

Data and dogma: the great Indian poverty debate

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ABSTRACT

What happened to poverty in India in the 1990s has been fiercely debated, politically and statistically. The Indian debate has run parallel to, and is itself a large part of, the wider debate about globalization and poverty in the 1990s. The economic reforms of the early 1990s were followed by rates of economic growth that were high by Indian historical standards. The effects on poverty remain controversial, and the official numbers published by the Government of India, showing a reduction of poverty from 36 percent of the population in 1993–94 to 26 percent of the population in 1999–00, have been challenged both for showing too little and too much poverty reduction. The various claims have often been frankly political, but there are also many important statistical issues, and the Indian debate, of which this paper is a review, provides an excellent example of how politics and statistics interact in an important, largely domestic debate. Although there is no full consensus on what happened to Indian poverty in the 1990s, there is good evidence that the official estimates of poverty reduction are too optimistic, particularly for rural India. This overoptimism was amplified by statistical uncertainty that created space for some commentators to argue that poverty had been virtually eliminated in India in the wake of the economic reforms. Although this paper is concerned with the measurement of poverty in India, all of the issues—discrepancies between surveys and national accounts, the effects of questionnaire design, reporting periods, survey non-response, repairing imperfect data, the choice of poverty lines, and the interplay between statistics and politics—have wide resonance elsewhere..

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1. Overview

Hundreds of millions of Indians are poor by national and international standards. Indian policy making and politics are dominated by discussions of poverty, and measures of poverty rightly attract a great deal of attention and debate. In the second half of the 1990s, India's GDP grew rapidly by (Indian) historical standards, and many commentators have associated this acceleration with the process of economic reform that began in the 1990s. Yet the reforms themselves, and the limited opening of the Indian economy that they involved, remain controversial, as does their effect on poverty. This debate is far from unique to India. The worldwide controversy about globalization and its effects on poverty and inequality has followed much the same lines as the internal debate in India. And indeed, India accounts for about 20 percent of the global count of those living on less than \$1 a person per day, so that what happens in India is not only a reflection of the worldwide trend, but is one of its major determinants.

Historically, the Indian statistical system led the world in the measurement of poverty. The sample surveys that were pioneered by Mahalanobis at the Indian Statistical Institute in Calcutta in the 1940s and 1950s were moved into the government statistical system as the National Sample Survey Organization (NSSO), whose household surveys are the basis for the regular publications on poverty by the Planning Commission. Where Mahalanobis and India led, the rest of the world has followed, so that today, most countries have a recent household income or expenditure survey from which it is possible to make a direct assessment of the living standards of the population. Mahalanobis and his colleagues in Calcutta also conducted experiments on the design of household surveys, investigating how to most accurately measure the levels of consumption that are the raw material for the estimation. Such experiments are too infrequently

carried out today although, as we shall see, the NSSO has recently used them to investigate a number of important questions of survey design.

National statistical systems collect more than survey data, and much non-survey information is relevant for the monitoring of living standards. Most important are the National Accounts Statistics (NAS), which provide widely used measures of aggregate performance, including GDP and consumption. Although the NAS consumption estimates rely on surveys for several of their components, much also comes from other sources. And although there are important differences in definition between the survey and national accounts concepts of consumption, a well-functioning statistical system will use multiple estimates to cross-check. And indeed, there is a long and distinguished tradition of empirical work in India on such comparisons. Of course, poverty depends not only on average consumption, but also on its distribution, particularly at the bottom of the distribution, so that the National Accounts cannot by themselves provide a direct estimate of poverty. Even so, the existence of serious and growing discrepancies between mean consumption from the surveys and from the National Accounts casts doubt on one or both of the two estimates, and if the discrepancy affects a number as important as the growth rate of average living standards, conflicts in measurement stop being the arcane purview of statisticians, and move into the public and political debate.

This is what happened in India in the late 1990s. The growth of average consumption measured from the National Accounts Statistics exceeded its measured rate of growth in the National Sample Surveys, with the result that measures of poverty, which are based entirely on the survey data, declined less rapidly than seemed to be warranted by the rate of growth of consumption (and GDP) in the national accounts. Given the political divisions that surrounded

the reforms, the discrepancy quickly ceased to be a purely statistical issue. Those with a stake in the success of the reforms emphasized the national accounts statistics, as well as the lack of evidence that the distribution of consumption had widened among the poor. According to this view, surveys are inherently unreliable and error prone, and some commentators (although without producing any evidence) went so far as to paint pictures of enumerators filling out the questionnaires in tea-shops, avoiding the time-consuming and repetitive task of actually interviewing respondents. On the other side, reform skeptics argued that the survey data showed exactly what *they* had expected, that the reforms, while benefitting the better-off groups in society, had failed to reach the poor, particularly the rural poor, and that the distribution of consumption had indeed widened. They also pointed to the differences in definition between the national accounts and survey measures of consumption, arguing that the latter were more relevant for assessing poverty. They also identified many areas where the National Accounts estimates of consumption are weak and prone to error.

Once again, the Indian debate is mirrored in the global debate about trade, development, and poverty. In many other countries, including many rich countries who spend a great deal more on their statistical systems, estimates of growth rates from National Accounts are larger than and inconsistent with those from household surveys. Mean consumption from the United States Consumer Expenditure Survey grows more slowly than does mean consumption in the national accounts, and the difference in the two growth rates is remarkably similar to the difference in India. And global poverty, measured from household income and consumption surveys from around the world, is not falling as rapidly as appears to be warranted from global growth rates and the limited change in global inequality. In consequence, the issues that roiled the Indian

debate in the 1990s are actual or potential issues in a number of countries, and are an immediate threat to the estimation of global poverty, of which Indian poverty is an important component.

Statistical agencies are part of the societies that they serve, so that debates over important numbers tend, quite properly, to provoke reviews of practice, and experimentation with new practices. To its credit, the National Sample Survey Organization of India organized a series of experiments with its consumption surveys in the late 1990s. These were primarily designed to investigate the effects of different reporting periods on the amount of consumption reported, for example whether people reported a higher or lower rate of consumption of rice when the question was posed with reference to consumption over the last 30 days, or over the last 7 days. The experimental questionnaires, which collected data on food, *pan*, and tobacco at 7-days, clothing, durables, and educational and institutional medical expenses at 365-days, and all other goods at 30-days, generated a modest increase in reported consumption over the traditional questionnaires, which collected all goods with a 30-day recall. For per capita total household expenditure, the average increase was between 15 to 18 percent, but *this was enough to halve the measured number of poor in India*, because a large fraction of households have consumption near the poverty line. While this apparently stunning result reveals more about the inadequacy of poverty counts and headcount ratios than it does about poverty in India, it had an important effect on the debate. Not only did it provide ammunition to those who argued that surveys were inherently unreliable for measuring poverty, but it turned an apparently arcane statistical issue—the choice of reporting periods—into one that was politicized and intensely debated. These debates were “resolved” by adopting a compromise design for the important consumption survey that was carried out in 1999–2000, the 55th Round of the NSS. This survey was the first large-scale survey

since 1993–94, and thus the first that would provide relevant information on the effects of the reforms. But in the event, the compromise design made it difficult to compare its results with earlier surveys. In consequence, the official poverty measures from that survey, which were published in February of 2001 and which showed a very large decline in poverty rates, only fueled the debate, instead of settling it.

Choices about poverty lines and measures have been important in the Indian debate, and are important more generally. Because so many people in India are so close to the poverty line, the simple headcount ratio, which gets almost all the attention, is extremely sensitive to small changes in both reality and measurement error. So while the choice of poverty line does not affect the fundamental underlying problem that the surveys and national accounts data are systematically diverging, the use of the headcount ratio, and the position of the poverty line, ensures that statistical problems have very large effects on outcomes. And even if accuracy were a less serious problem, large changes in headcount poverty are sometimes associated with very small changes in standards of living—the state of Uttar Pradesh is a recent example—overstating genuine reductions in poverty and vulnerability. Indian poverty lines are also differentiated geographically, by state, and by urban and rural households within each state. Multiple poverty lines are often difficult and controversial, and there is good evidence that the measurement of Indian regional patterns of poverty has gone seriously astray.

