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, both politically and statistically. The Indian debate has run parallel to, and is itself an important component 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 an acceleration in the rate of poverty reduction, 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 consensus on what happened to Indian poverty in the 1990s, there is good evidence both that poverty is falling and that the official estimates of poverty reduction are too optimistic, particularly for rural India. 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 international relevance.

Keywords: India, poverty, measurement, liberalization, growth, surveys, national accounts

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.

1. 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). These surveys, pioneered by P. C. Mahalanobis in the 1940s and 1950s, see Mahalanobis and Sen (1954), were the world's first system of household surveys conducted using the principles of random sampling that had been established in the 1920s and 1930s. Current NSS 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.) (The next will be the 61st Round of 2004–05, which is in the field at the time of writing.) 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 there exist intermediate, smaller, surveys, the Planning Commission does not use them to make official poverty counts, 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 that is specific to the state and sector (urban or rural) in which they live. The state and sectoral 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 for either 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 derived them from earlier academic studies that had compared prices across states. The poverty data from 1983 onwards are available according to current procedures, and it is these numbers which are the subject of the debate.

2 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. 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. The proponents of such procedures believe that national accounts data are superior to survey data, so that it is appropriate to use the additional information they contain to adjust the surveys. The opponents question the automatic assumption of superiority of the national accounts and note that scaling up the surveys by-passes a central question in the debate, which is whether the growth in the national accounts shows up in improvements in the living standards of the poor.

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 Chatterjhee (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 in any country, particularly anyone who does not appreciate the complexities and approximations involved in the construction of the former. It was Minhas' work 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. Cooking oil, particularly *vanaspati*, provides a good example from India. The National Accounts estimate consumption of vanaspati as the total production less imports, plus exports, less consumption by government or business. In an economy in which all *vanaspati* is used for cooking in households, this gives the right answer. However, as the economy grows, consumers eat more meals out, so that an increasing fraction of *vanaspati* is now used by commercial food suppliers, restaurants, hotels, or street vendors. Consumers' expenditure on these services is derived from (fairly shaky) data on the gross output of the service sector, adjusted to a value-added basis by deducting a value of intermediate inputs, including *vanaspati*. At best, this adjustment is done using one of the "rates and ratios," which means progressive and increasing overstatement if intermediation increases with income and the "rates and ratios" are infrequently adjusted. In the case of *vanaspati* in India, no adjustment is made at all, so that all *vanaspati* used in restaurants is counted twice, helping to overstate the rate of growth of consumption and GDP, and to increase the ratio of NAS to survey consumption, see Kulshrestha and Kar (2005).

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) provide contemporary discussions of the discrepancy. Kulshreshtha and Kar are the statisticians at the Central Statistical Organization who were primarily responsible for the production of the national accounts in the 1990s, 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." While this is perhaps too sanguine—the non-food items also play a role in poverty measurement—their work certainly establishes that there can be no automatic presumption in favor of the national accounts.

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 Kulsheshtra 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. Because the changed treatment of FISIM in the SNA has been adopted gradually by most countries over the last two decades, it has presumably contributed to

3. Survey methodology: reporting periods

The design of the Indian surveys has evolved over time, and is continually under discussion. One of the most important design issues for poverty measurement is the length of the reporting period. The NSS ran a series of experiments in Rounds 51 through 54, from 1994 through 1998, in which households were randomly assigned to one of two questionnaires with different reporting periods, each of which 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 (hospital) medical care, and educational expenses) and 30 days for everything else gave poverty counts that were only *a half* of those from ther questionnaire 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.

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 without 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 better. 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 uniform 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. 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 too long for most foods.

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

4.1 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 so that 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 prior to 1999–2000, 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 significantly 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 then available, 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 internal debate, a "compromise" solution was adopted according to which, 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 incompatible with any previous survey, so that the consumption and poverty estimates based upon it cannot reliably be used to assess trends, or even the change from the previous (50th Round) large survey. 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, not both. 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, most likely, 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 a separate sample of 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.

In spite of all these difficulties, the 30-day responses were adopted as the basis of 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 between 1993–94 and 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, on the plausible supposition 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 was Abhijit Sen (2000) who, even before the results were published, delineated the contamination problems that were to dominate the interpretation of the 55th Round, which he refers to as a "failed experiment."

4.2 Making adjustments

A number of authors developed ways of adjusting the 55th Round expenditure data in order to provide credible corrections to the official estimates. All of these adjustments are based on assumptions that allow the imputation of missing or contaminated data and, by the nature of the problem, at least some component of the assumptions is untestable. Different authors made different assumptions, none are uncontroversial, and all have been debated. As we shall see below, the official counts have also been challenged based on the poverty lines that they use, and how they are updated over time. But we leave these issues for the moment, and focus on the adjustments that have been proposed to deal with changes in the design of the questionnaire.

