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Data and Dogma: The Great Indian Poverty Debate

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What happened to poverty in India in the 1990s has been fiercely debated, both politically and statistically. The debate has run parallel to the wider debate about globalization and poverty in the 1990s and is also an important part of that debate. The economic reforms of the early 1990s in India were followed by rates of economic growth that were high by historical standards. The effects on poverty remain controversial, however. 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 in 1999/ 2000, have been challenged for showing both too little and too much poverty reduction. The various claims have often been frankly political, but there are also many important statistical issues. The debate, reviewed in this article, 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 poverty in India in the 1990s, there is good evidence both that poverty fell and that the official estimates of poverty reduction are too optimistic, particularly for rural India. The issues covered in this article, although concerned with the measurement of poverty in India, have wide international relevance—discrepancies between surveys and national accounts, the effects of questionnaire design, reporting periods, survey nonresponse, repair of imperfect data, choice of poverty lines, and interplay between statistics and politics.

Hundreds of millions of Indians are poor by national and international standards. Indian 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 gross domestic product (GDP) grew rapidly by historical standards, and many commentators have associated this acceleration with the economic reforms 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. India accounts for about 20 percent of the global count of those living on less than $1 a day, so what happens in India is not only a reflection of the worldwide trend but one of its major determinants.

A Brief Introduction to the Indian Poverty Monitoring System

India's official poverty estimates are based on the regular consumer expenditure surveys conducted by the National Sample Survey Organization (NSSO). These surveys, pioneered by P. C. Mahalanobis in the 1940s and 1950s (Mahalanobis and Sen 1954), were the world's first system of household surveys to apply the principles of random sampling established in the 1920s and 1930s.

The NSSO conducts large consumer expenditure surveys every five or six years and smaller surveys annually. In recent years even small surveys have collected some data on consumer expenditures. But to calculate official poverty statistics, the Planning Commission uses only the larger surveys that focus on consumer expenditures on the grounds that the larger surveys are required to estimate poverty accurately for each state because those estimates are the basis for various transfers from the central government to the states. In recent years large surveys were conducted in 1983 (Round 38), 1987/88 (Round 43), 1993/94 (Round 50), and most recently, 1999/2000 (Round 55). (The next, Round 61, for 2004–2005, was in the field at the time of writing.) For each of these surveys the Planning Commission has published estimates of the number and proportion of people in poverty, by state and sector.

These official poverty estimates count the number of people living in households with monthly per capita total expenditure below a poverty line specific to the state and sector (urban or rural). The original official state-specific poverty lines, based on state differences in price levels, come from the report of an expert group (India, Expert Group on Estimation of Proportion and Number of Poor 1993), which derived them from earlier academic studies that had compared prices across states. The poverty lines are updated periodically using a system of state-by-state price indexes, which are estimated separately for rural households (the consumer price index for agricultural laborers) and urban households (the consumer price index for industrial workers). There are no predetermined all-India urban and rural poverty lines. Instead, rural and urban poverty counts for each state are aggregated to get urban and rural totals. All-India urban and rural poverty lines are then set so that when they are applied to all urban or rural households without differentiation by state, the total number of urban and rural poor people matches the sum of the state counts. The poverty data from 1983 onward, which are based on these procedures, are the subject of the current debate.

Conflicts between National Accounts and Sample Surveys

Estimates of mean consumption are generated by both the national accounts statistics and the NSSO from their regular surveys of consumer expenditures. The two sets of estimates can be used to cross-check and validate one another, both for total consumer expenditure and for individual commodities or groups of commodities, such as grains, clothing, or services. There is a long tradition of such comparative work in India. More controversially, before the 1990s, the Planning Commission used the national accounts estimate of consumption as a "control" total for the surveys in estimating poverty. Thus, for example, if the ratio of the national accounts estimate to the survey estimate of mean consumption was greater than one, the Planning Commission would multiply the total expenditure of each household by that ratio before calculating the number of people living in households below the poverty line.

This procedure is practiced in many countries, particularly in Latin America, where income estimates from surveys are typically much smaller than those from national accounts. India's abandonment of such scaling up (for reasons discussed later) has been the subject of considerable controversy. 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. Bhalla (2002), like Sala-i-Martin (2002), uses a variant of the method to estimate global poverty (see Deaton 2005 for a discussion). Opponents of such procedures question the automatic assumption of superiority for the national accounts data and note that scaling up the surveys bypasses a central question in the debate: does the growth measured in the national accounts show up in improvements in the living standards of the poor?

In India during the 1990s the national accounts estimates of mean consumption grew much more rapidly than did the survey estimates. Scaling up would thus have shown a more rapid reduction in poverty in the 1990s than shown by the survey-based poverty estimates. In consequence, those who believe that the economic growth following the reforms has been associated with large-scale poverty reduction have tended to argue that the national accounts are right and the surveys wrong. Antireformers and skeptics have argued for the surveys.

