.40	458.04	328.68	3999	5801	6680	0.69	0.60
51	Jul 94-Jun 95	309.41	508.07	363.08	4417	6505	7506	0.68	0.59
52	Jul 95-Jun 96	344.29	599.26	413.74	5034	7202	8523	0.70	0.59
53	Jan 97-Dec 97	395.01	645.44	465.00	5658	8178	10031	0.69	0.56

Sources: NSSO (various) and National Accounts Statistics (1998, 2000). The NAS data by financial years has been interpolated linearly to correspond to the period of NSS rounds

The new revised NAS series (with 1993-94 as base) does show higher consumption growth than the older series, and the ratio of the NSS to NAS mean consumption does fall somewhat in 1997. But, nonetheless, even by this, the increase in the discrepancy between the two consumption estimates during the nineties is modest compared to the increase which occurred between 1977-78 and 1990-91, especially by the old series to which the Expert Group had access when it decided on the matter. What is, however, most surprising about the new series are the revisions to the pre 1993-94 data which have only just been released¹⁷. These show much higher consumption during the seventies and eighties than earlier estimated, and as a consequence the discrepancy between the NAS and NSS is much higher in the earlier years by the new series than by the old and show a correspondingly lower subsequent increase. The oddity of this is that the NSS estimates for 1972-73 and 1977-78, which Minhas had earlier validated, are now seen to be gross under-estimates by the new NAS series¹⁸. Unfortunately, no commodity details of this series

have yet been released for the years before 1993-94 to judge what new insight the CSO now has about consumption in those years.

The nature of these revisions to the NAS, and also the issue of alternative deflators, will be discussed later. Here, the important point to note is that when the claim was made that the divergence between the NSS and the NAS had increased sharply during the nineties, there was absolutely no substance for this in the NAS series then available. The only significant increase in divergence between the NSS and the NAS during the nineties is by the new series for the single year 1997. And, even by this, the increased divergence is much smaller than what the Expert Group had noticed with the old NAS series when it had decided not to anchor poverty estimates to the NAS data.

To take the comparisons further, Table 8 presents the ratios of the NSS to NAS estimates of consumption by broad items over successive full-year NSS rounds beginning 1977-78. Since only the NAS estimates with base 1980-81 cover this entire period with full commodity breakdown, these ratios have been calculated with these NAS estimates rather than the new estimates with 1993-94 as base. As may be seen there are certain persistent differences between these two data sets at the level of individual items. Thus, for cereals, the ratio has always been close to unity but with some tendency to decline over time. For sugar, edible oils, fruits & vegetables, milk & products, and other goods & services, the NSS has consistently measured lower consumption but with no obvious time trend in these ratios. In the case of meat, fish & eggs, pan, tobacco & intoxicants, and clothing, NSS has lower consumption and the ratio has fallen over time. But, for pulses, other food and fuel & light, the NSS has consistently measured higher consumption than the NAS.

TABLE 8: Ratio of NSS nominal consumption by items to corresponding estimates from the NAS (1980-81 base)

	1977- 78	1983	1987- 88	1988- 89	1989- 90	1990- 91	1992	1993- 94	1994- 95	1995- 96	1997
Cereals	1.09	1.08	1.02	1.02	1.02	0.96	1.03	0.95	0.99	0.95	0.98
Pulses	1.09	1.12	1.29	1.17	1.24	1.23	1.30	1.17	1.27	1.41	1.50
Sugar	0.60	0.49	0.50	0.60	0.52	0.51	0.46	0.45	0.45	0.49	0.48
Edible oils	0.73	0.65	0.67	0.64	0.73	0.81	0.72	0.68	0.71	0.71	0.66
Fruits & vegetables	0.58	0.70	0.75	0.74	0.71	0.75	0.68	0.71	0.62	0.69	0.65
Milk & products	0.86	0.74	0.73	0.74	0.75	0.72	0.73	0.72	0.68	0.73	0.79
Meat, fish & eggs	0.96	0.77	0.68	0.71	0.67	0.64	0.60	0.51	0.48	0.49	0.48
Other food and beverages	1.22	1.34	1.59	1.56	1.54	1.49	1.57	1.46	1.28	1.16	1.03
Pan, tobacco etc.	0.51	0.52	0.62	0.63	0.60	0.58	0.52	0.51	0.45	0.38	0.35
Fuel & light	1.20	1.08	1.07	1.06	1.10	1.13	1.16	1.17	1.17	1.30	1.42
Clothing	0.61	0.63	0.46	0.50	0.44	0.34	0.43	0.36	0.43	0.49	0.47
Footwear	1.03	1.00	0.83	0.73	0.76	0.52	0.74	0.75	0.86	1.14	1.24
Other goods & services	0.62	0.52	0.53	0.49	0.49	0.47	0.50	0.53	0.52	0.56	0.56
Total	0.81	0.76	0.73	0.72	0.72	0.69	0.71	0.68	0.67	0.69	0.69

These persistent differences between NSS and NAS data have in the past been analysed by independent researchers, notably by B.S. Minhas and his associates¹⁹. Their detailed item by item comparison of consumption by the two sources, which is impossible for us to replicate, had led to validation of the NSS data despite the large apparent differences with the NAS. It was noted in this

past research that it is normal all over the world for items like intoxicants to be underreported by respondents, and something similar is probably true also for non-vegetarian items in a country such as India. Moreover, these researchers noted reasons to doubt the reliability of the NAS data for meat, egg and fish and for fruits and vegetables, estimates of which undergo persistently large revisions and there also appears to be some double counting. For sugar, edible oils and milk & products, a considerable proportion is purchased by hotels, restaurants and food manufacturers as intermediate goods to produce final goods purchased by households. A large part of such processed and prepared food appear differently in the NSS and NAS data, with the former including these under “other foods” while the latter includes them directly under the item concerned. This explains why the NSS measures higher expenditure under “other food”. The relative overestimation by the NSS of fuel & light has likewise been explained by failure of the NAS to adequately capture fuel wood collected directly by households.

Thus, for most of the above items, the differences are not particularly surprising or unexpected, especially given that the NSS leaves out “non-household” consumption such as in hostels, prisons and ceremonials and, unlike the NAS, does not impute any rental on owned residential dwellings. However, for certain items such as clothing and “miscellaneous non-food goods and services” the differences are large and have been attributed in past analysis both to a failure of the NAS to measure household consumption correctly and to a failure of the NSS to adequately capture the consumption of the relatively richer household who consume relatively more of these. In this context it should be noted that if indeed much of the underestimation of non-food items is due to the inability of the NSS to sample the rich, this would also have led to considerable underestimation of food consumption by the NSS.

With this background, it is possible to return to the issue of the NSS reference period to see to what extent the change in recall improves the correspondence between the NSS and NAS. Table 9 gives the absolute values of consumption estimates for 1995-96 from both the Type 1 and Type 2 NSS schedules and from both the NAS estimates with base 1980-81 and base 1993-94. For the new NAS series, the estimates for 1995-96 as shown by both NAS 1999 and NAS 2000 are also presented to give an idea of the revisions which continue even after considerable lag²⁰.

Comparing the estimates by the two NSS schedules with NAS (1980-81), it may be noticed that for all items of food, beverages and intoxicants taken together, the Type 1 NSS estimates are 20 per cent lower than the NAS, while the Type 2 NSS estimates are about 7 per cent higher. However, although at this aggregate level of comparison the Type 2 NSS schedule (with one week recall) is closer to the NAS, this greater apparent concordance is something of a statistical artefact. This is so since the one week recall gives higher estimates for all these items including for those, such as pulses and spices, where NSS estimates with the 30 day recall are much higher than the NAS estimate. As a consequence, if correspondence between the NSS and NAS for food, beverage and intoxicants is judged on the statistically correct basis of *absolute* or squared differences, this is in fact slightly better by the one month recall than by the one week recall²¹. Also the large gap between the NAS and NSS for non-food items by the usual 30 day schedule is even wider by the Type 2 schedule. Thus, even treating the NAS as benchmark, the 30 day NSS schedule cannot be judged unambiguously inferior to the alternative schedule on the basis of NAS estimates available at the time that the experimental results from Rounds 51 to 54 were obtained.