The Indian poverty debate has also paid attention to other, broader lines of poverty research. In part this reflected a well-justified impatience with an exclusive focus on one set of numbers, which even if perfectly measured, have serious theoretical inadequacies, and in part, it reflected the worldwide trend in looking beyond consumption poverty to other important measures of

living standards, in particular to indicators of human development, such as mortality, morbidity, and education. Much of this work has been pioneered in India, and it continues to be a necessary and vital part of poverty assessment in India and in the world.

2. The Indian poverty monitoring system: a brief introduction

The Government of India's official poverty estimates are based on the results of regular consumer expenditure surveys by the National Sample Survey Organization (NSSO). Surveys are in the field continuously and, in recent years, all surveys have collected some data on consumers' expenditure. But only the larger surveys that focus on consumers' expenditures are used by the Planning Commission to calculate the official poverty statistics. In principle, these large surveys take place every five years, although in practice the gap has often been larger. Such surveys were conducted in 1983 (the 38th Round of the NSS), 1987–88 (the 43rd Round), 1993–94 (the 50th Round), and most recently, in 1999–2000 (the 55th Round.) For each of these years, the Planning Commission has published estimates of the proportion and number of people in poverty, broken down by state and sector. Although various scholars have calculated poverty rates based on the intermediate, smaller, surveys, notably Gaurav Datt (1999), the Planning Commission does not do so, on the grounds that the larger surveys are required to estimate poverty accurately for each state, and that accuracy is required because various transfers from the central government to the states depend on the numbers.

The poverty estimates published by the Planning Commission count the number of people who are living in households whose monthly per capita total expenditure is less than a poverty line for the sector and state in which they live. These poverty lines are updated over time using

the Indian system of state by state price indexes, which are estimated separately for rural (the consumer price index for agricultural laborers, CPIAL) and urban (the consumer price index for industrial workers, CPIIW) households. There is no predetermined All India poverty line, either for urban or rural. Instead, poverty counts are made for each state, within each sector, and added-up to get urban and rural totals. All India urban and rural poverty lines are then set to guarantee that, if applied to all urban or rural households without differentiation by state, the total number of those in urban and rural poverty matches the sum of the state counts. The original official state-level poverty lines, which incorporate state to state differences in price levels, come from the report of an Expert Group, Government of India (1993), which also recommended a number of other changes in previous practice. The poverty data from 1983 onwards are available according to current procedures, and it is these numbers which are the subject of the debate.

3 Conflicts between National Accounts and Sample Surveys

Estimates of mean consumption are generated both by the National Accounts Statistics and by the National Sample Survey Organization from their regular surveys of consumers' expenditures. The two sets of estimates can be used as cross-checks and external validators of one another, both at the level of total consumers' expenditure, and at the level of individual commodities, or groups of commodities, such as food-grains, clothing, or services. There is a long tradition of this comparative work in India. Much more controversially, the Planning Commission has, in the past, although not in the 1990s, used the National Accounts estimate of consumption as a "control" total for the surveys when they estimated poverty. If the ratio of the national accounts to survey estimate of mean consumption is R , say, with R a number greater than one, the

Planning Commission would multiply total expenditure of each household by R prior to counting the number of persons living in households below the poverty line. This procedure is not unique to India, and is currently practiced in many countries around the world, particularly in Latin America, where survey estimates of income are typically much smaller than those from the national accounts. The abandonment of scaling up in India, for reasons discussed below, has been the subject of considerable controversy, see particularly Bhalla (2001) who, like Sala-i-Martin (2002), uses a variant of the method to estimate global poverty. In India in the 1990s, where the national accounts estimate of mean consumption grew much more rapidly than the survey estimate, scaling up would have shown a rapid reduction in poverty in the 1990s, much more rapid than was the case for survey-based poverty estimates. In consequence, those who believe that the reforms and the post-reform economic growth have been associated with large scale poverty reduction have tended to argue that the national accounts are right, and the surveys wrong, with anti-reformers or skeptics arguing for the surveys, not the national accounts. Exactly the same argument has been made for the world as a whole by Bhalla and Sala-i-Martin, see Deaton (2005) for a discussion.

Early Indian comparisons of surveys and national accounts were carried out by Mukherjee and Chatterjee (1974) and by Srinivasan, Rhadakrishnan, and Vaidyanathan (1974). These authors examined the match between the two estimates of total consumption and its distribution over categories using NSS and NAS information from the 1950s and 1960s. For the decade up to 1963–64, Mukherjee and Chatterjee write that “the agreement between the revised series (for NAS consumption) and the NSS estimates remains surprisingly close,” although they note that the NSS estimates are systematically and (on average) increasingly below the NAS estimates in

the period up to the end of the 1960s. They also note discrepancies in the distribution of consumption over commodities, with the surveys recording a higher share of food in the budget than does national accounts consumption. Srinivasan, Rhadakrishnan and Vaidyanathan's analysis is broadly consistent with that of Mukherjee and Chatterjee, though they find that the surveys are lower than the NAS estimates from an earlier date. They also note that the distribution of consumption over categories is broadly similar in the two sources.

If the early comparisons of national accounts' and survey estimates of consumption were relatively reassuring, more recent ones are anything but; the gap between the two estimates of mean consumption has continued to widen, and has currently reached levels that would have been viewed with horror by the early writers. Depending on which set of adjustments we make, the NSS estimate is currently around two-thirds of the NAS estimate of consumption, and has been falling steadily since the late 1960s, by 5 to 10 percentage points per decade. It is worth noting that this differential rate of growth in consumption estimates is far from unique to India. As best we can tell, there is a similar discrepancy between survey and national accounts estimates of the growth rate of consumption for the world as a whole, see Deaton (2005), and to take a specific example at a very different level of development, the differential rate of growth in the United States is very similar to that in India, see Triplett (1997) and Garner et al (2003). While there are almost certainly errors in both sets of estimates, the view of what is happening to poverty in India (and in the world) depends a good deal on how much of the discrepancy is attributed to each.

For many economists, who are well-versed in the concepts of national income accounting, but much less so survey practice, the automatic reaction is to trust the national accounts over the

surveys. That there is little basis for such a judgment was splendidly argued by Minhas (1988) in a paper that should be compulsory reading for anyone concerned with the issue of national accounts versus surveys, particularly anyone who does not understand the complexities and approximations involved in the construction of the former. It was this paper that provided the central case for the Planning Commission to abandon its previous practice of scaling up the survey results to match the national accounts. Minhas lays out the issues that have dominated the contemporary debate, the differential definition and coverage of NAS and NSS consumption, differences in timing, and the heavy reliance in national accounting practice on various “rates and ratios” that link observable but irrelevant quantities to the relevant but unobservable ones. These ratios are in principle derived from surveys, for example surveys that link the earnings of those employed in services to the value added in the service sector, but are frequently many years, often decades, out of date. The use of outdated “rates and ratios” in an economy undergoing growth and structural development will typically lead to systematic *trend* errors in the accounts. A prime example is the netting out of intermediate production from value-added, which is frequently done using some fixed ratio. But the degree of intermediation tends to grow as the economy becomes more complex and more monetized, so that the rate of growth of GDP and of consumption will be systematically overstated in a growing economy.

Minhas notes that “Many discussions of sampling errors seem to imply as if only the NSS estimates suffer from those errors. This is a gross misconception.” and ends by warning against adjustments that assume that only the survey estimates are at fault. In particular, he writes

“it is indeed hazardous to carry out pro-rata adjustment in the observed size distribution of consumer expenditure in a particular NSS round by multiplying it with a scalar derived from

ratio between the NAS estimates of aggregate private consumption for the nearest financial year and the total NSS consumer expenditure available from that particular round of household budget survey. This kind of mindless tinkering with the NSS size distribution of consumer expenditure, as practiced by the Planning Commission in the Seventh Five Year Plan documents, does not seem permissible either in theory or in light of known facts.”