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. If the probability of being poor conditional on expenditures on the consistently-measured goods does not change from 1993–94 to 1999–2000, then we calibrate that probability from the 50th Round, and use it together with expenditure on the consistent items in the 55th Round in order to estimate poverty in that round.

According to Deaton and Tarozzi'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 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 corrections have been provided by Sundaram and Tendulkar (2001, 2002, 2003.) Using the results of an abbreviated consumption questionnaire given to the employment and unemployment subsample of the 55th Round they argue that, in spite of the presence of the 7-day questions, the 30-day responses were not much contaminated. If so, the only source of inconsistency is 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 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 ('mixed reference period" MRP) measure of per capita expenditure in both 50th and 55th Rounds.

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. 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 MRPs 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 (as 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.

Sen and Himanshu (2004) have very recently challenged both Deaton's and Sundaram and Tendulkar's conclusions. On the former, they apply the "consistent goods" methodology to calculate the probability of being poor according to the MRP definition of consumption used by Sundaram and Tendulkar. Given the validity of the mixed reference period correction, the incompatibility of the 50th and 55th Rounds comes from the responses on food, paan, and tobacco, which were most likely biased up in the 55th Round by the presence of the 7-day questions. In consequence, applying Deaton's method should adjust the poverty counts upwards compared with unadjusted MRP counts. But Sen and Himanshu show that exactly the reverse is true. Since it is extremely implausible that the presence of the 7-day questions *decreased* the 30-day reports, something has clearly gone wrong. Although exactly what is not yet entirely clear, the most likely possibility is that Indian households have been shifting their consumption away from food towards an assortment of non-food goods and services, not just by moving along the Engel curves (which would be no problem for Deaton's method) but by shifting the Engel curves themselves. As a result, the increase in miscellaneous goods and services from 1993–94 to 1999–2000 cannot be used to measure the extent of poverty reduction, as Deaton supposed.

Sen and Himanshu also find unconvincing Sundaram and Tendulkar's justification for their use of the uncorrected 30-day expenditures for food, *pan*, and tobacco and produce new estimates of their own using different, sensible, but largely *ad hoc* corrections. Their 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, it is implausible 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.

4.3 Other estimates of poverty

The poverty estimates by Deaton, Sundaram and Tendulkar, and Sen and Himanshu are all based on corrections of the 55th round unit record data from within the data itself. A number of other researchers have followed alternative approaches, particularly linking poverty to 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' 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 Maharasthra. 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 (Sen and Himanshu'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 among 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 Sen and Himanshu. The major question about these calculations is that, if there had been major reductions in poverty over the 1990s, and if, as might be expected (and hoped), 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), using the national accounts to adjust the surveys, 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." Bhalla's work defines the opposite pole to Sen and Himanshu in the Indian poverty debate.

5 The choice of poverty lines

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. While the recent debate has focused more on the measurement of expenditures, poverty lines are equally important. How they are updated over time and adjusted over regions or urban and rural households, has a major effect on poverty estimates, how they change over time, and on how poverty is distributed over space. In India, as in many countries, a base poverty line is spatially and temporally adjusted using price indexes, so the selection and construction of these indexes becomes a key input into poverty measurement.

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, more often 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. For example, Rudra (1974), discussing the history of Indian poverty lines up to that time, 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) using unit value data from the NSS. 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 1 above.

The Expert Group poverty lines have a serious problem in that the urban to rural price differentials that they imply 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. Even apart from the rural to urban differentials, it is unclear whether there is any good basis for the difference in poverty lines across states, given that the studies used by the Expert Group were outdated even a decade ago.

Another serious issue is the accuracy of the rate of inflation in the state level price indexes where errors will induce errors in the trend rate of poverty reduction. These indexes 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.

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 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. So even if their estimates for 1999–2000 are over optimistic, as argued by Sen and Himanshu, their general assessment of trends is unlikely to be wildly incorrect.