However, as discussed above, the CSO has revised its estimates of private consumption expenditure as part of constructing its new series of National Accounts with 1993-94 as base. For 1995-96, these new estimates are higher than the older NAS estimates with 1980-81 as base by about 16 per cent for total consumption and about 20 per cent for consumption of food, beverages and intoxicants. This has involved upward revisions for most items, with particularly large increases for fruits and vegetables, gross rent, medical services and miscellaneous goods and services. But for two items, “pan, tobacco and intoxicants” and “clothing”, the NAS has substantially revised downwards its consumption estimates. Although the net result is to increase the discrepancy between the NAS and NSS estimates of total consumption further, these revisions between the 1980-81 and 1993-94 series, particularly for fruits and vegetables and clothing, have the effect of increasing the divergence much more with the NSS Type 1 schedule than with the Type 2 schedule. In this sense, the revisions to NAS appear to have reflected to some extent the information obtained in these experimental NSS rounds. Nonetheless, as is evident from the changes made to the 1995-96 revised data between 1999 and 2000, these revisions are far from stable.

Thus, in the case of fruits and vegetables, sugar, edible oils, tobacco and clothing, the revisions earlier made while shifting base have been reversed within a period of one year by varying extents. For example, the estimate of fruit and vegetable consumption was increased by as much as 117 per cent in the new series when it was first released, but this has been revised downward by about 9 per cent this year. Nonetheless, even now the NAS estimate for fruits and vegetables is three times the NSS 30 day estimate and almost double even the higher NSS one week estimate, making it highly suspect. Similarly, the original downward revision in the estimate for clothing was by as much as 43 per cent, although this has since been increased by 12 per cent, while the estimate for tobacco was first reduced by 33 per cent and then upped by 54 per cent. It, therefore, still remains true, as Minhas and Kansal (1989) had pointed out over a decade ago, that “the margin of uncertainty (error) in NAS estimates - uncertainty caused by subjective adjustments, methodological innovations and changes in production data - is uncomfortably too large to sustain a healthy degree of confidence in them.”

TABLE 9: NOMINAL CONSUMER EXPENDITURE ESTIMATES 1995-96 (Rs. Crore)

	National Accounts Statistics			National Sample Survey			
	1980-81		1993-94 base	Unadjusted		Adjusted for under-enumeration	
	NAS 1998	NAS 1999	NAS 2000	Type 1	Type 2	Type 1	Type 2
Cereals	90616	103671	102330	90714	103227	102592	125696
Pulses	11569	13602	13589	16844	25774	20448	34979
Sugars	21375	23216	21537	11009	14332	13170	18877
Edible oils	27848	29113	26892	20651	25872	25479	36848
Fruits & vegetables	46297	100674	91733	33547	53232	44340	83350
Milk & products	59046	63326	62283	44116	51315	58597	81912
Meat, fish & eggs	29783	30857	30957	14917	23551	19425	35035
Spices & salt	8158	9677	9759	10344	17906	12279	23950
Other food & beverages	23499	26758	25231	21653	34044	34342	68471
Pan and intoxicants	5275	3708	3644	4957	7652	6283	10952
Tobacco	17347	11706	18046	6268	8008	7608	11059
Total food, beverages etc.	340813	416308	406001	275113	365064	344654	531281
Clothing	69453	39879	44503	35119	34168	55450	50419
Footwear	4592	4646	4693	5500	5119	9132	8595
Rent	33775	54004	58953	9223	11086	19065	31732

Fuels	24270	25745	25647	32070	30360	40240	43683
Medical	14222	30207	30207	18435	22042	26939	37652
Education	12998	13466	13466	16771	12792	32416	27703
Durable goods	18529	18896	18698	24615	12450	49558	27426
Other goods and services	130442	154233	156489	51938	49874	79968	98226
Total non-food excluding rent	274506	287072	293703	184448	166806	293703	293703
Total consumption	649094	757384	758657	468785	542956	657422	856716

Sources: National Accounts Statistics (1998, 1999, 2000) and NSSO (2000a)

With recent revisions not inspiring greater confidence in the NAS than what had been concluded from earlier research to validate the NSS, there are obvious problems in treating the NAS as benchmark. But, from the point of view of the choice of reference period in the NSS, there is a further problem in the validity of such comparisons. Since a large gap exists between the NSS and NAS estimates of non-food consumption, and since this is wider with the Type 2 schedule than with the Type 1 schedule and widens further if NAS 1993-94 rather than NSS 1980-81 is considered, there is a strong presumption that the NSS does under-enumerate the rich systematically. If so, tests of consistency between the NAS and NSS, and judgements about the best reference period for the NSS, must take into account explicitly the implications of such under-enumeration and also the fact that “non-household” private consumption is not covered by the NSS. Any underestimation of consumption resulting from these would not only have biased the NSS estimates of non-food consumption but also of food consumption.

The last two columns of Table 9 give details of a synthetic construction from the NSS data for 1995-96, separately for the Type 1 and Type 2 schedules, to give an idea of what the NSS estimates are likely to be if these are adjusted for such underestimation. For this, it is assumed that the difference between the NSS and NAS estimates of total consumption of non-food items (other than pan, tobacco & intoxicants and gross rent) arises entirely because of under-enumeration or non-coverage. And, using this as a controlling total, the item-wise NSS estimates from both schedules have been adjusted upwards, making the further assumption that the commodity composition of the underestimated consumption is the same as that of the top 15 per cent of the urban population from the relevant NSS schedule^[22]. Thus, each of these adjusted estimates have a total consumption for non-food (other than pan, tobacco & intoxicants and gross rent) which is the same as in NAS 1993-94, but the estimates of food consumption, as also the item-wise distribution of non-food consumption, are derived entirely from the NSS. The purpose is to arrive at NSS-based estimates of food consumption which are consistent with the assumption that the unexplained non-food consumption is due to under-enumeration of the rich and non-coverage of “non-household” private consumption.

Comparing these to the NAS shows that, as far as total food consumption is concerned, the adjusted estimates from the Type 1 schedule are fairly close to the NAS, while the adjusted estimates from the Type 2 schedule are considerably higher. The total consumption of food, beverages and intoxicants by the adjusted one month recall is 1 per cent higher than in NAS 1980-81 and 15 per cent lower than in NAS 1993-94; while by the adjusted one week recall this is 56 and 31 per cent higher than the two NAS estimates. There are, of course, even larger differences at the individual commodity level, but these are within plausible levels with the one month recall, especially if allowance is made for differences in the way the NSS and NAS treat items such as sugar which are used to produce other food and if it is agreed that the estimate for fruits and

vegetables in the NAS 1993-94 might be an overestimate. On the other hand, the adjusted estimates by the Type 2 schedule are much higher compared to both the NAS 1980-81 and the NAS 1993-94 for every item except sugar and fruits and vegetables. This suggests that Mahalanobis may in fact have been correct to suspect gross overestimation by the one-week recall prevalent elsewhere and to prefer the one-month response.