Early comparisons of surveys and national accounts in India were carried out by Mukherjee and Chatterjhee (1974) and by Srinivasan and others (1974). Using survey and national accounts data from the 1950s and 1960s, they examined the match between the two estimates of total consumption and its distribution. For the decade up to 1963/64 Mukherjee and Chatterjee write that agreement between the revised series for national accounts consumption and the survey estimates is close, although they note that the survey estimates are systematically and (on average) increasingly below the national accounts 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 do the national accounts. Srinivasan and colleagues' analysis is broadly consistent with that of Mukherjee and Chatterjee, though they find that the surveys are lower than the national accounts estimates from an earlier date. They also note that the distribution of consumption is broadly similar in the two sources.

If the early comparisons of national accounts and survey estimates of mean consumption were relatively reassuring, more recent comparisons are anything but. The gap between the two estimates has continued to widen, reaching levels that would have been viewed with horror by the early researchers. Depending on the set of adjustments made, the survey estimate of consumption is currently around two-thirds of the national accounts estimate and has been falling steadily since the late 1960s, by 5–10 percentage points a decade.

This differential rate of growth in consumption estimates is far from unique to India. There appears to be a similar discrepancy between survey and national accounts estimates of the growth rate of consumption for the world as a whole (Deaton 2005). 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 (Triplett 1997; Garner and others 2003). Although there are almost certainly errors in both sets of estimates, the view of what is happening to poverty 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 in survey practice, the automatic reaction is to trust the national accounts over the surveys. That there is little basis for such a judgment was carefully argued by Minhas (1988) in an article that should be read by anyone concerned with the issue of the differences between national accounts and survey data. Minhas's work was central to the Planning Commission's decision to abandon its 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 consumption in the surveys and the national accounts, differences in timing, and the heavy reliance in national accounting practice on various "rates and ratios" that link observable but irrelevant quantities to the unobservable but relevant ones. These ratios are derived from surveys (for example, surveys that link earnings in services sectors to value added in the sectors), but they are frequently many years—often decades—out of date.

The use of outdated rates and ratios in a growing economy experiencing structural development will typically lead to systematic trend errors in the accounts. Consider the netting out of intermediate production from value added, which is frequently done using a fixed ratio. Because the degree of intermediation tends to grow as the economy becomes more complex and more monetized, 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 for India. The

national accounts estimate consumption of vanaspati as total production less imports plus exports, less consumption by government or business. In an economy in which all vanaspati is used for household cooking, this gives the right answer. But as the economy grows, consumers eat more meals out, so that an increasing fraction of vanaspati is used by commercial food suppliers, restaurants, hotels, and street vendors. Consumer spending on these services is derived from (fairly shaky) data on the gross output of the services sector, adjusted to a value-added basis by deducting the 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 if 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 overstate the rate of growth of consumption and GDP and to increase the ratio of national accounts to survey consumption (see Kulshrestha and Kar 2005).

Minhas (1988, p. 14) 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." He warns against adjustments that assume that only the survey estimates are at fault:

> It is indeed hazardous to carry out pro-rata adjustment in the observed size distribution of consumer expenditure in a particular NSS [National Sample Survey] round by multiplying it with a scalar derived from the ratio between the NAS [National Accounts Statistics] 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. (p. 37)

With consumption growing more than 1 percent a year faster in the national accounts estimates than in the survey estimates, application of the pro rata adjustment 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.

Kulshrestha and Kar (2005) and Sundaram and Tendulkar (2003b) provide a more contemporary perspective on the discrepancy. Kulshrestha and Kar were primarily responsible for the production of the national accounts in the 1990s, so their views on the accuracy of the consumption estimates carry considerable 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 is larger and increasing faster for nonfood items than for food.

These authors explore food items in more detail because there is often enough additional information to make an informed judgment about the likely balance of accuracy. With a few exceptions, their findings are similar to those of Minhas (who comes at the issue from the survey side): when there is a discrepancy, the national accounts estimates are typically less plausible and more likely to be in error. They determine that the discrepancy for food and tobacco 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 the 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 nothing in their findings would make the survey data on household consumption expenditure "unfit for measurement of poverty incidence" (p. 117). Although this assessment may be too sanguine—nonfood items also play a role in poverty measurement—this work establishes that there can be no automatic presumption in favor of the national accounts.

Sundaram and Tendulkar (2003b) report on the findings of a joint Central Statistical Organization–NSSO exercise on cross-validation of the two sets of estimates. They draw attention to the fluidity of the national accounts estimates, noting that revisions for some categories are often so large as to cast doubt on the estimates in general. This is closely related to Minhas's concern with the outdated rates and ratios. When the Central Statistical Office abandons a long-used ratio and new survey or other information is collected, information based on actual data paints a very different picture from that based on the long-used approximation. Such revisions, though welcome, do little to improve the large number of items still held hostage to the inaccuracy 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 national accounts statistics, which derive consumption as a residual at the end of a long chain of calculations.