Drèze and Deaton show that the new 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 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 evidence on the extent to which there has been increasing inequality among the very best-off Indians, and this is likely also to trigger further analysis and debate. This work 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. Updates and lessons

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—though diminishing returns have surely set in—and new data are continuously being collected. There have been three (thin) rounds since the large survey, the 56th, 57th, and 58th, the last a half year survey. But a preliminary examination suggests as much confusion as 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, which appears to have been even more sharply reversed in the 58th collected in the last six months of 2001–02. It is unclear why there should be such a pattern, although the disposal of large public cereal stocks in 2000–01 temporarily reduced the rural consumer price index and certainly induced a fall in rural poverty. Yet these sharp fluctuations from survey to survey also raise the suspicion that, as was the case in 1994 to 1998, the thin round surveys have some little-understood problem in their sampling.

It is impossible for any country to pursue a coherent anti-poverty policy without an adequate poverty monitoring system. The statistical problems and debates discussed in this paper happened in a country which has one of the best developed survey capacities 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. Matters are almost certainly worse elsewhere.

What conclusions can be drawn from the great Indian poverty debate, for India itself, and for other countries, many of whom can only envy India's statistical capacity? Much of what we have to say is most relevant to statistical offices who are responsible for the survey design that has been the main topic our review. But policymakers are responsible for the funding of statistical offices and for the rules under which they operate, as well as for interpreting the data and using them in policy. And researchers also have an important role in providing independent analyses, raising alarms about problems with the data, and helping generate the debate in which policy evolves. We address each group in turn.

6.1 Lessons for statistical offices

The Indian experience has demonstrated the importance of *annual* consumption or income surveys. Having a major poverty survey only every five or six years means that the failure of one such survey means that there is no reliable estimate of the rate of poverty decline for more than a decade. 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, and if this coincides with important policy changes, there can be an unacceptably long gap between a policy change and measurement of the post-policy change in poverty. 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, which is typically not the same as that used for the large poverty-monitoring rounds. In consequence, and although the NSS is in the field collecting consumption data on a continuous basis, the program of surveys dos not provide regular annual surveys on a consistent basis.

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 relatively 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, which is essentially arbitrary, 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.

The Indian NSS has a long tradition of *experimentation*, or using what are essentially randomized controlled trials to assess the consequences of changes in survey practice. The fact that such experimentation precipitated the failure of the major 1999–2000 round should not be taken as a condemnation of experimentation, but of an oversight mechanism that failed to stop the introduction of what was effectively a completely untested design into an important and long-awaited survey. It is often difficult to persuade statistical offices to undertake costly experiments that have no immediate input into published statistics. But without them, there is no satisfactory progress in survey design, and if properly used, they help prevent bad designs ever being adopted into regular production surveys. The NSSO has undertaken experiments since its foundation in the 1940s, and that is a tradition that should be followed more widely elsewhere.

Another general issue is the relationship between the National Accounts and the surveys. These are usually the responsibility of different government agencies, which complicates, although it does not prevent, collaboration on resolving discrepancies and using each source of data to improve the other. Making such collaboration easier, even to the extent of placing both agencies within an overall national statistical office, is likely to improve the performance of both surveys and national accounts in many countries. More substantively, the Indian debate showed that for the agency charged with monitoring poverty (in India the Planning Commission, which is separate from the NSS), there is no simple shortcut to better poverty estimates by scaling up the survey data to match the national accounts. The Indian debate made it 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.

Finally, the Indian debate shows how important it is for the survey agency to develop a dialog with outside users, particularly independent researchers and journalists. Very little of the Indian debate would have taken place without public access to the raw data, particularly by local analysts. This openness by the NSSO is a major change from policy of a decade ago, when the raw data were not released to outside researchers. That said, there still appears to be relatively little feedback from the user community to the NSSO, for example to help improve survey design, and the NSSO has not so far been successful in developing a mechanism through which users can bring questions and suggestions to those who process and design the surveys.

6.2 Lessons for policymakers

Policymakers are accountable to the whole population, not just the poor. But poverty reduction is a key aim of policy in India and in most other poor countries, policymakers are accountable to the public for poverty reduction, and progress cannot be monitored without an adequate statistical agency. Policymakers need not only to pay for such a system, but they need to pay for a system that produces annual surveys of consumption or income. They are also responsible for ensuring the openness of statistical data, including unit record data, something that statistical offices are sometimes reluctant to implement without orders from above. Policies and laws under which data are made available need to be put in place and administered. Without domestic openness and debate, policymakers are unlikely to be held accountable for their policies. While many countries do not have the domestic statistical or analytical capacity to generate a debate like the Indian one, moving in that direction would be a worthy aim for many countries.

There is no suggestion here that the statistical failures in India in the 1990s were the result of undue interference by politicians or policymakers on data collection or publication. Yet *politics* in the broad sense played a role because, in the context of the evaluation of the reforms, the right had an interest in showing low poverty, and the left in showing high poverty, and this undoubtedly intensified the debate on survey design, and led to the unfortunate compromise design that, at least temporarily, destroyed the poverty monitoring system.