Thus, allowing for under-enumeration and non-coverage, there is a much stronger case for continuing with the one month recall for food than to adopt the one week recall, especially if it is accepted that the large revision to “fruits and vegetables” in NAS 1993-94 is suspect. The revised estimate for fruits and vegetables is 133 per cent higher than the adjusted Type 1 estimate and 10 per cent higher than the adjusted Type 2 NSS estimate. Similarly, the revision to clothing in NAS 1993-94 deserves a re-look, and there also appears to be considerable difference between the NAS and the NSS in the definition of which goods are “durable”.

None of this can, of course, be taken as settling either the issue of the most appropriate recall period for the NSS, or of the precise reasons for differences between the NAS and the NSS. There *is* evidence that the NSS does underestimate consumption, but it is unclear to what extent this is due to poor recall as against an under-enumeration of the rich. Both of these would lead to the NSS overestimating poverty, although this would be much more if errors of recall dominate over those due to under-enumeration. Also, these two sources of error have very different implications for demand estimation. If under-enumeration were the main problem, the NSS would be expected to underestimate the income elasticity of relative luxuries. On the other hand, accepting the reference periods used in the 55th round would require upward revision to the income elasticity for food derived from the 30-day recall and a downward revision to the elasticity for non-food items. Since the NSS is the most important source of data for demand assessment, ignoring these to concentrate only on the implications for poverty may lead to inappropriate policy input. Clearly, there is considerable need for further research.

Nonetheless, some conclusions are possible. First, that if the validity of the NSS reference period is tested by comparing with the NAS, then the one month recall for food in the NSS is not necessarily inferior to the one week recall. On making allowance for the fact that the NSS does under-enumerate the rich, it is in fact superior. Second, that the validation of the NSS by the NAS is itself a suspect procedure because of frequent and large revisions to the NAS, and that, in this context, there is a strong case to re-examine the revised NAS 1993-94, especially for “fruits and vegetables” and clothing. Moreover, there is the important observation that although there is a fairly large difference between the NSS and NAS estimates of total consumer expenditure, this divergence did not increase significantly during the nineties. On the basis of these, it can be concluded that *not only was there no in-survey warrant from the earlier experimental rounds for the revised schedule used in the 55th Round, comparisons with the NAS also did not warrant the change in reference period leading to non-comparability with past data.*

IV. Consistency of estimates of growth, inequality and poverty

Notwithstanding all the above, some reservations can be expected from those who find it inconsistent that rural poverty has failed to decline during a period of fairly rapid economic growth. A priori, of course, there can be a lack of correspondence between trends in income growth and poverty incidence if inequality has increased, but this can also be a result of measurement problems. The argument so far would suggest increased inequality, but the presumption among many is that it is otherwise. This presumption is informed to a considerable extent by the findings of Ravallion and Datt (1992, 1996)²³, based on the widely used World Bank database on Poverty and Growth in India constructed by them with NSS data. They had found that almost the entire reduction in poverty in India during the 1970s and 1980s was due to growth rather than to redistribution. This was reinforced by Datt (1999) who concluded that poverty as measured from the NSS had failed to decline during the 1990s, not so much because of any increase in inequality as measured by the NSS but because measured rural consumption from the NSS failed to reflect the income growth in the NAS. This was widely interpreted to imply that there must have been measurement problems in the NSS which caused poverty to be overestimated during the nineties.

However, although Datt did not rule this out as a possibility, he had himself been rather more careful about the matter. He had noted that it was well known that the NSS under-enumerates the rich, and that, therefore, the income growth estimated by the NAS during the nineties might not have been captured fully by the NSS if this had accrued disproportionately to the under-enumerated rich. If so, the NSS would not only underestimate true income growth but also fail to measure the increase in true inequality. This possibility, that sampling biases at the upper tail of the distribution may cause the NSS to underestimate both the mean consumption and the inequality of distribution, was essentially why the Expert Group on Poverty Estimates had recommended against constructing hybrid poverty estimates using the NAS consumption mean and the NSS distribution. As discussed earlier, Ravallion (2000c) has also come out strongly against anchoring poverty estimates to the NAS, noting that errors if any in the NSS are unlikely to be distribution-neutral, buttressing this by providing evidence from within the NSS of a significant increase in inequality during the nineties.

Thus, Ravallion-Datt have themselves diluted considerably the support claimed from their earlier research by those who argue that the existing NSS estimates overestimate poverty. But, nonetheless, their recent writings continue to suggest (a) that the gap between the NAS and NSS estimates of consumption expenditure has widened during the nineties, and (b) that although there is strong evidence of an increase in inequality at the national level during the nineties, this is entirely a result of an widening of the urban-rural gap and not of inequalities within either sector. Since such ambiguities can lead to persisting doubts on the credibility of the NSS data which show no decline in poverty despite measured income growth from the NAS, it is important to emphasise that both these suggestions are somewhat misleading.

On the first of these, i.e. the issue of divergence between the NAS and the NSS, it has already been shown that there is very little evidence of any widening of the gap between the NAS and NSS estimates of *nominal* consumption during the nineties. However, it is important to note that, despite this, the *real* per capita consumption calculated from these two sources do show differential movement during the nineties (Table 10). Real per capita consumption obtained directly from the two national accounts series have increased much more than the real consumption

estimates derived by Datt (1999) from the NSS. The latter shows an increase of less than 8 per cent between 1990-91 and 1997 against increases of 18 and 20 per cent according to the old and new NAS series.

With this fairly large difference in movements of real consumption between the NAS and NSS occurring without any corresponding difference in movements of nominal consumption, the conclusion must be that the price deflators used to move from nominal to real consumption are of central importance in the so-called poverty puzzle. And, as is evident from Table 10, the Ravallion-Datt deflator did increase more during the 1990s than those implicit in the NAS estimates. Thus the ratio of the Ravallion-Datt deflator to the NAS 1980-81 deflator increased 9 per cent between 1990-91 and 1997, explaining almost totally the difference in the measured increase in real per capita consumption between the two series^[24].

TABLE 10: INDICES OF REAL PER CAPITA CONSUMPTION AND RELATIVE DEFLATORS

NSS Round	Year	Indices of Real Per capita Consumption					Ratio of Datt deflator to NAS deflator	
		National Sample Survey (Datt)			National Accounts		80-81 base	93-94 base
		Rural	Urban	Total	80-81 base	93-94 base	80-81 base	93-94 base
27	Oct 72-Sep 73	81.8	84.7	74.8	70.3	72.5	98.3	102.1
32	Jul 77-Jun 78	87.3	90.4	86.6	76.3	77.7	101.5	100.9
38	Jan 83-Dec 83	92.1	96.0	92.1	84.9	85.9	103.3	101.5
42	Jul 86-Jun 87	100.4	103.0	100.6	91.6	90.6	98.9	95.4
43	Jul 87-Jun 88	99.8	97.8	98.9	93.5	92.3	102.2	100.9
44	Jul 88-Jun 89	99.8	99.4	99.5	96.8	95.7	102.6	101.6
45	Jul 89-Jun 90	103.4	102.4	103.0	98.8	98.2	100.4	99.3
46	Jul 90-Jun 91	100.0	100.0	100.0	100.0	100.0	100.0	100.0
48	Jan 92-Dec 92	95.6	102.2	97.8	101.4	100.6	107.0	107.0
50	Jul 93-Jun 94	101.1	105.3	102.7	105.7	104.2	102.2	101.5
51	Jul 94-Jun 95	99.5	105.9	102.0	109.3	107.8	104.8	104.0
52	Jul 95-Jun 96	101.0	113.6	105.6	113.0	113.3	107.6	105.9
53	Jan 97-Dec 97	106.0	109.3	107.6	118.3	120.1	109.2	105.1

Sources: Computed from Datt (1999) and National Accounts Statistics (1998, 2000)

It is important to recognise that this difference between the two deflators is not due to any measurement error. The implicit deflators from the NAS relate to the consumption basket of the nation as a whole and is properly measured as such by the CSO. On the other hand, the Ravallion-Datt deflators are for poverty calculations, and are again properly constructed to reflect more closely the consumption basket near the poverty line. There may well be errors in the construction of National Accounts, and differences can exist on how to construct the best deflator for the poverty line, but conceptually the latter cannot be the same as the NAS deflator and errors in these, if any, have nothing to do with the NSS data^[25].