Sundaram and Tendulkar also draw attention to items included in the national accounts estimates but not in the surveys, such as the imputed rents of owner-occupiers and expenditures by nonprofit institutions serving households. Like Kulshrestha and Kar, they demonstrate the increasing importance of a relatively new item, financial intermediation services indirectly measured (FISIM), introduced in accord with the recommendations of the 1993 revision of the UN System of National Accounts. FISIM—the difference between interest paid to and interest paid by banks and other financial intermediaries—has been added to national accounts estimates of household consumption since 1993, with some backdating to the 1980s. 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 rates, 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. 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, accounting for a quarter of a percentage point per year of the difference in annual growth rates in consumption between national accounts and surveys. In measuring the living standards of the poor, it can reasonably be doubted whether any of the value of financial intermediation is relevant.

Survey Methodology: Reporting Periods

The design of the Indian surveys has evolved over time and is continually under discussion. An important design issue for poverty measurement is the length of the reporting period. The NSSO had adopted a uniform 30-day recall period, based on experiments carried out by Mahalanobis and Sen (1954) in the 1950s. The NSSO ran a new series of experiments in the "thin" survey Rounds 51 (1994/95) through 54 (1998), which are run between the quinquennial rounds, randomly assigning households to one of two questionnaires with different reporting periods. A questionnaire with a 7-day reporting period 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 half of those derived from the 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 past 7 days rather than over the past 30 days. Higher 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. With 30-day reporting periods, most households report no purchase of low-frequency items, but in 365-day periods most households report at least some purchases. Thus despite 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 low-frequency items. Because the mean falls and the bottom tail increases, measured dispersion in these purchases is much reduced, and this carries through to total expenditure. This means that it is never legitimate to compare measured inequality across surveys with different reporting periods without some sort of correction.

The questionnaire experiments showed that reporting periods make a difference but did not settle the question of which is better. To find out, the NSSO launched another set of experiments in the Round 55 survey in 1999/2000, updating and extending the experiments of Mahalanobis and Sen (1954). Alternative question-naires were 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. According to initial results reported by the NSSO Expert Group on Non-Sampling Errors (2003), for rural households the 7-day estimates were on average 23 percent higher than the 30-day estimates, somewhat lower than the dis-crepancy in the large-scale NSSO thin-round surveys. But comparisons with the daily estimates show that the 30-day estimates were more accurate than the 7-day esti-mates for many important commodities, including cereals and cereal products. Over all the goods examined, there is no clear superiority of one reporting 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. It also does not support the idea that the discrepancy between the national accounts and sample survey measures of consumption is due largely to underestimation in the survey data associated with an overly long report-ing period.

How reporting periods affect estimates of consumption and poverty is relevant for surveys in 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 inter-views. Surveyors can also visit households several times, for example, to take account of seasonality in expenditures or to gather data a day or two at a time when it is believed that respondents cannot accurately recall expenditures over long refer-ence periods. There is considerable international experience with these issues (see, for example, Deaton and Grosh 2000), although there can be no presumption that a design that is good for one country will be good for another.

What Happened to Poverty in India in the 1990s?

At the end of the 1990s there had not been a large-scale consumer expenditure sur-vey in India since 1993/94, so there were no official estimates of national or state poverty rates after that date. The Planning Commission does not endorse poverty estimates based on the smaller consumer expenditure surveys that the NSSO runs between the quinquennial rounds, even though the sample sizes are large enough to support accurate poverty estimates at the national level. The last of these thin sur-veys before the Round 55 survey of 1999/2000 was a half-year survey in 1998.

Neither it nor the immediately preceding small surveys showed progress in reducing poverty (see, for example, Datt 1999). It is widely believed that there were problems with the sampling in the rounds from 1994 through 1998, but there has been no official confirmation of any such difficulty.

The Design of Round 55, 1999/2000

In planning for Round 55, the NSSO faced the immediate problem of deciding what design to use. The experimental questionnaires tried in Rounds 51 through 54 had showed more consumption and less poverty. The results of the expert group analysis of the experimental questionnaires were not yet 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.

After considerable internal debate, a compromise solution was adopted. For food, pan, and tobacco each household was asked to report all items over both a 7-day and a 30-day period. The traditional 30-day reporting period for durables, clothing, education, and institutional medical expenses was replaced by a 365-day period only.