Given that NAS consumption is growing at more than one percent per annum faster than NSS consumption, the application of the pro rata adjustment, either “correction” or “mindless tinkering,” depending on one’s point of view, makes an enormous difference to the trend in measured Indian poverty. It is unfortunate that so much of the current debate over this issue should have been so little informed by what Minhas wrote 15 years ago.

Kulshreshtha and Kar (2003) and Sundaram and Tendulkar (2003) are contemporary discussions of the discrepancy. Kulshreshtha and Kar are the statisticians at the Central Statistical Organization who are primarily responsible for the production of the national accounts, so that their views on the accuracy of the consumption estimates should be accorded great weight. They document the growing discrepancy between the two sources, from 5 percent in 1957–58 to more than 38 percent in 1993–94, and note that the discrepancy for non-food is both larger and more rapidly growing than the discrepancy for food. They then go on to explore the food items in detail, because it is in this area where most is known, and because there is often enough additional information to make an informed judgment about the likely balance of accuracy. Although there are some exceptions, the general finding is the same as Minhas (who comes at the issue from the survey side, as opposed to Kulshreshtha and Kar, who are national income statisticians), that when there is a discrepancy, it is the National Accounts estimates that are

typically less plausible and more likely to be in error. They note that the food and tobacco discrepancy can be attributed to a few specific commodities (fruit, milk products, chicken, eggs, fish, minor cereals and their products, *vanaspati*, oilseeds, and tobacco), and that for major subgroups that are important in poverty studies (major cereals, more commonly used pulses, edible oils, liquid milk, and vegetables), the two estimates are relatively close. They conclude that there is nothing in their findings that would “render the NSSO data on household consumption expenditure unfit for measurement of poverty incidence.”

Sundaram and Tendulkar (2003) report on the findings of a joint CSO-NSSO exercise concerned with the cross-validation of the two sets of estimates. They draw particular attention to the “fluidity” of the NAS estimates, that revisions for some categories are often so large as to cast serious doubt on the estimates in general. This is closely related to the outdated “rates and ratios” point emphasized by Minhas; when eventually a long-used ratio is abandoned by the CSO, and new survey or other information collected, information based on actual data paints a very different picture to that based on the long-used approximation. Such revisions, while always welcome, do little for the large number of items still hostage to the accuracy of old, and aging, ratios. Sundaram and Tendulkar also argue that survey data are to be preferred because they measure living standards *directly*, as opposed to NAS statistics, which derive consumption as a residual at the end of a long chain of calculations.

Sundaram and Tendulkar also draw attention to those items included in the NAS estimates but not in the surveys, such as the imputed rents of owner occupiers and expenditures by non-profit institutions serving households. Like Kulsheshta and Kar, they demonstrate the increasing importance of a relatively new item, introduced in accord with the recommendations of the 1993

version of the United Nations *System of National Accounts* (SNA), “financial intermediation services indirectly measured” or FISIM, for short. FISIM is measured as the difference between interest paid to banks and other financial intermediaries and interest paid by them. The idea is that interest charged to borrowers contains, in addition to the market rate of interest, a charge for intermediation services to lenders, while interest paid to lenders is lower than market, with the difference attributed to financial intermediation services to depositors. The difference between interest paid and interest received is therefore a measure of the value of financial intermediation to borrowers and lenders, and since the 1993 revision of the SNA, has been added to national accounts estimates of household consumption, with some backdating into the 1980s. A similar item is included for risk-bearing services, measured as the profits of insurance companies. In India, the value of FISIM increased from close to zero in 1983/84 to 2.5 percent of consumption in 1993/94, so that this item alone accounts for a quarter of a percentage point per year of the difference in annual growth rates between NAS and survey consumption in India. Note also that, to the extent we are interested in measuring the living standards of the poor, it can reasonably be doubted whether any of the value of financial intermediation is relevant.

4 Survey methodology: reporting periods

The design of the Indian surveys has evolved over time, and is continually under discussion. As we outlined in the introduction, one of the most important design issues for poverty measurement is the length of the reporting period. The NSS experiments in Rounds 51 through 54, from 1994 through 1998, showed that different reporting periods generated different amounts of total expenditure. A questionnaire with 7 days for high frequency items (food, *pan*, tobacco), 365 days

for low frequency items (durable goods, clothing, footwear, institutional medical care, and educational expenses) and 30 days for everything else produced sharply lower poverty counts than one with a uniform 30-day reporting period. The reduction in measured poverty comes from two quite separate effects. The first is that a higher rate of monthly expenditure is reported when people are asked to report food, *pan*, and tobacco over the last seven days rather than when they are asked to report over the last thirty days. More reported expenditure, other things being equal, decreases measured poverty. The second effect comes from the low frequency items. Although the mean reported expenditure for this category *decreases* for the longer reporting period, the lower tail of the distribution *increases*. Over the last 30-days, most households report no purchase of the low frequency items, but at 365-days, most households report something. In consequence, and in spite of the decrease in the mean, the longer reporting period for the low frequency items also acts to reduce measured poverty.

It is also important to note that measures of inequality are substantially reduced by moving from a 30-day to a 365-day reporting period for the low frequency items. Because the mean goes down and the bottom tail comes up, measured dispersion in these purchases is much reduced, and this carries through to total expenditures. This means that it is never legitimate to compare measured inequality across surveys with different reporting periods, at least not without making some sort of correction.

The experiments with the different questionnaires showed that reporting periods make a difference, but did not settle the question of which was correct. Although there was no information on this before the 55th Round data were collected in 1999–2000, the NSSO subsequently launched a set of experiments designed to find out. The first results are reported in

NSSO Working Group on Non-sampling Errors (2003), who updated and extended the experiments carried out in the 1950s by Mahalanobis and Sen (1954), whose results were the basis for the NSSO's use of a 30-day recall period for all goods. Alternative questionnaires are randomized over the experimental households, using three different reporting periods, 7-days, 30-days, and a "gold standard" of daily visits accompanied by direct measurement. A pilot study was undertaken in five Indian states from January through June 2000. In the rural households in the experiment, the 7-day estimates were on average 23 percent higher than the 30-day estimates, somewhat lower than the discrepancy in the large scale NSSO "thin" round surveys. But comparison with the daily estimates shows that, for many important commodities, including cereals and cereal products, the 30-day estimates are *more* accurate than the 7-day estimates. Over all the goods examined, there is no clear superiority of one recall period over another, and there is little evidence that the traditional 30-day reporting period is seriously inadequate. This important study does not support the apparently sensible hypothesis that high-frequency items in India are better measured with a 7-day than a 30-day recall (for example, because people forget), nor does it support the idea that the discrepancy between the NAS and NSS measures of consumption is largely due to underestimation in the latter associated with an overly long reporting period.

How reporting periods affect estimates of consumption and poverty is a general issue that affects many countries other than India. Nor is it the only such issue. In literate populations, respondents can be asked to keep diaries as an alternative or supplement to interviews. It is also possible for surveyors to visit the households on multiple occasions, for example to take account of seasonality in expenditures, or because it is thought that respondents cannot remember

accurately for anything other than short periods, so that longer reference periods must be gathered a day or two at a time. There has been a good deal of international experience on these issues, reviewed for example in Deaton and Grosh (2001), although there can be no presumption that a good design for one country will be a good design for another. Indeed, the results of the Indian experiments came as something of a surprise, in that prevailing opinion would certainly have judged the 30-day recall period as much too long for most foods.

5 What happened to poverty in India in the 1990s?

5.1 The design of the 55th Round, 1999–2000

At the end of the 1990s, there had not been a large scale consumers' expenditure survey since 1993–94, and so there were no official estimates of national or state poverty rates for any later date. The NSSO runs smaller consumers' expenditure surveys between the quinquennial rounds, but poverty estimates based on them are not endorsed by the Planning Commission, even though the sample sizes are large enough to support accurate poverty estimates at the national level. These "thin" surveys, the last of which was a half year survey in 1998, appeared to show that there had been little or no progress in reducing poverty, see for example the widely noted paper by Gaurav Datt (1999). It is widely believed that there was some problem with the sampling in the rounds from 1994 through 1998, but no official statement has ever confirmed such a problem, nor is it clear that whatever problems there were seriously affected the results. In consequence, until the results from the 1999–2000 survey were due in March 2001, the thin surveys provided the only survey data on trends in consumption poverty at the end of the decade.