In practice, the deflators used to convert NSS rural and urban nominal expenditures into real trends are based on the official consumer price indices for agricultural labourers (CPIAL) and industrial workers (CPI-IW). Both these consumer price indices have increased faster than the implicit NAS consumption deflators during the 1990s, essentially because food items have a larger weight in the indices of consumer prices and because prices of various food items, particularly cereals, have increased much more during the nineties than the prices of other items which are

consumed more by the rich (Table 11). This has reversed a past trend. Thus, the main reason why there is a stagnation in per capita real consumption according to the Ravallion-Datt series (as indeed in all similar series derived from the NSS to calculate poverty), despite the NAS showing fairly substantial growth in this, is because of the reality that food prices have increased faster during the nineties than the prices of other goods. At least in this purely statistical sense *the increase in the relative price of food, particularly of cereals, is a major factor explaining why the incidence of poverty did not decline during the 1990s despite an increase in the mean real per capita consumption as measured by the NAS.*

TABLE 11: Ratio of various price indices to the Wholesale Price Index (all commodities)

	WPI cereal	CPIAL food	CPIAL general	NAS (1993-94) consumption deflator	CPIAL non-food
1977-78	119.3	109.3	109.2	101.3	108.6
1978-79	116.6	105.8	106.8	104.0	111.9
1979-80	109.5	99.4	99.6	97.6	100.2
1980-81	104.0	96.8	96.0	91.4	91.7
1981-82	106.5	102.6	101.0	93.9	92.5
1982-83	113.2	102.4	101.3	95.3	95.2
1983-84	114.1	106.4	106.5	97.1	106.9
1984-85	101.9	99.1	98.7	97.5	96.6
1985-86	103.9	99.1	99.0	98.1	98.9
1986-87	104.0	97.9	98.1	99.8	98.9
1987-88	103.4	99.7	99.7	99.6	99.5
1988-89	107.3	105.3	104.2	100.2	98.5
1989-90	102.2	102.9	102.6	100.8	100.8
1990-91	100.0	100.0	100.0	100.0	100.0
1991-92	108.4	106.0	104.9	99.2	98.7
1992-93	111.8	108.8	107.2	98.7	98.6
1993-94	108.9	103.0	102.3	100.1	98.4
1994-95	109.4	104.2	103.3	98.4	98.5
1995-96	107.7	107.0	106.1	98.4	101.0
1996-97	114.7	110.8	109.0	100.6	99.3
1997-98	113.2	107.4	107.3	101.7	106.7
1998-99	114.8	118.4	116.0	103.8	103.0
1999-00	126.0	117.6	114.9	NA	100.3

On the second issue, of inequality, it is important to note that, in the presence of these significant shifts in relative prices during the nineties, Gini coefficients calculated from the distribution of nominal consumption are only partial indicators of true inequality. Even with unchanged distribution of nominal consumption, the higher relative price of foodgrains would have increased real inequality (as measured by any welfare measure) in both rural and urban areas, while simultaneously *reducing* the gap between rural and urban incomes. This needs to be emphasised since advocates of anchoring poverty estimates to the NAS have claimed that inequality has not increased, and because Ravallion's disclaimer to this, pointing out that there *was* an increase in inequality at the national level, is a qualified one. This stresses the increase in rural-urban disparity during the nineties but not the trends in inequality in either the rural or urban areas.

In fact, the Gini coefficients calculated by Ravallion and Datt from the NSS distribution of nominal consumption show a clear, and statistically significant, reversal during the nineties of an earlier trend of declining rural inequality between 1977-78 and 1990-91^[26]. Since then, this has fluctuated wildly around a *rising* trend, exhibiting movements very similar to that of the national inequality (Table 12). Urban inequality, which had no trend earlier, also shows a statistically significant increase during the nineties. Thus, even ignoring the strong possibility of the NSS missing out on increasing consumption by the rich which it does under-enumerate, there is clear evidence that inequality increased during the nineties in three different ways. First, the differential impact on the poor of the increase in the relative price of food; second, the increased inequality of nominal consumption in urban areas and the reversal during the nineties of an earlier trend of declining inequality in rural areas; and, third, as Ravallion has correctly emphasised, the increased disparity between urban and rural areas.

TABLE 12: Some indicators of Inequality

NSS round	Year	Gini Coefficients (Datt-Ravallion)			Ratio of urban to rural mean consumption	
		Rural	Urban	National	Nominal	1973-74 prices
27	Oct 72-Sep 73	30.67	34.70		1.43	1.41
32	Jul 77-Jun 78	30.92	34.71		1.40	1.41
38	Jan 83-Dec 83	30.10	34.08	32.06	1.46	1.42
42	Jul 86-Jun 87	30.22	36.75	33.68	1.58	1.40
43	Jul 87-Jun 88	29.39	34.64	33.08	1.58	1.34
44	Jul 88-Jun 89	29.51	34.80	32.93	1.52	1.36
45	Jul 89-Jun 90	28.23	35.59	31.84	1.57	1.35
46	Jul 90-Jun 91	27.72	33.98	31.21	1.62	1.37
48	Jan 92-Dec 92	29.88	35.11	34.31	1.61	1.46
50	Jul 93-Jun 94	28.58	34.34	31.52	1.63	1.42
51	Jul 94-Jun 95	30.17	37.18	36.32	1.64	1.45
52	Jul 95-Jun 96	28.43	35.53	32.86	1.74	1.54
53	Jan 97-Dec 97	30.56	36.54	37.83	1.63	1.41

Source: Datt (1999) and Ravallion (2000c)

This last point is particularly important because a noteworthy feature of the trend revealed by the NSS during the 1990s is that the absence of any reduction in poverty is confined only to the rural areas. The NSS ratio of urban to rural mean per capita consumption has increased during the 1990s in both nominal and real terms. Although this increase in nominal terms continues a trend observed during the previous two decades, the increase in real disparity reverses an earlier decreasing trend. Thus, according to Ravallion-Datt estimates from the NSS, the real per capita consumption in rural areas, which had increased 14 per cent between 1977-78 and 1987-88, averaged only 2 per cent higher during 1993-94 to 1997 than the average for 1987-88 to 1990-91. The corresponding figures for urban areas are 8 and 9 per cent respectively. Even in 1997, when the rural-urban gap had closed considerably and rural real consumption reached its highest ever level, this was less than 6 per cent higher than during 1990-91. Moreover, according to the NSS, the share of the bottom 40 per cent of the rural population declined 5 per cent to 22.3 per cent in 1997 from 23.4 per cent in 1990-91. As a result, the real consumption of the bottom 40 per cent of the rural population in 1997 was virtually unchanged from that in 1990-91.

Thus, the picture from the NSS is quite clear. Rural poverty did not decline during the nineties both because there was somewhat higher inequality within the rural sector and because the rural real mean consumption lagged behind its urban counterpart, causing negligible or even negative growth in the real consumption of the bottom 40 per cent of the rural population. The growth of overall mean consumption as measured by the NSS has already been validated against that revealed by the NAS. But, in order to clinch the matter, it is necessary to examine whether the NAS is consistent with the Ravallion-Datt result from the NSS that per capita *rural* consumption grew by only around 6 per cent between 1990-91 and 1997, as against a growth of around 20 per cent in real national per capita consumption according to the NAS. For this, it is useful to consider measures of rural output and incomes, remembering that there does exist a strong likelihood of the NSS underestimating increases in the incomes of the rich, and that if anything, consumption is likely to have lagged behind income. Even those who may doubt whether inequality has increased would agree that rural poverty is unlikely to have declined if average rural per capita incomes have stagnated.