This politicization of data collection and interpretation is often bemoaned. Yet political accountability for poverty is an essential key to its reduction, and policymakers have a legitimate interest in monitoring the statistical system, and asking for changes that serve their interests. 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 just its statistical capacity, but also the central part that poverty and poverty measurement plays in Indian public life.

6.3 Lessons for researchers

Inevitably, mistakes are sometimes made, and survey data are 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 seem absurd to another, and people’s political positions inevitably 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.

Another lesson for researchers from India is the importance of high-quality domestic researchers. Analysis by foreign scholars or by World Bank researchers can often be helpful, but it has a quite different effect from a domestic debate among local researchers, policymakers, and the press. Although few countries currently have India's group of domestic researchers who can work with the basic data, it should be noted that this is a relatively new phenomenon in India too. The Indian press also plays a distinguished role in the debate, not only through the daily newspapers that regularly give attention to new findings on poverty, and regularly carry serious op-ed pieces, but also through the presence of *Economic and Political Weekly*, in the pages of which much of the Indian debate took place. *EPW* is a cross between an academic journal (it has equations, and econometrics) and a magazine, such as *The Economist*. It provides rapid publication and acts as a unique bridge between research, the press, and policymakers, and it could well be emulated in other countries, including many rich countries.

5. List of works cited:

Banerjee, Abhijit, and Thomas Piketty, 2004, Are the rich growing richer: evidence from the Indian tax data," *World Bank Economic Review*, forthcoming.

Bardhan, Pranab and T. N. Srinivasan, *Poverty and Income Distribution in India,* Calcutta. Statistical Publishing Society, 1974.

Bery, Suman and R. K. Shukla, 2003, "NCAER's Market Information Survey of Households—statistical properties and applications for policy analysis,"*Economic and Political Weekly*, 37(4), Jan. 25, 350–354.

Bhalla, Surjit S., 2002, *Imagine there is no country: poverty, inequality, and growth in the era of globalization*, Washington, DC. Institute for International Economics.

Bhalla, Surjit, S., 2003, "Recounting the poor: poverty in India, 1983-1999," *Economic and Political Weekly*, 37(4), Jan 25, 338–49.

Chaterjee, G. S., and N. Bhattacharya, 1974, “Between state variations in consumer prices and per capita household consumption in rural India,” in Bardhan and Srinivasan (1974).
Datt, Gaurav. “Has poverty declined since economic reforms? Statistical data analysis." *Economic and Political Weekly*, no. December 11-17. (1999).
Datt, Gaurav, Valerie Kozel, and Martin Ravallion, 2004, “A model-based assessment of India’s progress in reducing poverty in the 1990s,” *Economic and Political Weekly*, 37(4), Jan 25, 355–61.
Deaton, Angus, 2003, “Adjusted Indian poverty estimates for 1999-2000,” *Economic and Political Weekly*, 37(4), Jan 25, 322–26.
Deaton, Angus, 2003, “Prices and poverty in India, 1987-2000,” *Economic and Political Weekly*, 37(4), Jan. 25, 362–68.
Deaton, Angus, 2005, “Measuring poverty in a growing world: or measuring growth in a poor world,” *Review of Economics and Statistics*, forthcoming (February.)
Deaton, Angus and Jean Drèze, 2002, “Poverty and inequality in India, a reexamination,” *Economic and Political Weekly*, Sep. 7, 3729–48.
Deaton, Angus and Valerie Kozel, 2004, *Data and dogma: the great Indian poverty debate*, New Delhi. Macmillan.
Deaton, Angus, and Alessandro Tarozzi, 2004,. “Prices and poverty in India,” Chapter 16 in Deaton and Kozel (2004).
Deaton, Angus , and Margaret Grosh. “Consumption,.” in *Designing Household Questionnaires for Developing Countries: Lessons from Fifteen Years of the Living Standard Measurement Study*, edited by Margaret Grosh and Paul Glewwe, 91-133. Washington, DC: World Bank, 2000.
Garner, Thesia I., George Janini, William Passero, Laura Paszkiewicz, and Mark Vendemia, "The Consumer Expenditure Survey in Comparison: Focus on Personal Consumption Expenditures." Washington, DC. Bureau of Labor Statistics, processed., 2003.
Government of India. *Final Report of the National Statistical Commission*. New Delhi, 2000.
———. *Report of the Expert Group on Estimation of Proportion and Number of Poor*. New Delhi: Planning Commission, 1993.
Himanshu and Abhijit Sen, 2004, “Poverty and inequality in India: getting closer to the truth,” *Economic and Political Weekly*, forthcoming.
Kijima, Yoko and Peter Lanjouw, 2003, “Poverty in India during the 1990s: a regional perspective,”Washington, DC. World Bank Research Working Paper No. 3141, Oct.
Kozel, Valerie, and Barbara Parker, 2003, “A diagnostic profile of poverty in Uttar Pradesh,” *Economic and Political Weekly*, 37(4), Jan. 25, 385–403.
Lal, Deepak, Rakesh Mohan, and I. Natarajan, 2001, “Economic reform and poverty alleviation: a tale of two surveys,”*Economic and Political Weekly*, March 24, 1017–28.
Kulshrestha, A. C. and Aloke Kar, 2004, “Consumer expenditure from the National Accounts and National Sample Survey,” in Deaton and Kozel (2004).
Mahalanobis, P. C., and S. B. Sen. "On Some Aspects of the Indian National Sample Survey." *Bulletin of the International Statistical Institute* 34 (1954).
Martin, Margaret E., Miron E. Straf, and Constance F. Citro. *Principles and Practices for a Federal Statistical Agency (Second Edition)*. Washington, DC: National Research Council,