However, there was an immediate problem. As we have seen, the 51st through 54th Rounds

had tried out a new questionnaire, the results of which showed more consumption, and less poverty. The NSSO therefore had to face the question of what design to use in the all important 55th Round. The experimental results of the working group discussed above were not available when the decision had to be made, so there was little solid scientific guidance. By contrast, the consequences for poverty estimation of adopting the new questionnaire were well understood, so that a decision that would normally be left to statistical experts became politicized. In the event, and after a good deal of controversy, a “compromise” solution was adopted whereby, for food, *pan*, and tobacco, each household was asked to report all items over *both* a 7-day and 30-day recall period. At the same time, the traditional 30-day recall period for durables, clothing, educational and institutional medical expenses was replaced by a 365-day recall *only*. While this new, compound, design might well be defended on its own terms, it is clearly not comparable to any previous survey, so that the consumption and poverty estimates based upon it cannot reliably be used to assess trends. Because the experimental questionnaire generates higher responses for high frequency items than does the traditional questionnaire, the presence of both is likely to prompt respondents to reconcile the two reports. We would therefore expect, for example, the reported consumption of milk over 30-days often to be quite similar to $(30/7)$ times the reported consumption of milk over the last 7-days, something that might not happen if the same respondent were asked one, or the other, but not both. And indeed, the means of total estimated consumption using the new and old questionnaires are much more similar in the 55th Round than was the case in the experimental thin rounds, where each household was randomly assigned to one or other questionnaire. It remained unclear whether this meant that consumption reported with 30-day recall was pulled up to meet the 7-day reports, or whether the latter was pulled down

by the presence of the traditional questions, or some combination of both.

The presence of both questionnaires on the survey increased the interviewing time, and forced a number of other changes to the survey. The employment and unemployment survey, usually given to the same households who answer the consumers' expenditure schedules, was given to separate households in the 55th Round. But even within the consumer expenditure schedule, there were important changes, nearly all in the interests of compression and time saving. Questions about the source of consumption, from purchases, home production, in kind, or gifts, were asked in an abbreviated form, and several items of consumption that had previously been asked separately, were now asked together. For example, there is a single question about wheat and *atta*, rather than questions about each.

In spite of all these difficulties, the 30-day responses were adopted as the new official poverty totals, although the Planning Commission, in its Press Release, also provided (lower) estimates of poverty using the 7-day recall. The estimates based on 30-day recall, which were the only ones even nominally comparable with the previous poverty estimates from 1993–94, showed a marked reduction in poverty rates from to 1999-2000. Among rural households, estimated poverty fell from 37 to 27 percent, and among urban households from 33 to 24 percent, so that All India poverty fell a full ten points over the six year period, from 36 to 26 percent. Although these estimates were accepted by the Government of India, and vigorously defended by at least one government minister, there was widespread skepticism about their validity, with a fairly general belief that estimated poverty was too low because reported consumption over the 30-day reporting period had been upwardly biased by the simultaneous presence of the 7-day questions. But no one knew by how much the official estimates were out. Again, such problems are far from

unique to India. There is always a conflict between updating and improving a survey instrument on the one hand, and consistency of estimation on the other. Yet there have been few cases as dramatic as the Indian one, or where the consequences of the change were so little anticipated in advance.

One of the first writers to see the difficulties with the 55th Round, even before the results were published, was Abhijit Sen (2000), who delineated the contamination problems that were to dominate the interpretation of the full results of the 55th Round, which Sen refers to as a “failed experiment.” He argued that the traditional 30-day recall period is more reliable, based on a review of relative standard errors, though that argument would not convince a skeptic who believed that the longer recall period led to more bias. And like Minhas earlier, he cautioned against using the NAS means to scale up the survey measures of consumption, noting that, to the extent that the NSS understates consumption, it is most likely by undercounting the rich and their expenditures, so that most of the shortfall of NSS from NAS will be accounted for by expenditures by those at the top of the distribution. He argues that, in these circumstances, the NSS will underestimate the degree of inequality, so that scaling up the NSS data to match the NAS mean will understate poverty, and if inequality is widening, overstate the rate of decline of poverty. Such a procedure, by spreading the discrepancy proportionately over all households, effectively attributes to the poor some of the unmeasured consumption of the rich.

Although these arguments are initially persuasive, recent work by Mistiaen and Ravallion (2003) has shown that this apparently obvious argument, that selectively missing the rich understates inequality, does not necessarily hold, and that higher refusal rates by better-off households have an ambiguous effect on measured inequality. Missing the rich certainly reduces

spread, but it also reduces the mean, so that the effects on inequality, which is the ratio of the two, are ambiguous. Indeed, Deaton (2005) has constructed simple cases where, even with a higher probability of survey refusal by the rich, only the mean is biased down, and inequality is correctly measured, which would support the original practice of scaling up to the national accounts (appropriately adjusted to match the concept of consumption from the survey).

Although this sort of adjustment does not work in general, Deaton's argument shows that it is *not* true that poverty is *necessarily* underestimated by scaling up when the source of the discrepancy is selective refusal by better-off households.

5.2 Making adjustments

Although there were some who accepted the results of the 55th Round, and the large reductions in poverty that they implied, most scholars and commentators agreed with Sen that the survey was a “failed experiment,” whose results could not be taken at face value. As time passed, a number of authors developed ways of adjusting the data in order to provide credible corrections to the official estimates. Because the 55th Round is ultimately not compatible with the 50th (and earlier) rounds, all of these adjustments are based on assumptions that allow the imputation of missing data. Different authors made different assumptions, none are uncontroversial, and all have been debated.

Deaton (2001), and the related paper of Tarozzi (2003), base their corrections on the fact that an important section of the questionnaire was unchanged between the 50th and 55th Rounds, and can therefore be compared between them. This relates to items that are neither “high-frequency” nor “low-frequency” and for which a 30-day reporting period was used in all surveys. This group

of “30-day” goods comprises six broad categories, fuel and light, miscellaneous goods, miscellaneous services, non-institutional medical expenses, rent, and consumer cesses and taxes. The first four are quantitatively important items, and the first three are purchased by almost all households in all surveys. Total expenditure on the six categories accounts for more than 20 percent of all rural household expenditures, and more in urban areas. Importantly, expenditure on these items is also very well correlated with total household expenditure; in the 50th Round, the correlation between (the logarithm of) per capita total household expenditure and (the logarithm of) per capita household expenditure on these “30-day” goods is 0.79 in the rural sector and 0.86 in the urban sector. As a result, we have a part of expenditure that is consistently measured across the surveys, and that is highly correlated with total expenditure whose direct measurement cannot be trusted in the 55th Round. Deaton uses the 50th Round data to calculate the probability of being poor as a function of household per capita expenditure on the 30-day goods. This estimated probability can then be taken to the 55th Round, and used together with the (inflation-adjusted) expenditures on the 30-day goods in that round, to estimate for each household a probability that it is poor according to the procedures and definitions of the 50th Round. Adding up these probabilities over all households gives an estimate of the fraction in poverty as it would have been measured had the 55th Round questionnaire been identical to that in the 50th Round.

The validity of Deaton’s and Tarozzi’s procedures depends on two key assumptions: first, that changes elsewhere in the survey do not affect the way that the 30-day goods are reported, and second, that the probability of being poor (i.e. of the poverty line being more than per capita expenditure, measured according to 50th Round protocols) is the same function of 30-day expenditures in 1999–2000 as it was in 1993–94. The first assumption is unlikely to be

problematic, and if it is, it is hard to imagine that we could trust *any* 55th Round data. The second assumption could potentially fail. For example, if it were the case that, at any given level of per capita total expenditure, households are buying more of these 30-day goods now than they used to, then the procedure would understate poverty, and overstate its rate of decline over time.

Tarozzi (2003) shows that there is no evidence of any such trend in the rounds between 1993–94 and 1999–2000, but the possibility of failure remains and as we shall see, there is indeed some evidence for a problem.