TABLE 13: Indices of Rural Per capita Output and Incomes

	Indices of agricultural output per capita of rural population			Indices of Rural income per capita		
	IAP	GDP from Agriculture etc.		NAS 1980-81 deflated by		NAS 1993-94 CPIAL deflated
		1980-81 base	1993-94 base	Output price	CPIAL	
1977-78	86.3	89.0		73.0	61.0	
1978-79	87.7	89.4		74.7	64.6	
1979-80	72.6	76.5		67.2	62.0	
1980-81	82.1	84.8	85.8	72.2	65.1	
1981-82	86.5	88.5	88.9	75.4	65.8	
1982-83	81.4	85.7	86.7	74.9	67.1	
1983-84	90.5	93.3	93.2	80.4	70.1	
1984-85	88.4	91.6	92.8	80.9	75.5	
1985-86	88.0	90.2	91.7	81.7	78.2	
1986-87	83.3	87.1	89.5	82.1	80.6	
1987-88	81.9	85.8	86.6	83.2	81.4	
1988-89	97.6	98.1	98.1	93.2	87.1	
1989-90	98.0	98.0	97.8	97.5	94.1	
1990-91	100.0	100.0	100.0	100.0	100.0	100.0
1991-92	96.5	96.1	96.8	94.2	91.7	91.7
1992-93	99.0	100.4	100.7	98.2	91.5	91.5
1993-94	100.8	102.2	102.7	101.2	100.4	100.4
1994-95	105.0	105.8	106.2	105.7	103.0	102.3
1995-96	100.6	100.8	103.5	106.5	101.9	102.1
1996-97	107.9	107.3	111.9	111.6	103.1	107.0
1997-98	101.0	103.5	108.3	110.6	103.7	108.8
1998-99	107.6		114.5			115.5
1999-00	104.6		114.5			

Source: Chandrasekhar and Ghosh (2000) for rural income estimates

The easiest comparison in this context is with measures of agricultural income and output per head of rural population. Table 13 gives indices of agricultural output per capita of rural population computed from the official Index Numbers of Agricultural Production (IAP) and from the CSO's constant price estimates of the Gross Domestic Product in Agriculture, Forestry and Fishing, by both the series with base 1980-81 and 1993-94. The IAP shows that per capita agricultural output

during the nineties peaked in 1996-97 (when there was no NSS survey) at a level less than 8 per cent higher than in 1990-91, with the average for 1993-98 only 3 per cent higher than in 1990-91. A more or less similar picture is obtained from the GDP series with 1980-81 as base, according to which agricultural income per capita of rural population averaged only 4 per cent higher during 1993-98 than in 1990-91, with a peak in 1996-97 which was 7 per cent higher. However, the series with 1993-94 as base shows better performance, with per capita agricultural income 12 per cent higher in 1996-97 than in 1990-91 and the average for 1993-98 higher by 6 per cent.

TABLE 14: Indices of Rural Per capita Output and Incomes

Year	Index of NSS real rural consumption	Indices of agricultural output per capita of rural population		Indices of Rural income per capita				
		IAP	GDP from Agriculture		NAS 1980-81 deflated by		NAS 93-94 CPIAL deflated	
			80-81 base	93-94 base	Output price	CPIAL		
Jul 77-Jun 78	87.3	87.5	90.0			74.5	63.2	
Jan 83-Dec 83	92.1	89.0	92.3	92.3		80.2	70.8	
Jul 86-Jun 87	100.4	83.7	87.6	89.5		83.6	82.5	
Jul 87-Jun 88	99.8	86.6	89.8	90.2		87.0	84.6	
Jul 88-Jun 89	99.8	98.6	99.0	98.8		95.7	90.7	
Jul 89-Jun 90	103.4	99.4	99.5	99.2		99.6	97.6	
Jul 90-Jun 91	100.0	100.0	100.0	100.0		100.0	100.0	100.0
Jan 92-Dec 92	95.6	99.2	100.3	100.5		98.6	93.5	93.5
Jul 93-Jun 94	101.1	102.8	104.1	104.4		103.9	103.2	103.0
Jul 94-Jun 95	99.5	104.9	105.6	106.3		107.5	104.9	104.4
Jul 95-Jun 96	101.0	103.3	103.5	106.4		109.3	104.3	105.5
Jan 97-Dec 97	106.0	103.6	105.5	110.0		112.5	105.8	110.6

On re-tabulating the data to correspond to the NSS rounds (Table 14), both the IAP and NAS 1980-81 show somewhat lower growth between 1990-91 and 1997 than the NSS. Also, although the NAS 1993-94 does show higher growth, this is not so seriously out of line as to be outside the range of error likely to be caused by under-enumeration of the rich. Thus, *the NSS-based estimates of the growth of rural consumption are not out of line with agricultural growth*. Nonetheless, it is important to note the difference between the two NAS series. While agricultural growth is similar by the old GDP series and the IAP, the new series diverges considerably, especially after 1996-97. On the basis of provisional estimates for 1999-2000, per capita agricultural output by this series is 14.5 per cent higher than in 1990-91 compared to an increase of only 4.6 per cent by the IAP. Further, it can be argued that agricultural growth is an insufficient measure of the growth of rural incomes. Non-agricultural incomes have percolated to rural areas in the past, particularly during 1977 to 1987 when per capita agricultural output and income was stagnant but poverty declined significantly.

The first of these, i.e. the revision of CSO's National accounts, is interesting since almost the entire increase in both the level and the rate of growth of agricultural output and incomes is because the coverage of fruits, vegetables and floriculture has been enlarged in the new NAS series with 1993-94 as base. The data source for output of these crops has been shifted partly from the Directorate of Economics and Statistics, Ministry of Agriculture (DESAg) to the National Horticulture Board (NHB), and the prices used to calculate value of horticultural output have also

been revised. For 1993-94, these revisions have led to an increase in the value of horticultural output by over 60 per cent, leading to an upward revision of total agricultural GDP by about 7 per cent. Also, the rate of growth of horticultural output has been put at more than double the rate of growth of remaining crops, so that about 35 per cent of the total increase in the value of crop output between 1993-94 and 1998-99 is attributed to fruits and vegetables alone. Fruits and vegetables are thus currently estimated to account for over 25 per cent of the value of total crop output (almost the same as that of rice and wheat put together), although grown on only 4.5 per cent of crop area.

These revisions were made because it was felt that the DESAg was missing out on a considerable part of horticultural production. But, although there is certainly evidence that changing patterns of demand *have* caused significant diversification of agriculture during the nineties, the magnitude of these revisions to fruits and vegetables output appear inexplicably large. Unlike the DESAg estimates of forecast crops for which a system of area statistics exists and scientific crop-cutting estimates are done to determine yields, the system of estimation for horticultural crops is weak. Reliance on NHB data has meant including some estimates of output which are based purely on seed distribution, while some of the prices used are inclusive of high trade and transport margins. In the earlier discussion, it has already been observed that the resulting revisions have led to estimates of consumption of fruits and vegetable which are totally out of line with the NSS data. Accepting these revisions would thus require reassessment of all recent research into factor productivity in Indian agriculture and of all earlier estimates of future demand. Such reassessment should of course be carried out if there is compelling evidence, but before this there is a need for the CSO to review its changed methodology for horticultural crops which leads to estimates of production and consumption that are totally out of line with other data. Pending such review, it is safer to rely on the data by 1980-81 as base.