Although this new, compound design might well be defensible on its own terms, it was incompatible with previous surveys. Consumption and poverty estimates based on it thus cannot reliably be used to assess trends or even changes from the previous large survey, Round 50. For high-frequency items, having both the 7-day and the 30-day reporting periods is likely to prompt respondents to reconcile the two reports. Thus, for example, the reported consumption of milk might be expected to be quite similar for the 30-day period, on the one hand, and for the 7-day period, on the other, once the 7-day results have been scaled up by a factor of 30 over 7, something that might not happen if the same respondent were asked to report over one period or the other but not both. Indeed, the means of total estimated consumption for the two reporting periods are much more similar in Round 55 than was the case in the experimental thin rounds, which randomly assigned each household to one or the other reporting period, not both. It remained unclear whether this meant that consumption reported with the 30-day recall was pulled up to meet the 7-day reports or whether consumption reported for the 7-day recall periods was cut back to accord more closely with the 30-day reports. Most likely, some combination of both was at work.

The presence of both questionnaires on the survey increased the interviewing time and forced a number of other changes in the survey. The employment and unemployment survey, usually given to the same households that answer the consumer expenditure schedules, was given to a separate sample of households in the Round 55 survey. But even within the consumer expenditure schedule, there were important changes, nearly all in the interests of compression and time saving.

Despite these difficulties, the 30-day responses were adopted as the basis of the new official poverty totals, although Planning Commission press releases also provided (lower) estimates of poverty using the 7-day reporting period results. 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. Estimated poverty fell from 37 percent to 27 percent among rural households and from 33 percent to 24 percent among urban households. All-India poverty fell 10 percentage points over the six-year period, from 36 percent 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. Estimated poverty was believed to be too low because reported consumption over the 30-day reporting period had been upwardly biased by the simultaneous presence of the 7-day reporting period. But no one knew how far off the official estimates were. One of the first researchers to see the difficulties with Round 55 was Abhijit Sen (2000). Even before the results were published, he delineated the contamination problems that were to dominate the interpretation of Round 55, which he refers to as a "failed experiment."

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 maintaining consistency in estimation on the other. Yet there have been few cases as dramatic or where the consequences of the change were so little anticipated in advance.

Making Adjustments

Various researchers have developed ways of adjusting the Round 55 expenditure data to correct the official estimates. These adjustments are based on assumptions that allow imputation of missing data or adjustments to contaminated data and, by the nature of the problem, at least some components of the assumptions are untestable. Assumptions differ, none is uncontroversial, and all have been debated. As will be discussed later, the official counts have also been challenged based on the poverty lines used and how they have been updated over time.

Deaton (2003a) and Tarozzi (2003) base their corrections on the fact that an important section of the questionnaire was unchanged between Rounds 50 and 55 and can therefore be compared. If the probability of being poor conditional on expenditures on the consistently measured goods does not change from 1993/94 to 1999/2000, then that probability can be calibrated from Round 50 and used together with expenditure on the consistent items in Round 55 to estimate poverty in that round.

According to Deaton's and Tarozzi's calculations, most of the "official" decline in poverty is real. For rural households, for which official calculations show the poverty headcount ratio falling from 37.3 percent in 1993/94 to 27.0 percent in 1999/2000,

Deaton finds a fall from 37.3 percent to 30.2 percent, or 70 percent of the official estimate. In the urban sector, where the official headcount ratio is 23.6, his estimate is 24.7 percent, which reduces the decline from 8.8 percentage points to 7.5. Driving these results is the substantial increase in consumer expenditures on items that were consistently surveyed using the 30-day reporting period and the difficulty of explaining the increase without a substantial increase in total expenditure and thus in the fraction of the population that is poor.

Sundaram and Tendulkar (2003a, c, d) argue that the 30-day responses were not much contaminated by the presence of the 7-day responses, based on an examination of the results of an abbreviated consumption questionnaire given to the employment and unemployment subsample of Round 55. If that is the case, the only source of inconsistency between Round 50 and Round 55 questionnaires is the treatment of the low-frequency items (clothing, durables, educational expenses, and institutional medical expenditures), which were surveyed at 30-day reporting periods in Round 50 and at 365-day periods in Round 55. Because Round 50 solicited expenditures on these goods for both 30-day and 365-day reporting periods, however, reconstructing total expenditure for Round 50 using the 365-day data permits construction of a notionally consistent "mixed reference period" measure of per capita expenditure for both Rounds 50 and 55.

Sundaram and Tendulkar (2003d) estimate a decline in rural poverty from 34 percent in 1993/94 to 29 percent in 1999/2000, or about half the official estimate and less than Deaton's estimate, which was about 70 percent of the official estimate. For urban households, Sundaram and Tendulkar estimate a poverty decline from 26 percent in 1993/94 to 23 percent in 1999/2000, confirming only about a third of the official decline compared with Deaton's confirmation of 85 percent.