According to Deaton’s calculations, most of the “official” decline in poverty is real. For rural households, where the official calculations show the headcount ratio falling from 37.3 percent in 1993–94 to 27.0 percent in 1999–2000, Deaton finds that the fall is from 37.3 to 30.2 percent, so that seven out of the ten points are confirmed. In the urban sector, he estimates a headcount ratio of 24.7 percent, as opposed to the official 23.6 percent, so that the fall in the poverty rate is reduced from 8.8 points to 7.5 points. The underlying fact that drives these results is that there was a very substantial increase in consumers’ expenditures on the six expenditure categories that were consistently surveyed using 30-day recall, and that it is hard to reconcile that increase without there having been a substantial increase in total expenditure, and thus in the fraction of the population that is poor.

A different set of internal corrections to the 55th Round were provided in a series of papers by Sundaram and Tendulkar (2001, 2002, 2003.) Because the questionnaire for the 55th Round asked households to report their high-frequency purchases (food, *pan*, and tobacco) at *both* thirty and seven days, the length of the interview was much longer than had been the case in the 50th Round. In consequence, the NSSO abandoned the traditional practice of asking the same

households who answered the consumer expenditure schedule also to answer the questions on the employment and unemployment schedule, instead using different sample of households for each schedule. Such a procedure has the disadvantage that there is no measure of household expenditure for the households in the employment-unemployment sample, so the NSSO introduced a new, abbreviated (one-page) questionnaire on consumers' expenditure that was used for the households in this sample. The reporting period for this supplementary survey is 30-days for all of the "high" and "intermediate" frequency goods, so that, in principle, these data can be used instead of the data on food, *pan* and tobacco in the CE survey, avoiding any contamination of the 30-day reports by the inclusion of the 7-day recall in the questionnaire.

When Sundaram and Tendulkar compare the 30-day reports from the employment and unemployment survey with the comparable 30-day expenditures from the consumption expenditure survey, they find that, at least at the mean, there is a reasonably good match. They use this evidence to argue that the 30-day reports in the main CE survey are more or less accurate, at least on average, in spite of the presence of the potentially contaminating 7-day recall questions. If this much is accepted, the only remaining source of inconsistency between the 50th and 55th Round questionnaires is the treatment of the low frequency items, clothing, durables, educational expenses, and institutional medical expenditures, which were surveyed at 30-days in the 50th Round, but at 365-days in the 55th. But Sundaram and Tendulkar note that the 50th Round actually solicited expenditures on these goods at *both* 30-days and 365-days, so that, if total expenditure for the 50th Round is reconstructed using the latter, it is possible to construct a notionally consistent measure of per capita expenditure in both 50th and 55th Rounds, and thence an estimate of poverty.

Any correction procedure requires a number of untestable assumptions, and, as with Deaton's method, there are a number of potentially weak links in Sundaram and Tendulkar's procedure. The concordance of the reports from the employment unemployment and consumer expenditure surveys is evidence only that those two measures are equal, and not necessarily that they are both equal to the hypothetical measure that would have been obtained had the 55th Round been carried out in the same way as was the 50th Round. This is more than a theoretical point, because the survey literature, as reviewed for example in Deaton and Grosh (2001), shows that abbreviation of questionnaires by aggregating groups of goods tends to reduce the total amount reported. It is therefore surprising that the highly aggregated employment unemployment questionnaire should give the same results as the highly disaggregated consumer expenditure questionnaire, especially if the presupposition is that the latter are biased upwards. And indeed the match is far from exact. The abbreviated questions generate less reported consumption for all food items, and much less for tobacco and *pan*. Secondly, the 50th Round's reports of expenditure on low frequency items (durables, footwear, education, etc.) over the last 365-days were collected side by side with reports of such expenditures over the last 30-days, while in the 55th Round, the 30-day question was not asked for these items. Much of the concern about the food items in the 55th Round has come from possibility that dual reporting periods generate different results than a single reporting period, and it is not clear why we can ignore this problem for the low frequency items in the 50th Round.

Sundaram and Tendulkar estimate that there has been substantial poverty decline in India in the 1990s, though less than, not only the official figures, but also than those calculated by Deaton's method. Based on the mixed reference periods for the 50th Round, (365 days for low

frequency, 30 days for everything else,) they estimate that rural poverty in 1993–94 was 34 percent and that this had fallen to 29 percent in 1999–2000 so that the Sundaram and Tendulkar decline is about half of the official one, as opposed to Deaton’s, which is about seventy percent of the official one. For urban households, they estimate poverty in 1993–94 to be 26 percent and find that it has fallen to 23 percent in 1999–2000 so that they confirm only about a third of the official decline, whereas Deaton confirms 85 percent of it. Note that Sundaram and Tendulkar’s estimates are not comparable with the official ones, in part because of their use of the mixed reference periods in the 50th Round, but also because they use different poverty lines from those used by the Planning Commission. Rather than work with the Planning Commission’s state- and sector-specific poverty lines, which have been called into doubt by a number of authors (and which we discuss below), they use the All India lines for 1973-74, updated only for the general rate of price inflation. Sundaram and Tendulkar have also extended their results to the major states, and have used the same corrections to investigate what has happened to the poverty rates of different social and economic groups, Sundaram and Tendulkar (2002, 2003). In line with other work, particularly that of Deaton and Drèze discussed below, it is clear that some groups have done very much better than others. In particular, Sundaram and Tendulkar find that while some of the most vulnerable groups (scheduled castes, agricultural laborers, and urban casual laborers) have had poverty reductions in line with those of the general population, others, such as the scheduled tribes, have been left behind.

Himanshu and Sen (2004) have recently rejoined the argument, challenging both Deaton’s and Sundaram and Tendulkar’s conclusions. They show that the Deaton and Tarozzi corrections to the 55th Round data have some quite unexpected consequences. Starting from the 50th Round

estimates of total expenditure from the “mixed” reference period (365 days for the low-frequency items and 30 days for everything else), they follow Deaton (who used total expenditure from the “uniform” reference period of 30-days), and calculate the probability of being poor conditional on expenditures on the consistently measured 30-day goods. Turning to the (contaminated) 55th Round, they repeat the calculation, to give the probability of being poor conditional on the consistently measured 30-day goods, with poverty calculated from total expenditure from 30-day responses for all but low frequency items, and 365-day responses for the latter. This second calculation is not to be trusted because of the contamination of the 30-day food responses by the presence of the 7-day questions though, if the 30-day responses are biased upwards by the presence of the 7-day questions, as was generally believed, this “probability of being poor” function should be too low. For any given expenditure on the consistently measured 30-day goods, food estimates are upwardly biased so that total expenditures are too high and poverty too low. In consequence, if Deaton’s assumption of stability of the function is correct, the contaminated probability-of-poverty schedule from the 55th Round should lie *below* the similar schedule from the 50th Round, provided also that the 365-day responses in the 50th Round are truly comparable to the 365-day responses in the 55th Round. In fact Himanshu and Sen show that the contaminated “probability of being poor” function is actually *above* the schedule from the 50th Round. In consequence, if food expenditures were indeed biased upwards in the 55th Round, Deaton’s stability assumption must be false, for example because the food Engel curve has shifted, with people at the same total expenditure level spending *less* on food relative to other things, such as the consistently-measured 30-day goods. If so, assessing poverty decline by looking at the increase in those expenditures will overstate the decline in poverty.

These findings are puzzling, not because it is impossible or even implausible that consumers have switched their expenditures from food to non-food at the same level of total expenditure, but because there is no evidence of them having done so prior to the 55th Round. Indeed, Tarozzi (2003) looked for such shifts in the rounds between the 50th and 55th, and found none. More investigation of Engel curves is certainly called for, particularly in the recently available 56th and 57th Round data. In the meantime, while Himanshu and Sen's work has certainly shown that Deaton's method, and the poverty estimates based on it, use assumptions that have implications that are hard to explain, it is far from clear that these problems do indeed come from a genuine shift in the food Engel curve that would cause the method to understate poverty. For example, there were many other changes in the 55th Round questionnaire (particularly compressions in the food schedule, see above) that might have affected the amount of food reported.