As for the second point, that proper assessment of trends in rural incomes requires considering non-agricultural incomes as well, the problem is that, excepting for the two base years 1980-81 and 1993-94, the CSO does not give a break up of sectoral and total incomes by rural and urban areas. However, the rural-urban break-up of the sectoral employment estimates from the NSS, which the CSO now uses for the NAS, is available. On the basis of these, and the assumption that the growth of labour productivity is the same in rural and urban areas, Chandrasekhar and Ghosh (2000) have made some estimates of rural incomes, indices of which are also presented in Tables 13 and 14²⁷. It should, however, be noted that there is no independent evidence for the assumption of equal productivity growth, which would if anything exaggerate the growth of rural incomes.

These estimates suggest a sharp slowdown in the growth of per capita rural output and real incomes during the nineties. Such incomes can be measured in two ways: either using the implicit NAS deflators for or by deflating nominal income with the CPIAL as in the poverty calculations. The first method based on the NAS 1980-81 shows a decline in growth rate from 3.1 per cent per annum during the triennium ending (TE) 1980-81 to TE 1990-91 to 1.8 per cent per annum during TE 1990-91 to TE 1997-98. The second method, also based NAS 1980-81, shows an even larger decline, from 3.9 to 1.4 per cent per annum. As discussed earlier, the new NAS with base 1993-94 shows higher incomes and higher growth, but, if this is spliced backward to the old series, the result is still a decline in per capita rural income (CPIAL deflated) from 3.9 to 1.8 per cent. As against these, the residual estimates of per capita urban incomes show sharp acceleration in growth (increasing from less than 2.5 per cent per annum during TE 1980-81 to TE 1990-91 to almost 5

per cent during TE 1990-91 to TE 1997-98), confirming a very significant increase in rural-urban disparity.

One reason for this increased disparity is the fall in agriculture's share in GDP, but an even more significant reason is the trend in rural non-agricultural employment. According to NSS data, rural non-agricultural employment had increased from 36 million in 1977-78 to 66 million in 1989-90, but this fell sharply in 1991-92 and has since fluctuated at between 58 and 63 million. As a result, the rural share in non-agricultural employment, which had increased from 47 to 52 per cent between 1977-78 and 1989-90, has declined almost continually during the nineties to reach only 43 per cent in 1997. Because of the methodology adopted, this fall in employment share is reflected in the estimates of rural incomes above. These show fairly rapid growth during 1977-78 to 1987-88, explaining why NSS consumption increased significantly despite stagnation in per capita agricultural output. But, with productivity gains neutralised by declining non-agricultural employment thereafter, the rates of growth of real rural per capita incomes during the nineties are very similar to those of agricultural output.

Since NSS estimates of real consumption growth during the nineties have already been validated against the latter, the validation extends also to rural incomes including non-agriculture, especially using the CPIAL deflator as for the NSS. This implies basically that *the rapid growth of non-agriculture measured by the NAS at the national level during the nineties did not spill over into rural areas*. In conjunction with the small increase in intra-rural inequality measured by the NSS, this large increase in rural-urban disparity makes the *trends* in NSS estimates of consumption compatible with the NAS estimates of national income.

In fact, even the *level* of NSS rural consumption is not unreasonably low compared to the NAS estimate of rural income. As base for its new series, the NAS has estimated rural NDP per capita in 1993-94 at Rs 5783, of which Rs 3052 originates in agriculture. These are likely to be over-estimates for reasons already discussed, and are about 9 per cent higher than by the old series. But even compared to these, the corresponding NSS estimate of annual per capita rural consumption, at Rs 3424, is not implausible. In addition to savings by rural households, the NSS excludes imputed rental on rural dwellings (put at Rs 240 per capita in the NAS) and factor incomes originating in rural areas but appropriated elsewhere. The latter are not insignificant. On the basis of NAS sector-wise factor shares and the sector break-up of rural NDP, operating surpluses accruing to the organised sector account for almost 10 per cent of rural NDP. Deducting these, NSS rural consumption in 1993-94 would be about 70 per cent of rural disposable incomes by the new NAS series and around 75 per cent by the old series. This still implies that the NSS under-estimates rural consumption, but clearly any such underestimation is disproportionately more in urban areas since total NSS consumption is only around 50 per cent of total personal disposable incomes²⁸.

Thus, NSS estimates of rural consumption during the nineties are not seriously out of line with NAS-based estimates of rural incomes. In particular, there is no essential discrepancy between the growth of real rural consumption from the NSS and of real rural incomes from the NAS, provided NSS employment trends are accepted and the same deflator is used in both cases. Hence, GDP growth during the nineties can be reconciled with the NSS-based trend in rural poverty without assuming much more inequality than what the NSS already shows. Nonetheless, since the issue of rural-urban differentials is important, and the CSO does have the required data, a priority is that the NAS provide regular annual estimates of rural and urban NDP. Also, there is a need to re-

examine some of the revisions made in NAS 1993-94, particularly to the value of output from fruits and vegetables.

V. Conclusion

In anticipation of the full results of the 55th Round, this article has attempted to validate the existing NSS series and consider the issue of the most appropriate reference period for the NSS. Comparison of the existing NSS series by the 30-day recall with the NAS shows that although the NSS does estimate a lower level of consumption as compared to the NAS, the trend during the nineties are very similar. Also, any underestimation is likely to be disproportionately in urban areas. Especially on trends in rural consumption, the two sources agree if proper deflators are used and the available evidence on the rural-urban distribution of the workforce is considered. From the point of view of valid poverty calculations using the NSS, nothing of substance has changed since the exhaustive analysis of the matter in 1993 by the Expert Group on Estimation of Proportion and Number of Poor. If anything, the correspondence between the trends revealed by the NSS and those by alternative data is much closer during the nineties than these were during the seventies and eighties.

It remains true, however, as the Expert Group had observed, that there is need for “a sustained effort to refine and improve the survey design and procedure” in order that NSS estimates are considered reliable not only in regard to trends, but also the level of consumption. To this end, experiments involving alternative reference periods are not only desirable but are also a scientific necessity. However, since errors in the NSS are unlikely to be on account of recall alone, an equal emphasis requires to be put on improving the sampling procedure to better capture the consumption of the rich, or, at least, to provide some reliable estimate of errors involved.

In such experimentation particular attention needs to be paid to two issues. First, since comparison with external benchmarks cannot discriminate between errors due to under-enumeration in the samples and the non-sampling errors of recall, it is impossible to judge the relative accuracy of alternative schedules unless there is in-survey validation. Validation of NSS schedules by comparison with the NAS are thus prone to error even if the NAS were error-free, which it is not. Secondly, it is vital in all experimentation to continue in parallel with the existing 30 day schedule canvassed independently. This is required not only because of the need for comparable time-series data for policy purposes, but also because this is a benchmark necessary to draw valid conclusions from alternative schedules.

Analysis of the experiments in rounds 51 to 54 shows certain systematic differences in results across schedules, which are very important and require further investigation. However, these did not provide any in-survey statistical warrant for the change in schedule used in the 55th Round. Nor is there any statistical warrant for this from valid comparisons with the NAS. Thus, although the one-week recall leads to higher survey estimates of food consumption, apparently closer to the NAS, this does not necessarily imply greater accuracy of this recall if the NSS under-enumerates the rich. Indeed, there is some evidence that on correction for such under-enumeration the schedule used in the 55th Round may be overestimating food consumption. For this reason, it is necessary that the NSSO include some in-survey test of bias in its design of experiments by different recalls and also that an analysis be conducted of the pattern of non-response in its samples. Further, since

the 365-day recall alters substantially the distribution obtained for the consumption of certain goods and services, it is necessary that there be an analysis of the comparative results obtained in past rounds in which both the 30-day and 365-day questions were asked.