Sundaram and Tendulkar's estimates are not comparable with official estimates because of their use of the mixed reference period measure for Round 50 and because they use different poverty lines than the state- and sector-specific poverty lines of the Planning Commission. Rather than work with the Planning Commission's poverty lines, which have been called into doubt (as discussed later), Sundaram and Tendulkar use the all-India lines for 1973/74, updated only for the general rate of price inflation. Sundaram and Tendulkar (2003c, d, e) have also extended their results to the major states and have used the same corrections to investigate what happened to the poverty rates of different social and economic groups. In line with other work, particularly that of Deaton and Drèze, discussed shortly, they find that some groups have done much better than others. They conclude that although poverty reduction among some of the most vulnerable groups (scheduled castes, agricultural laborers, and urban casual laborers) has been in line with that of the general population, groups such as the scheduled tribes have been left behind.

Sen and Himanshu (2004) have recently challenged both Deaton's and Sundaram and Tendulkar's conclusions. With respect to Deaton, they apply the

"consistent goods" methodology to calculate the probability of being poor according to the mixed reference period definition of consumption used by Sundaram and Tendulkar. Given the validity of the mixed reference period correction, the incompatibility of Rounds 50 and 55 comes from the responses on food, pan, and tobacco, which were most likely biased upward in Round 55 by the presence of the seven-day questions. In consequence, applying Deaton's method with the mixed reference period adjustment should raise the poverty count over the unadjusted counts. But Sen and Himanshu show that exactly the reverse is true. Since it is implausible that the presence of the 7-day questions resulted in a decrease in the consumption responses in the 30-day reports, something had obviously gone wrong.

Although exactly what went wrong is not yet entirely clear, the most likely possibility is that Indian households have been shifting their consumption away from food toward an assortment of nonfood goods and services, not just by moving along the Engel curves (which would not be a problem for Deaton's method) but by shifting the Engel curves. As a result, the increase in consumption of 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. Sen and Himanshu produce new estimates using different, sensible, but largely ad hoc corrections. Their estimates are in line with Sen's (2000) original view that there was little decline in headcount poverty in India in the 1990s. Using comparable mixed reference periods for both rounds, they estimate that the rural headcount ratio fell by only 2.7 percentage points between the two rounds, 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. Although there is no way to know with certainty what the results of Round 55 would have been had the questionnaire been unchanged, this small a drop in poverty during the 1990s seems implausible. There is considerable evidence from sources other than the consumption surveys, such as information on wage rates, ownership of durable goods, and incomes from other surveys, which, though imperfect indicators on their own, taken together are extremely difficult to reconcile with an India in which poverty rates are not declining.

Other Estimates of Poverty

The poverty estimates by Deaton (2003a, b), Sundaram and Tendulkar (2003d), and Sen and Himanshu (2004) are all based on corrections to the Round 55 unit record data. Several other researchers have followed alternative approaches, linking poverty to external evidence. This is perhaps most explicit in the work by Datt and others (2003), who use an econometric model that links poverty rates to their

plausible determinants, including agricultural yields, nonfarm growth, development spending, and inflation. Using the estimated model to project poverty for 1999/2000 and ignoring the flawed data from Round 55, they find that the changes in the explanatory variables would have warranted a decrease in the headcount ratio from 39 percent in 1993/94 to 34 percent in 1999/2000. This suggests that the pace of poverty reduction was slightly lower in the 1990s than in the 1980s and lower than might be expected given India's high rate of economic growth in second half of the 1990s. The differences between official and predicted rates of progress are largely 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 and others' projections are similar to Sundaram and Tendulkar's (2003d) adjusted calculations but show a good deal less poverty reduction than Deaton's method does and a good deal more than Sen and Himanshu's.

Kijima and Lanjouw (2003) combine forecasting with limited use of Round 55 data. Their forecasts are based on household characteristics that are among the ultimate determinants of poverty. They use Round 50 data to regress household expenditure on household characteristics such as education of household members, land holding, household size, caste, and a set of district dummy variables. The fitted regression is then taken to the Round 55 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 estimates are close to those of Sen and Himanshu but show about half as much poverty reduction as those based on the Deaton method.

In assuming that returns to factors cannot change, Kijima and Lanjouw's method is inherently conservative. The major objection to these calculations is that if poverty declined substantially over the 1990s and if, as might be expected (and certainly hoped), the reductions were driven by increases in the rate of return to factors such as labor, education, and land, then the calculations would not pick them up. Those who argue that the reforms have reduced poverty in India do not claim that the reforms have augmented India's supply 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 in global markets previously closed to them. The Kijima and Lanjouw poverty calculations rule out any such effects by construction and so cannot address whether they exist.