Himanshu and Sen also criticize Sundaram and Tendulkar's justification for their use of the uncorrected 30-day expenditures for food, *pan*, and tobacco (on grounds similar to those outlined above), and produce new estimates using their own set of corrections. They use the following procedure. For each category of consumers' expenditure, they calculate three possible estimates: (a) the mean from the consumer's expenditure section of the questionnaire, (b) the mean from the corresponding category from the employment-unemployment section of the questionnaire, and (c) a "counterfactual" based on extrapolation to the 55th Round of results from the 53rd and 56th Rounds. (The 54th Round was only a half-year survey, and appears to be unreliable.) Given that the estimates from the employment-unemployment part of the survey are likely to be biased down (because the categories are broadly aggregated), they first take whichever is the larger of (b) and (c), and second, choose the smaller of this and the original estimate (a). Their estimate of

the mean is thus the smaller of (a) and whichever is the larger of (b) and (c). Like the other methods of adjustment, these procedures are unlikely to command universal assent. If the 53rd Round estimates are too low (and indeed there has always been at least some suspicion of the thin rounds, and mean consumption from the 53rd Round relative to the national accounts estimate is low relative to trend) and if the downward bias in the employment and unemployment schedule estimates is large (as the literature suggests it ought to be), then taking the minimum will yield estimates of poverty that are too high.

Himanshu and Sen's final estimates are in line with Sen's original view, that there has been very little decline in headcount poverty in India in the 1990s. Using comparable mixed reference periods for both rounds, they estimate that between the 50th and 55th Rounds the rural headcount ratio fell by only 2.7 percentage points, from 31.9 percent to 29.1 percent, and the urban headcount ratio by 3.1 percentage points, from 29.2 percent to 26.1 percent. These estimates define the "pessimistic" pole in the Indian poverty debate. While we shall probably never know for sure what the results of the 55th Round would have been had the questionnaire been unchanged, most participants in the debate do not accept that there has been so little poverty reduction in the 1990s. Indeed, there is a great deal of evidence from sources other than the consumption surveys, for example on wage rates, on the ownership rates of durable goods, and on incomes in other surveys, none of which are perfect indicators on their own, but which taken together are extremely difficult to reconcile with an India in which poverty rates are not declining.

5.3 Other estimates of poverty

The poverty estimates by Deaton, Sundaram and Tendulkar, and Himanshu and Sen are all based

on internal corrections of the 55th round unit record data. A number of other researchers have followed alternative approaches. While there is no clear dividing line between these other methods and those already discussed, the papers discussed in this subsection rely more heavily on forecasting using external evidence. This is perhaps most explicit in the paper by Datt, Kozel, and Ravallion (2001) which takes a forecasting approach using an econometric model that links poverty rates to their plausible determinants, including agricultural yields, non-farm growth, development spending and inflation. They use the estimated model to project poverty in 1999-2000, ignoring the flawed data from the 55th Round. According to their calculations, the changes in the explanatory variables would have warranted a decrease in the headcount ratio from 39 percent of India's population in 1993-94 to 34 percent in 1999-00, suggesting that the pace of poverty reduction in the 1990s was slightly lower than in the 1980s, and lower than might be expected given India's high rate of economic growth in latter half of the 1990s. The differences between official and predicted rates of progress are in large part due to slower progress in some of India's largest and poorest states, particularly Bihar, Uttar Pradesh, and Maharashtra. Bihar and Uttar Pradesh alone account for over half the difference between official estimates and predicted poverty levels. Datt, Kozel and Ravallion's projections are similar to the adjusted calculations by Sundaram and Tendulkar, but show a good deal less (more) poverty reduction than does Deaton's (Himanshu and Sen's) method.

A more recent treatment that combines forecasting with limited use of the 55th round data is that of Kijima and Lanjouw (2003). Their forecasts are based on the household level factors that are the ultimate determinants of poverty, and the procedure works by using 50th Round data to regress household expenditure on household characteristics, such as education of household

members, land-holding, household size, caste, and a set of district dummies. The fitted regression is then taken to the 55th round data and used to predict total household expenditure based on the same factors, and these predictions are then used to estimate poverty. Kijima and Lanjouw's poverty projections show about half as much poverty reduction as those from the Deaton method, close to the estimates of Himanshu and Sen. The major question about these calculations is that, if there had been major reductions in poverty over the 1990s, and if, as might be hoped and expected, those reductions in poverty were driven by increases in the rate of return to factors such as labor, education, and land, then the calculations would not pick it up. The procedure is based on the assumption that the returns to factors cannot change, and is thus an inherently conservative methodology. Those who argue that the reforms have reduced poverty in India are not arguing that they have augmented India's supplies of factors, at least not in the short run, but that they have raised the rate of return to those factors, for example by allowing people to participate (directly or indirectly) in global markets that were previously closed to them. The Kijima and Lanjouw poverty calculations rule out any such effects by construction, and so cannot address the question of whether or not they exist.

Yet another set of poverty estimates are contained in Bery and Shukla (2003) which updates a similar exercise by Lal, Mohan, and Natarajan (2001). These studies use information collected from the Market Information Surveys of Households (MISH) that are run by the National Council on Applied Economic Research. MISH is a series of large scale annual surveys of household expenditures on consumer durables and other items of consumption. Because the design, sample size and the survey methodology have been consistent over time, Bery and Shukla argue that the MISH is useful for identifying trends in consumption patterns. The MISH also asks

several questions on household incomes, and these aggregate income measures are used to provide alternative estimates of poverty. The analysis of the MISH data suggests that poverty has fallen sharply in India, whether estimated from the income data, or from the increase in the ownership of durable goods. One major concern about the MISH surveys is the adequacy of a single income question “What is your annual household income from all sources?” Household income is always a difficult concept to explain and to measure, especially for rural households, many of whom are self-employed in agriculture. Incomes for such households require a great deal of imputation, as well as a careful separation of business from personal expenses. In consequence, most creators and users of household surveys would not regard such a question as useful. Unfortunately, it is difficult to explore these issues in detail, because the MISH data are proprietary and have never been made available to independent researchers, even many years after their proprietary value has expired.

Surjit Bhalla has been one of the most consistent advocates of the position that poverty has fallen rapidly in the 1990s, not only in India, but in the world as a whole. In Bhalla (2001), he argues that there has been a very sharp decline in poverty in India in the 1990s, and that the official estimate of 24 percent in 1999–2000 is an underestimate. He writes that “it is almost incontrovertible that poverty in India was less than 15 percent in 1999-2000.” He also argues that inequality has *improved* in the late 1990s. Bhalla’s conclusions are based on the argument that the national accounts estimates of consumption are more reliable than the survey estimates, and on a procedure that scales each household’s consumption of each item so that the totals from the survey agree with the totals from the national accounts on an item by item basis, at least up to a scale factor. Not surprisingly, these imputations induce a rapid decrease in poverty. Bhalla does

not address the detailed arguments by Minhas against this sort of adjustment, nor Kulsheshta and Kar's demonstration of the inferiority of the numbers that Bhalla treats as correct compared with those that he rejects. Indeed, Bhalla does not reference their papers. And just as Kijima and Lanjouw *assume* that there has been no increase in the return to factors in the 1990s, which assumes away one of the main arguments of the globalizers, Bhalla *assumes* that growth at the mean automatically translates into growth for the poor, which assumes away one of the main arguments of the anti-globalizers. The use of either assumption bypasses the poverty debate because the answer is guaranteed before we do any calculations.

Bhalla's argument that there has been no increase in inequality is based on measures that appear to be taken directly from the unadjusted 1999–2000 survey, and are compared with similar measures from earlier surveys. But as we have seen, the unadjusted data from the 55th Round understate measured inequality because of the change in response periods for the low frequency items such as durables and clothing.

Bhalla's work is important, not so much for its calculations and conclusions, which are not credible, but because it represents an important and widespread strand in recent Indian thinking, that the reforms have not only been associated with rapid growth of national income, but with the virtual elimination of poverty in India.