Moreover, the limited results now available from the 55th Round show clearly that answers to both the one week and 30 day questions have been contaminated by the presence of the other. Quite possibly, exclusive reliance on the 365-day question in the case of clothing etc. has also altered responses. As a result, consumption estimates from this round are not comparable to those from previous NSS rounds, and will probably be virtually useless for any assessment of changes in consumer demand. This also leads to a major contradiction whereby the 55th Round shows both large reductions in poverty by the 30-day recall and also significant increases in poverty by the 7-day recall.

For this reason, it is important for the credibility of the NSSO to stress the experimental nature of the 55th Round and its non-comparability with past rounds. While experiments with different schedules canvassed separately should continue, it is absolutely necessary to conduct another large sample Consumer Expenditure Survey using the 30-day reference period as soon as possible. Failure to do this, for whatever reason, could not only give misleading indicators to policy makers using this data, it would also compromise the reputation of India's statistical system. To admit the non-comparability of the 55th Round may mar some celebrations, but in order to maintain the integrity of the statistical system it is vital that truth continues to be given priority over joy.

In this context, it is also necessary to re-examine some of the revisions that have been made in the National Accounts series with 1993-94 as base. This series has involved some very welcome changes in methodology – most notably in the use of workforce data from the NSS rather than the Census. But it is also known that certain extraneous considerations had led the CSO to make large upward revisions in some sectors where the database was weak. In particular, the estimates of the value of “fruits and vegetables” in the new series appear to be totally out of line with other data and lead to conclusions about agricultural growth and productivity which are at variance with those derived from the more reliable data for forecast crops. This is an important matter that ultimately requires that the system of agricultural statistics be modernised to cope with requirements of an agricultural sector undergoing diversification towards horticulture and livestock. However, till then, NAS estimates for non-forecast crops should at least bear consistency with area statistics and with independent estimates of consumption. Also, since most of the recent scepticism about data has concerned rural poverty, it is desirable that the CSO make available on a regular annual basis the estimates of the rural-urban break-up that are already implicit in the NAS data.

END NOTES:

¹ The author is grateful to C.P. Chandrasekhar and Jayati Ghosh. Some of the data reported here have been taken from earlier issues of their column “Macroscan” in Hindu *Businessline*. In particular, see “The Poverty Puzzle” (February 22nd, 2000) and “The Choice of Reference Period” (September 19th, 2000). These can also be accessed on the Internet at <http://www.Macroscan.com>

² With nine NSS surveys on Consumer Expenditure conducted during the nineties, there is more information on the matter than during the previous two decades. Results are currently available for July-June 1990-91 (46th round), July-December 1991 (47th round), January-December 1992 (48th round), January-June 1993 (49th round), July-June 1993-94 (50th round), July-June 1994-95 (51st round), July-June 1995-96 (52nd round), January-December 1997 (53rd round) and January-June 1998 (54th round). However, of these, only the 50th round is a quinquennial large sample survey, while the rest are based on “thin samples” involving a much smaller sample size. Moreover, rounds 47, 49 and 54 are half-year surveys which are not necessarily comparable to the rest because of possible seasonal biases and, for this reason, have not been considered in the discussion to follow. During July 1999-June 2000, the NSS has completed another quinquennial large sample survey, the 55th Round, results of which are expected later this year.

³ Three internally consistent series on poverty incidence are available for the nineties, all of which are based on the NSS distribution of nominal consumption expenditure and on the official poverty line but use somewhat different deflators: Datt, G. (1999) (“Has poverty declined since economic reforms? Statistical Data Analysis” *Economic and Political Weekly*, December 11th), Gupta, S.P. (1999) (“Trickle Down Theory Re-visited: The Role of Employment and Poverty”, V.B. Singh Memorial Lecture, Indian Society of Labour Economics, November 18-20) and Sundaram, K. and S. Tendulkar (2000) (Poverty in India: An Assessment and Analysis, mimeo, Delhi School of Economics). Each of these show that the head-count poverty ratio in Rural India declined almost steadily between 1972-73 and 1989-90, and that after July 1991 this has fluctuated at levels which in every subsequent year has been higher than during 1989-90. Urban poverty is, however, seen to be declining during the nineties by all the three series.

⁴ See, for example, Tendulkar, S. and L.R. Jain (1995) (“Economic Reforms and Poverty”, *Economic and Political Weekly*, June 10th), Datt and Ravallion (1997) (“Macroeconomic Crises and Poverty Monitoring” *Review of Development Economics*, 1(2)), Ravallion, M. (2000, a, b, and c) (“Food Prices, Real Wages and Rural Poverty”, *Food Policy*, August; “What is needed for a more pro-poor growth prospect in India?” *Economic and Political Weekly*, Special Number; “Should Poverty Measures be anchored to the National Accounts”, *Economic and Political Weekly*, August 26) and Sundaram and Tendulkar (2000 op.cit.).

⁵ See, for example, Aiyer, S. (2000) (“Has economic reforms bypassed the poor?”, *The Times of India*, April 23rd), Bhalla, Surjit (2000a) (“Growth and Poverty in India - Myth and Reality”, mimeo), Lal, Deepak., I. Natarajan and Rakesh Mohan (2000) (“Economic Reforms and Poverty Alleviation: a tale of two surveys”, mimeo, UCLA and NCAER), and Srinivasan, T.N. (2000) (“Growth, Poverty Reduction and Inequality, mimeo, Yale University).

⁶ “Poverty falls: What, me worry?” *Business Standard*, September 30th, 2000.

⁷ *Bulletin of the International Statistical Institute*, Volume 34, Part II, 1954.

⁸ NSS Report No. 447: Choice of Reference Period for Consumption Data, NSSO, March 2000.

⁹ The issue of why there should be such a discrepancy, and why the seven-day recall should provide a higher estimate, has not been resolved adequately. Intuitively it would appear that a seven-day recall should be more accurate, particularly for items consumed infrequently, since informants are less likely to forget their consumption by a shorter recall. But one possibility is that there is a certain “overhead” characteristic to food consumption which may result in overestimation by recall relating

to a shorter period. Thus, suppose that a certain item of food is consumed during in a particular week without there being any purchase or household production of that item during that week. There would be double counting if the seven day response leads to respondents reporting accurately their expenditure during the reference week, but are then prompted by the questionnaire to report consumption of items which were not purchased or otherwise acquired during that week but were consumed nonetheless. A related problem is the difficulty faced by informants in calculating consumption for a week in cases where this is less than the quantity normally purchased at a time. Since the quantity best known to the informant is the latter, this might be reported without making tedious calculations.

¹⁰ Pravin Visaria (2000a): “Alternative estimates of poverty in India”, *Economic Times*, June 29.

¹¹ NSSO (2000b), “Report No. 453: Household Consumer Expenditure in India (July-December 1999) Key Results” contains a set of key results giving expenditure class wise distribution of persons and average monthly consumption expenditure by all-India and states from sub-rounds 1 and 2 of the 55th round, covering the period July-December 1999. This is available by both the one-week and one month recall for food consumption. However, the only details of consumption by commodities are in a single table. This gives a breakdown of the all-India consumption by items at the aggregate rural and urban levels without any expenditure class wise distribution.

¹² Since the NSSO does not make any poverty calculations on its own, this must be derived from the distribution of consumer expenditure using some poverty line. We assume poverty lines of Rs 328 and Rs 458 per capita per month for rural and urban areas for July-December 1999. These are obtained by updating the Planning Commission’s official poverty lines with available consumer price indices as recommended by the Expert Group (Planning Commission, 1993). It should be noted, however, that we have updated the national poverty line using national level price indices and have applied this to the national distribution, and have not followed the Expert Group’s recommendation of constructing state-wise poverty lines to derive poverty estimates separately by states. As a result, our estimates will differ from those obtained by proper application of the Expert Group method, but are conceptually similar to all the estimates in Table 1 which also apply a national poverty line to the national distribution.