In another set of poverty estimates, Bery and Shukla (2003) update a similar exercise by Lal and others (2001). These studies use information collected from the Market Information Surveys of Households (MISH), large-scale annual surveys of household expenditures on consumer durables and other consumption items run by the National Council on Applied Economic Research. Because the design, sample size, and 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 includes a question on total household income, and these data are used to estimate poverty. This analysis suggests that poverty has fallen sharply in India, whether estimated from the income data or from the data showing increased ownership of durable goods.

A major concern with the MISH surveys is the adequacy of a single income question: "What is your annual household income from all sources?" Household income is a difficult concept to explain and to measure, especially for rural households, many of whose members are self-employed in agriculture. For these households, calculating incomes requires a great deal of imputation, as well as a careful separation of business and personal expenses. For these reasons most creators and users of household surveys do 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.

Surjit Bhalla has been one of the most consistent advocates of the position that poverty fell rapidly during the 1990s, not only in India but worldwide. Using national accounts data to adjust the survey data, Bhalla (2003, p. 338) argues that there was a sharp decline in poverty in India in the 1990s and that the official estimate of 24 percent in 1999/2000 is too low: "It is almost incontrovertible that poverty in India was less than 15 percent in 1999–2000." Bhalla's work defines the opposite pole to that of Sen and Himanshu (2004) in the Indian poverty debate.

The Choice of Poverty Lines

Although the recent debate on poverty in India has focused mainly on the measurement of expenditures, poverty lines are equally important. How they are updated and adjusted across regions or urban and rural households has a major effect on poverty estimates. In India, as in many other countries, a base poverty line is adjusted across time and space using price indexes, so the selection and construction of these indexes become a key input into poverty measurement.

The history of poverty lines in India is a case study in the interaction of science and politics, with political decisions often claiming a scientific basis, sometimes with justification, more often without. Although poverty lines are often linked to the amount of money needed for a minimally adequate diet, the use and long-term survival of poverty lines depend on policymakers and others accepting them as useful. For example, Rudra (1974), in discussing the history of Indian poverty lines up to that time and the persistence of the "magic number" of 20 rupees per head in 1960/61 prices 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 but because, once established as useful in economic and political discussions, poverty lines are resistant to change.

From the late 1970s to the mid-1990s the Planning Commission used only two poverty lines for per capita household expenditure, 49 rupees for rural households and 57 rupees for urban households at 1973/74 prices, which was close to the 15 percent urban price differential estimated by Bhattacharya and Chatterjee (1971) using unit value data from the National Sample Survey. 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 ignored interstate differences in price levels and in urban to rural price differentials. Furthermore, the national accounts consumption deflator is probably not the best measure of inflation for households near the poverty line. These problems and several others were dealt with by an expert group in 1993 (India, Expert Group on Estimation of Proportion and Number of Poor 1993). Their recommendations for new poverty lines were adopted (in somewhat modified form) by the Planning Commission, and these poverty lines have been used in official calculations since 1983 (back-casting the methodology), as described in the first section.

The expert group poverty lines have a serious flaw, however: the urban to rural price differentials that they imply are too large to be credible. It is unclear how this happened, whether because of an error in calculation or because the price indexes used in the calculations produced the result through some unexpected cumulative effect. 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 Round 50 (1993/94) it is more than 1.7 in Andhra Pradesh and nearly as high in Maharashtra, Madhya Pradesh, and Karnataka, but less than unity in Assam. As a result, official headcount measures of poverty are higher in urban than in rural areas in some states, and the all-India headcount ratios differ little for urban and rural areas. In Andhra Pradesh, which is the most dramatic example, the 1999/2000 official estimates give a poverty rate of 27.2 percent for urban areas and only 10.8 percent for rural areas. In addition, it is unclear whether there is any good basis for the differences 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 inflation rate used in the state-level price indexes. Errors in the indexes will induce errors in the trend rate of poverty reduction. These indexes are reweighted infrequently. For example, until 1995 the consumer price index for agricultural laborers used weights based on a 1960/61 survey. And although this index and the index for industrial workers 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 National Sample Survey consumption surveys. Because respondents report both expenditures and quantities for most foods in the survey as well as for tobacco, alcohol, and fuels, it is possible to calculate unit values for each good for each household and to use these unit values to form price indexes for urban and rural areas, states, and different rounds of the survey. The details of this work were originally laid out in Deaton and Tarozzi (2005) and were brought up to date by Deaton (2003b), who presents a set of poverty lines based on the indexes. These price indexes are quite different from the indexes implicit in the official poverty lines and much closer to the uniform 16 percent urban to rural price differential originally calculated by Bhattacharya and Chatterjee (1971) using similar procedures. As a result, headcount ratios calculated using Deaton and Tarozzi's lines show much higher rates of rural poverty relative to urban poverty than do the traditional lines. Because their price indexes also rise somewhat less rapidly than do the official indexes, the associated poverty rates for the country as a whole decline somewhat more rapidly than do the official rates, at least until Round 55 in 1999/2000, when contamination becomes an issue.