5.4 The selection of poverty lines and the effects on poverty

The calculation of poverty measures requires two components, a distribution of household expenditure, and a poverty line or cutoff that separates poor from non-poor households. As is true in many countries, the history of Indian poverty lines is a case study in the interaction of science

and politics, with essentially political decisions often claiming a scientific basis, sometimes with justification, and sometimes without. Although poverty lines are often linked to the amount of money that households need to be able to buy a minimally satisfactory diet, the use and long-term survival of poverty lines depends on policymakers and others accepting them as useful. Rudra (1974) discusses the history of Indian poverty lines up to that time. He writes about the “magic number” of 20 Rupees per head in 1960–61 prices, and shows that a food-based analysis would lead to a considerably higher number. Yet the “magic number” persisted, as similar magic numbers have persisted in other countries, not because they are correct in any scientific or nutritional sense, but because, once established as useful in economic and political discussions, poverty numbers are resistant to change, unless they are wildly at variance with people’s intuitive notions of the amount of money needed to stay out of poverty.

From late 1970s into the mid 1990s, the Planning Commission used only two poverty lines for per capita household expenditure in India, 49 rupees for rural households, and 57 rupees for urban households at 1973–4 prices, so that prices were taken to be about 16 percent higher for urban households, close to the 15 percent urban price differential estimated by Bhattacharya and Chatterjee (1974). The poverty lines were held constant in real terms, and were converted to current rupees using the implicit price deflator of consumption in the National Accounts. This process had the disadvantage of ignoring interstate differences in price levels, as well as variations from state to state in urban to rural price differential. Furthermore, it might be doubted whether the national accounts consumption deflator was an ideal measure of inflation for households near the poverty line. These problems (and several others) were dealt with by an Expert Group in 1993, whose recommendations for new poverty lines were adopted (in a

somewhat modified form) by the Planning Commission, and these “Expert Group” poverty lines have been used in the official calculations of poverty in India from 1983 (back-casting the methodology) to the present day. This is the current system as described in Section 2 above.

The Expert Group poverty lines have a serious problem in that the urban to rural price differentials that are implied by them are too large to be credible. It is unclear how this happened, whether there was an error in the calculations, or whether the price indexes that went into the calculations produced the result through some unfortunate cumulative effect. Certainly, the state by state urban and rural poverty lines were calculated independently, without consideration of the implicit urban to rural price differentials. In any case, the average ratio of urban to rural poverty lines is around 1.4, and varies widely across states; in the 50th Round (1993–94), it is more than 1.7 in Andhra Pradesh, and nearly as much in Maharashtra, Madhya Pradesh, and Karnataka, but actually less than unity in Assam. As a result, the official headcount ratio measures of poverty are actually *higher* in urban than rural areas in some states, and the All India headcount ratios differ little between the two sectors. In Andhra Pradesh, which is the most dramatic example, the 1999–2000 official estimates show that 27.2 percent of the urban population were poor, but only 10.8 percent of the rural population.

A perhaps less serious problem with the poverty lines, although the issue has attracted more comment, is the question of the accuracy of the state level price indexes. These are reweighted only infrequently, so that, for example, the CPIAL used weights based on a 1960–61 survey until 1995. And although the CPIAL and CPIIW indexes are almost certainly better than the price deflator of national accounts consumption, it is unclear whether the prices or the weights that go into these indexes are the right ones for a national poverty measure.

Alternative price indexes can be calculated directly from the NSSO's consumption surveys. For almost all of the foods in the survey, as well as for tobacco, alcohol, and fuels, respondents report both expenditures and quantities. It is therefore possible to calculate a price, or more accurately a unit value, for each good for each household, and these can be used to form price indexes, for urban, for rural, for states, and for different rounds of the survey. The details of this work were originally laid out in Deaton and Tarozzi (2000) and were brought up to date in the brief note by Deaton (2003), who presents a set of poverty lines based on the indexes. These price indexes are quite different from the price indexes implicit in the official poverty lines, and much closer to the uniform 16 percent urban/rural price differential that was originally calculated, using similar procedures, by Bhattacharya and Chatterjee (1974). As a result, headcount ratios calculated using Deaton and Tarozzi's lines show much higher rates of rural relative to urban poverty than do the traditional lines. Their price indexes also rise somewhat less rapidly than do the official indexes, with the result that the associated poverty rates for the country as a whole decline somewhat more rapidly than do the official rates, at least until we reach the 55th Round in 1999-2000, where the contamination becomes an issue.

Not all of the papers reviewed above have used the official poverty lines although most present calculations using some version of the official lines in order to compare their poverty estimates with those presented by the Planning Commission. In our previous discussion, we have tended to focus on the different estimates of the *change* in poverty rates during the 1990s, if only because changes are certainly more robust to the choice of poverty lines than are levels. But just *how* robust the changes are to the different poverty lines used by different authors is a largely unexplored topic.

Deaton and Tarozzi's poverty lines, together with their procedures for correcting the 55th Round data, form the basis for the analysis of poverty and inequality in the paper by Deaton and Jean Drèze (2003). According to their estimates, there has been fairly steady poverty decline in India in the 1970s, 1980s, and 1990s, with neither acceleration nor deceleration over the last decade. Deaton and Drèze argue that their estimates are broadly consistent with a range of other evidence, including state level growth rates from state GDP accounts and the growth of agricultural wages. The Deaton and Tarozzi poverty lines sometimes make a dramatic difference to the estimates, particularly for urban-rural comparisons. In Andhra Pradesh, quoted above, they estimate that only 10.5 percent of the urban population were poor in 1999-2000 (27.2 percent in the official numbers), but that 26.2 percent of the rural population were poor (10.5 percent in the official numbers.) They also note that there has been a marked *increase* in consumption inequality in the late 1990s, between states, with the already better-off states in the south and west growing more rapidly than the poorer states in the north and east, between rural and urban households, with growth a good deal more rapid for the latter, and within the urban sectors of many states, where consumption has been growing more rapidly among the best off.

It is important to note that the trends in inequality are obscured by the questionnaire changes in the 55th Round. The substitution of a 365-day for a 30-day reference period for the low frequency items pulls up the bottom tail of the distribution of expenditures for those items, while simultaneously depressing its mean, so that expenditures on low frequency items are less unequally distributed using a 365-day as opposed to 30-day recall. This effect carries through to the distribution of per capita total household expenditure, whose distribution is artificially compressed in the 55th Round compared with the 50th. This questionnaire-driven reduction in

measured inequality offsets and obscures the underlying increase in inequality which cannot be detected if the old and new questionnaires are compared without explicit correction.

Deaton and Drèze also note that progress on consumption poverty is only one dimension of progress on poverty, and that broader perspectives reveal a picture that is a good deal more mixed. Although there has been rapid and most welcome progress in enrolment rates in elementary education, there has been a slowing down in progress in reducing the infant mortality rate. And even the economic progress has been far from even, with some groups losing out as others made historically impressive gains.

A different perspective on inequality is provided by Banerjee and Piketty (2004). Their data come from the individual income tax returns which, under some fairly heroic assumptions, can be used to document the very top end of the Indian income distribution. As many observers have conjectured, Indians with the highest incomes have done extremely well during the boom of the 1990s. Banerjee and Piketty calculate that the average incomes of the top percent have increased by 70 percent in real terms, while at the very top, among the top one percent of one percent, average incomes tripled. Banerjee and Piketty provide the first good evidence on the extent to which there has been increasing inequality among the very best-off Indians. It is also relevant for the debate between the national accounts and the surveys; if inequality is increasing, and if people are less likely to cooperate with the survey the higher their income, then the ratio of measured consumption in the surveys to true consumption will fall. And indeed, Banerjee and Piketty show that around a quarter of the increase in the NAS/NSS gap can plausibly be attributed to the increase in inequality among high income Indians that is documented in their chapter.