¹³ Using the distributions for the 51st, 52nd, 53rd and 54th rounds, rural poverty by the Type 2 schedule corresponding a 27.4 per cent rural poverty by the Type 1 schedule work out to 12.2, 12.2, 14.2 and 12.1 respectively, giving an average of 12.7 per cent. Similarly, for urban poverty, an incidence of 25.2 per cent by the Type 1 schedule corresponds to an incidence by the Type 2 schedule of 9.9, 11.2, 12.5 and 13.4 in Rounds 51 to 54.

¹⁴ It should be noted that a 24.4 per cent rural poverty incidence by the Type 2 schedule corresponds to poverty incidences of 43.1, 44.4, 40.1 and 43.3 per cent by the Type 1 schedule using the distributions in Rounds 51, 52, 53 and 54 respectively. For urban areas, Type 1 estimates corresponding similarly to a Type 2 estimate of 23.4 per cent are 41.0, 38.9, 39.3 and 36.4 per cent respectively. On the basis of these correspondences, the poverty incidence found using the 7-day recall during July-December 1999 would be higher than not only in 1993-94 but also 1987-88.

¹⁵ This is especially so because, according to Visaria (2000b), the decision on the 55th round schedule was “a last minute compromise”.

¹⁶ Thus, Datt (1999) writes “in nominal terms, NSS consumption grew by 198 per cent between 1990-91 and 1997, while NAS consumption grew by 233 per cent in the same period”. In fact,

NSS nominal per capita consumption increased 98 per cent between 1990-91 and 1997. The corresponding growth in NAS, interpolating linearly to NSS survey mid-points is 99 per cent when the old 1980-81 series is extended from 1996-97 to 1997-98 using the new 1993-94 series and 111 per cent with the new series as reported in NAS 2000. The NAS consumption growth would be 145 per cent if the 1997 figure from the series with 1993-94 as base is compared incorrectly to the 1990-91 figure from the series with 1980-81 as base. Thus Datt appears to have reported the level of indices as their growth and, using an earlier estimate for 1997 from the NAS with 1993-94 as base, compared this with the 1990-91 figure from the NAS with 1980-81 as base.

¹⁷ The *National Accounts Statistics 2000* gives macroeconomic aggregates by the revised (1993-94 base) series from 1950-51 onwards in a special statement, but no commodity wise break-ups are available as yet.

¹⁸ Minhas, B.S.(1988): “Validation of Large Scale Sample Survey Data – case of NSS estimates of household consumption expenditure”, *Sankhya*, Series B, Vol 50, Part 3, May. The present NAS estimates of total consumer expenditure for 1972-73 and 1977-78 are about 34 per cent higher than those used in this article.

¹⁹ See e.g. Minhas, B.S., S.M. Kansal, Jagdish Kumar and P.D. Joshi (1986), “On the reliability of the available estimates of consumer expenditure in India”, *Journal of Income and Wealth*, Vol 9, No. 2, July; Minhas, B.S.(1988): “Validation of Large Scale Sample Survey Data – case of NSS estimates of household consumption expenditure”, *Sankhya*, Series B, Vol 50, Part 3 May; Minhas, B.S. and S.M. Kansal (1989): Comparisons of the NSS and CSO estimates of Private Consumption, *Journal of Income and Wealth*, Vol 11, No. 1, January.

²⁰ The reason for considering 1995-96 is that this is the latest year for which full details are available both by the two schedules in the NSS and by the two alternative series of the NAS. The only other year for which such details are available is 1994-96, and this shows similar differences.

²¹ The sum of absolute differences between the NSS and NAS 1980-81 estimates for food items are Rs 69514 crore and Rs 77028 crore by schedules 1 and 2 respectively. This is as against the corresponding algebraic differences of Rs 54303 crore and Rs (-) 31213 crore.

²² In, other words, the adjustment consists of first estimating the NAS-NSS difference in total non-food consumption. From this, the total underestimated consumption is derived by dividing by the share of non-food in the consumption bundle of the rich. The commodity composition of the consumption bundle of the rich is also used to distribute item-wise the derived estimate of total underestimated consumption. This exercise was also done using the NAS 1980-81 as the controlling total for non-food consumption, and with the assumption that the missing consumption had a commodity composition similar to the consumption of the top decile of the rural population. The qualitative results are similar to those reported.

²³ Datt, G. and M. Ravallion (1992): “Growth and Redistribution Components of Changes in Poverty Measures: A Decomposition with Application to Brazil and India in the 1980s” *Journal of Development Economics*, Vol 38; Ravallion, M. and G. Datt (1996): “India’s Checkered History in Fight Against Poverty – Are there Lessons for the Future?”, *Economic and Political Weekly*, Special Number.

²⁴ The ratio of the Ravallion-Datt deflator to the consumption deflator in NAS 1993-94 increases less, by 5 per cent between 1990-91 and 1997, leaving some unexplained gap between the two real

consumption estimates. But this corresponds exactly to the difference found earlier between the NSS nominal consumption and that according to NAS 1993-94.

²⁵ Differences in deflators explain most of the differences in the different poverty estimates in Table 1, but this does not alter the basic conclusion. All these deflators are constructed from data external to the NSS. See, however, Deaton, A, and A. Tarozzi (1999) (“Prices and Poverty in India, mimeo, Woodrow Wilson School, Princeton) who have constructed Tornquist indices from prices implicit in the NSS.

²⁶ There is evidence of a significant trend-break in rural inequality after 1990-91, although, unlike for urban areas or the national, the trend fitted to rural Gini coefficients during the nineties is not statistically significant. Fitting a kinked exponential model to all full year rounds between 1972-73 and 1997 shows the rural Gini declining at 0.5 per cent per annum between 1972-73 and 1990-91 and increasing at 0.6 per cent per annum thereafter, with both the earlier negative trend and the break after 1990-91 statistically significant. The statistically significant negative trend in rural Gini between 1972-73 and 1990-91 was not picked up by the earlier Ravallion-Datt analysis because of inclusion of the 28th round which is not a full year round.

²⁷ Chandrasekhar, C.P. and Jayati Ghosh: “Poverty Puzzle”, Macroscan, *Businessline*, February, 2000. The method adopted for this is as follows. Since both sectoral employment and income figures for rural and urban areas are available for 1980-81 and 1993-94, it is possible to compute sectoral productivities for the primary, secondary and tertiary sectors in the rural areas and urban areas. This allows computation of the ratio of urban to rural productivities for each sector for those years. It is assumed that these ratios remain constant in all subsequent or previous years. Based on this assumption, and using the overall GDP figures and the figures on rural and urban sectoral employment from the NSS, the level of rural and urban sectoral incomes is estimated for the remaining years.

²⁸ The simple ratios of NSS consumption to NDP (new series) are 0.59 and 0.41 for rural and urban India respectively in 1993-94. Adjusting the respective numerators upward for the NAS-NSS difference in estimates of rent on dwellings, these become 0.63 and 0.46. And further, on deducting from the respective denominators the operating surpluses accruing to the organised sector, these are 0.70 and 0.57. To arrive at the correct ratio of consumption to disposable income, further adjustments to the denominators are required: i.e. deduction of personal taxes and interest payments by unorganised enterprises and addition of receipts on account of transfer payments and distribution of profit and interest by the organised sectors. The data available does not allow these further adjustments, which in the net are likely to add much more substantially to urban incomes.