Deaton and Tarozzi's poverty lines and their procedures for correcting the Round 55 data form the basis for an analysis of poverty and inequality by Deaton and Drèze (2002). Their estimates show a fairly steady rate of poverty decline in India in the 1980s and 1990s. Deaton and Drèze argue that their estimates are broadly consistent with a range of other evidence, including growth rates from state GDP accounts and growth in agricultural wages. So even if their estimates for 1999/2000 are overoptimistic, as Sen and Himanshu (2004) argue, their general assessment of trends is unlikely to be wildly incorrect.

Deaton and Drèze show that the new poverty lines sometimes make a dramatic difference to the estimates, particularly for urban and rural comparisons. For the previously mentioned example of Andhra Pradesh, they estimate that 10.5 percent of the urban population were poor in 1999/2000 and 26.2 percent of the rural population, compared with the official estimate of 27.2 percent urban poverty and 10.5 percent rural poverty. They also note that there was a marked increase in consumption inequality in the late 1990s between states, with the 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 more rapid in urban than in rural areas; and within the urban sectors of many states, where consumption has been growing more rapidly among the wealthiest.

The questionnaire changes in Round 55 have obscured the trends in inequality. The substitution of a 365-day reporting period for a 30-day period for 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 than with a 30-day reporting period. This effect carries through to per capita total household expenditure, artificially compressing

its distribution in Round 55 compared with Round 50. This questionnaire-driven reduction in measured inequality obscures the underlying increase in inequality, which cannot be detected if the old and new questionnaires are compared without explicit correction.

A different perspective on inequality is provided by Banerjee and Piketty (2005). Their data come from 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 did extremely well during the boom of the 1990s. Banerjee and Piketty calculate that average incomes increased by 70 percent in real terms for the top 1 percent and tripled for the top 0.01 percent. This is the first evidence on the extent of increasing inequality attributable to the rising income share of the wealthiest Indians. Their findings are likely to trigger further analysis and debate. Their work is also relevant for the debate over the use of national accounts and surveys. If inequality is increasing, and if people with higher incomes are less likely than others to respond to surveys, then the ratio of measured consumption in the surveys to true consumption will fall. Indeed, Banerjee and Piketty show that around a quarter of the increase in the gap between national accounts and survey estimates can plausibly be attributed to the increase in inequality because of the rising income share of high-income Indians.

Updates and Lessons

Despite an extensive body of empirical work by eminent researchers, the debate on what happened to poverty in India 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 four thin rounds since the large survey, Rounds 56, 57, 58 (a half-year survey), and 59. But a preliminary examination suggests as much confusion as clarification. Round 56, conducted in 2000/01, shows a further reduction in poverty, very much along the trend line calculated by Deaton and Drèze (2002). The Round 57 data for 2001–2002 show a sharp increase in poverty over the previous year, which appears to have been even more sharply reversed in the last six months of 2001–2002, according to Round 58. The Round 59 poverty numbers are close to the level in Round 58. It is unclear why there should be such a pattern, although the disposal of large public cereal stocks in 2000–2001 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 from 1994 to 1998, the thin round surveys suffer from some little understood sampling problem.

It is impossible for a country to pursue a coherent antipoverty strategy without an adequate poverty monitoring system. The statistical problems and debates discussed here concern a country with one of the best developed survey capacities in the world. The NSSO statisticians are highly skilled, and field staff are disciplined and well trained. Matters are almost certainly worse elsewhere.

Several conclusions and lessons can be drawn from the great Indian poverty debate, for India and for other countries. Most lessons are especially relevant for statistical offices, but several are important for policymakers and researchers.

Lessons for Statistical Offices

India's experience demonstrates the importance of annual consumption or income surveys. With major poverty surveys occurring only every five or six years, the failure of even one survey means that there is no reliable estimate of the rate of poverty decline for more than a decade. India switched from large annual surveys to less frequent surveys because it was thought that poverty changed too slowly to make annual measurement necessary. But the reduced frequency does not make adequate allowance for variability in conditions and survey structure. In largely agricultural societies, where harvests vary from year to year, a 5-year cycle runs the risk of having surveys coincide with unusual harvests, so that it can take 10 or 15 years to establish a trend. Or, as with Round 55 in India, if a particular survey is compromised, and if the survey period coincides with important policy changes, there can be a long gap between a policy change and measurement of the effect on poverty. In India there will be no poverty measures that are comparable to the 1993/94 estimates until the 2004/05 survey results are available in early 2006.

Good annual surveys, even if not comprehensive, are an insurance against these problems. In India the thin rounds could serve this function in principle, but there have been persistent doubts about their validity based 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 rounds.