6. Lessons and wider implications

Despite an extensive body of empirical work by eminent researchers, the debate on what happened to India poverty in the 1990s continues. No doubt there will be more reinterpretations of the 1999–2000 survey, and new data are continuously being collected. Indeed there are two (thin) rounds since the large survey, the 56th and 57th rounds. But these provide little clarification. The 56th Round, conducted in 2000-2001, shows a further reduction in poverty, very much along the trend line calculated by Deaton and Drèze. The 57th Round data, from 2001-2, show that there was a sharp *increase* in poverty compared with the previous year. It is unclear why this should be so, and there is no obvious economic event that would have been expected to generate more measured poverty. In fact, the results from this latest round are more in line with the poverty estimates from the 51st through 53rd Rounds, in accord with the calculations of Sen and Himanshu that there has been little or no reduction in poverty. Yet the other evidence on living standards appears to contradict such a view.

In May of 2004, there was an election in India, in which the Congress Party and its allies emerged as unexpected winners. The election campaign of the ruling BJP had featured “India Shining,” a vision that is consistent with the views of the poverty optimists, that the 1990s saw the virtual elimination of poverty in India, and that extrapolated the successes of the urban middle classes to all of India. Many have argued that the election result reflects the truth that many of India’s poor did not share in the growth elsewhere in the economy, as argued by the poverty pessimists. The testing of such a conjecture would be a major undertaking, and it is wise to remember that the complexities of coalition politics in India make any single explanation more than usually hazardous. But one particular case is worth mentioning. In Andhra Pradesh, whose

government was in the vanguard of proclaiming the new India, and where the election reversal was one of the most dramatic, the official poverty figures showed a decline in rural poverty of a third (15.9 percent to 10.5 percent) between 1993–94 and 1999–2000, whereas all of the “corrected” estimates showed much smaller reductions, by 10 percent in Drèze and Deaton, 8 percent in Sundaram and Tendulkar, and by 11 percent in Himanshu and Sen. In Andhra, as in all of India, the official data dramatically overestimated urban poverty relative to rural poverty, so that the urban people who were doing well from the reforms could perhaps be forgiven for assuming that their success was being shared by their rural fellows. Karnataka, with Bangalore replacing Hyderabad as a great technology center is another example where both urban poverty relative to rural poverty, and the rate of decline of rural poverty, were much exaggerated. And again, there were election reversals for those in power. It is not impossible that the breakdown of the poverty monitoring system, its biases, and the room for “mindless” optimism that they created, helped instill a false sense of security in reformist governments in India.

Although election results rarely hinge on the measurement of poverty, it is certainly impossible to pursue a coherent anti-poverty policy without an adequate poverty monitoring system. Yet, in spite of its occasional lapses, India’s system is one of the best developed in the world. The statisticians who work in the National Sample Survey Organization are highly skilled, and the field staff are disciplined and well-trained. India has an enviable capacity for the collection of survey data and the monitoring of living standards. Perhaps even more important are the facts that the system is domestically-owned, that Indian politicians and policy-makers are accountable for poverty reduction, and that the Indian press and Indian academics are deeply involved in interpreting and monitoring what is happening to poverty. The politicization of data

collection and interpretation is often bemoaned, and indeed political pressures and controversies can sometimes lead to poor decisions about data collection. But political accountability for poverty is key to its reduction. The principles of insulating statistical services from undue political influence are well-understood, Martin et al. (2002), but poverty monitoring systems are ultimately part of the political process. Most countries in the world can only envy India, not only its statistical capacity, but also the central part that poverty and poverty measurement in Indian public life.

The recent experience of poverty monitoring in India also contains a number of more specific lessons that are generally applicable. There is no substitute for survey capacity. In India, the census, the national accounts, and the sample surveys are all run by different organizations, and there is a very large private data collection capability so that, for example, India's Demographic and Health Surveys (The National Health and Fertility Surveys) are not carried out by central government statistical agencies. The fact that sample sizes do not (and should not) expand in proportion to population, and that there are substantial fixed costs of surveys gives large countries an advantage in survey cost per capita. The Indian capacity is in sharp contrast to the situation in many African countries, where a single statistical office, without the Indian traditions of statistical excellence, struggle to fulfil all of the functions listed above. As a result, it is impossible to collect data on living standards in a census year, or when there is a Demographic and Health Survey. Poverty monitoring is done only sporadically, perhaps once or twice a decade. At such low frequency, surveys are usually not compatible with one another, with the consequences that we have seen in India.

The Indian debate also illustrates the importance of putting the underlying data into the public

domain. Almost none of the corrections to the 55th Round could have been done without access to the raw data, and most of the analysis is done locally, by Indian scholars and commentators, and this openness is a major change from policy of a decade ago, when the raw data were not released to outside researchers. That said, there is still less feedback from the user community to the NSSO, and too many decisions about survey design have been taken without adequate transparency and debate.

The Indian (and other) experience has also clarified the importance of annual consumption or income surveys. In the early days of the Indian NSS, there were annual surveys, but it was thought that poverty changed too slowly to make it necessary to measure it every year. But this argument does not make adequate allowance for variability of conditions and of survey structure. In largely agricultural societies, where the state of the harvest varies from year to year, a five year cycle runs the risk that the survey coincides with an unusual harvest, so that it can take ten or fifteen years to establish a trend. Similarly, if a particular survey is compromised, as in the 55th Round of the NSS in India, there can be unacceptably long gaps between policies and their evaluations. In India, there will be no poverty measures that are comparable with the 1993–94 estimates until the 2005–2006 survey, whose results will be available only towards the end of 2007. The presence of a good, annual, survey, even if not fully comprehensive, is an insurance against all these problems. In India, the “thin” rounds could play this role in principle, but there have been persistent doubts about their validity, not so much on sample size, which is large enough for national monitoring and for many large states, but on sampling methods, and on the frame from which the samples are drawn.

It is tempting also to emphasize the benefits of a *consistent* survey design, although it is also

important that survey content and design can change as the economy develops, as the focus of interest changes, and as survey technologies improve. The key point is that survey design must change slowly relative to the frequency of poverty monitoring. Revisions once every five years can be acceptable if poverty surveys are run every year, but catastrophic if poverty monitoring is done at five year intervals. As we saw in India, most of the debate is about *changes* in poverty, not the level in any particular year, so a single survey by itself is of little value unless without an earlier comparable survey to which it can be compared. In any case, when major design changes (such as changes in reporting periods) are implemented, it is desirable that the survey be randomly split between the old and new design, so that there is a basis for both forward and backward comparability.

Another general issue is the relationship between the National Accounts and the surveys. During the Indian debate, it became clear that the two measures of consumption are *not* the same thing, and that there is no straightforward way to adjust one to the other. It was also clear that there are large errors in the National Accounts, so that when surveys and National Accounts disagree, it should never be assumed that one or the other is correct. In many countries around the world, the growth in the national accounts appears to be greater than the growth in survey measures of income and consumption. This makes it difficult to use the National Accounts as a cross-check, or to forecast poverty trends in the absence of good survey data. Once again, there is no short-cut to poverty monitoring without a regular survey. This is not because surveys do not have problems of their own, nor even that survey measures are more accurate than extrapolations from the national accounts. The key advantage of surveys is they provide the only *direct* measures of what we are ultimately concerned with, the living standards of households.

Inevitably, mistakes will be made, surveys will be compromised by internal or external factors, so that poverty assessments will have to be made using imperfectly comparable surveys. The Indian experience illustrates the possibility of repair to enhance the credibility of estimates. But that experience also made it clear that repairs, however creative, are a poor substitute for the collection of clean, credible, and comprehensive data. What are convincing assumptions to one can be absurd to another, and people's political positions seem to play a role in the assumptions that they are prepared to make. Nevertheless, the Indian debate has shown that discussion and advance is possible, even among those with very different preconceptions, and that the balance of opinion can be changed by well-reasoned and transparent argument. Some of the methods used in the Indian debate were country-specific (for example, the use of the supplementary consumption questions in the employment-unemployment survey), but others have quite general applicability for the repair of partially contaminated data. This is an area in which there are likely to be returns to good, technical statistical work, although it will always be necessary to exercise caution that the methods, which are sometimes less than transparent, are not effectively assuming the answer at the outset.

Finally, there are no substitutes for good, regular, comparable surveys, for local ownership of the poverty debate, and most important of all, for political accountability for poverty reduction.

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