2002.
Meenakshi, J. V., and Brinda Vishwanathan, 2003, “Calorie deprivation in rural India, 1983–1999/2000,” *Economic and Political Weekly*, 37(4), Jan 25, 369–375.
Minhas, B. S., 1988, “Validation of large-scale sample survey data: case of NSS household consumption expenditure,” *Sankhya, Series B* 50, Supplement, 1-63.
Mistiaen, Johan A., and Martin Ravallion, "Survey Compliance and the Distribution of Income." Washington, DC. The World Bank, processed., 2003.
Mukherjee, M., and G. S. Chatterjhee, 1974, “On the Validity of NSS Estimates of Consumption Expenditure” in Bardhan and Srinivasan (1974).
Nurghai, Rinku, M. H. Suryanarayana, and Salman Zaidi, 2003, “Measuring poverty in Karnataka: the regional dimension,” *Economic and Political Weekly*, 37(4), Jan 25, 404–408.
NSSO Expert Group on Sampling Errors, 2003, “Suitability of different reference periods for measuring household consumption: results of a pilot study." *Economic and Political Weekly*, 37(4),.Jan 25, 307–321.
Rudra, Ashok, 1974, “Minimum levels of living: a statistical examination,” in Bardhan and Srinivasan (1974).
Sala-i-Martin, Xavier, 2002, “The disturbing ‘rise’ in global income inequality,” Cambridge, MA. National Bureau of Economic Research Working Paper No. 8904.
Sen, Abhijit, 2000, “Estimates of consumer expenditure and its distribution: statistical priorities after the 55th Round,”.*Economic and Political Weekly* 35 (51), Dec 16, 4499-518.
Srinivasan, T. N., P. N. Radhakrishnan, and A. Vaidyanathan, 1974, “On the distribution of expenditures in India: An evaluation” in Srinivasan and Bardhan (1974).
Sundaram, K., and Suresh Tendulkar, 2003a, “Poverty *has* declined in the 1990s: a resolution of the comparability problems in NSS consumer expenditure data,” *Economic and Political Weekly*, Jan 25, 327–337.
Sundaram, K., and Suresh Tendulkar, 2003b “NAS-NSS estimates of private consumption for poverty estimation: a further comparative examination,” *Economic and Political Weekly*, Jan. 25, 376–384.
Sundaram, K., and Suresh Tendulkar, 2003c, “Poverty in India in the 1990s: an analysis of changes in 15 major states,” *Economic and Political Weekly*, Apr. 5, 1385–1393.
Sundaram, K, and Suresh Tendulkar, 2003d, “Poverty in India in the 1990s: revised results for All-India and 15 major states for 1993–94,” *Economic and Political Weekly*, Nov 15, 4865–4872.
Sundaram, K, and Suresh Tendulkar, 2003e, “Poverty among social and economic groups in India in 1990s,” *Economic and Political Weekly*, Dec 13., 5263–5276.
Tarozzi, Alessandro, "Calculating Comparable Statistics from Incomparable Surveys, with an Application to Poverty in India." Duke University, Department of Economics, processed, March., 2003.
Triplett, Jack E. "Measuring Consumption: The Post-1973 Slowdown and the Research Issues." *Federal Reserve Bank of St Louis Review*, no. May/June (1997): 9-42.