In balancing consistent survey design with changes to reflect economic development, new areas of interest, and improved survey technologies, the key point is that survey design should change slowly relative to the frequency of poverty monitoring. Revisions once every five years may be acceptable if poverty surveys are run every year, but catastrophic if poverty monitoring is done at five-year intervals. For measuring changes in poverty, a single survey is of little value without an earlier, comparable survey. When major design changes (such as changes in reporting periods) are implemented, it is desirable to randomly split the survey between the old and new design, to provide a basis for both forward and backward comparability.

The Indian NSSO has had a long history of experimentation in using what are essentially randomized controlled trials to assess the consequences of changes in

survey practice. That such experimentation precipitated the failure of the major 1999/2000 round should not be taken as a condemnation of experimentation. Rather, the fault lies with an oversight mechanism that failed to stop the introduction of what was effectively a completely new and untested design into an important and long-awaited survey. Statistical offices are often reluctant to undertake costly experiments that have no immediate input into published statistics. But without experimentation there can be no satisfactory progress in survey design. If properly used, experimentation can help prevent bad designs from being adopted in regular surveys.

Another general issue concerns the relationship between the national accounts and the surveys. These are usually the responsibility of different government agencies, which complicates collaboration for resolving discrepancies and sharing data to improve performance. Placing both functions within an overall national statistical office or otherwise making collaboration easier is likely to improve the performance of both. India's experience shows that scaling up survey data to match the national accounts is not a simple shortcut to better poverty estimates. The two measures of consumption are not the same, and there is no straightforward way to adjust one to the other. India's experience also demonstrates that when surveys and national accounts disagree, it should never be assumed that one or the other is correct. In many countries growth as measured in the national accounts appears to be greater than growth as measured through surveys of income and consumption. That makes it difficult to use the national accounts to cross-check survey data or to forecast poverty trends in the absence of good survey data. Once again, there is no shortcut to poverty monitoring without a sound, regular survey.

Finally, India's experience shows how important it is for the survey agency to develop a dialogue 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 is a major change from policy in the 1980s, when the raw data were not released to outside researchers. That said, there still appears to be no effective mechanism through which users can bring questions and suggestions to those who process and design the surveys.

Lessons for Policymakers

Policymakers are accountable to the whole population, not just to the poor. But poverty reduction is a key aim of policy in India and in most other poor countries. Thus policymakers are accountable to the public for poverty reduction, and progress cannot be monitored without an adequate statistical agency. Policymakers need to establish and fund such an agency and ensure adequate financing for annual surveys of consumption or income. Policymakers 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. Without domestic openness and debate, policymakers are unlikely to be held accountable for their policies. Although many countries do not have the domestic statistical or analytical capacity to generate broad debate, as in India, 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 in data collection or publication. Yet politics in the broad sense played a role. In evaluating the reforms, the political right had an interest in showing low poverty, and the political left in showing high poverty, and this undoubtedly intensified the debate on survey design and led to the unfortunate compromise design that temporarily undermined the poverty monitoring system.

This politicization of data collection and interpretation is often bemoaned. Yet political accountability is essential to poverty 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 (see Martin and others 2002), but poverty monitoring systems are ultimately part of the political process. Most countries can only envy India its statistical capacity and the central part that poverty and poverty measurement play in Indian public life.

Lessons for Researchers

Mistakes are inevitable, and survey data can be compromised by internal and external factors. Thus poverty assessments will often have to be made using imperfectly comparable surveys. India's experience illustrates the possibility of repairs to enhance the credibility of estimates. But that experience also demonstrates that repairs, however creative, are a poor substitute for the collection of clean, credible, and comprehensive data. What are convincing assumptions to one person can be unconvincing to another, and political positions inevitably influence the assumptions that people are prepared to make or accept.

Nevertheless, the Indian debate shows that discussion and advance are possible, even among those with very different preconceptions, and that the balance of opinion can be changed by well-reasoned and transparent arguments. Some of the arguments and methods used in the Indian debate were country specific (for example, the use of the supplementary consumption questions in the employment and unemployment survey), but some are more broadly applicable for dealing with partially contaminated data. This is an area in which there are likely to be returns to good, technical statistical work.

Another lesson is the importance of high-quality domestic researchers. Analysis by outside researchers can often be helpful, but it has a quite different effect from a

domestic debate among local researchers, policymakers, and the press. India is still exceptional in having a large group of domestic researchers who can work with basic data, but this is a relatively new phenomenon even in India. The Indian press has also played a distinguished role in the debate. Daily newspapers regularly report on new findings on poverty and regularly carry related op-ed pieces. A unique contribution to the poverty debate was made by *Economic and Political Weekly*, in whose pages much of the Indian debate took place. It is a cross between an academic journal and a popular magazine like *The Economist*. Published weekly, it provides rapid dissemination of viewpoints and research findings and links researchers, the press, and policymakers. Other countries, including many rich countries, would do well to emulate its approach.

Notes

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