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Data for monitoring the poverty MDG

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0. Introduction

I consider two of the most important unresolved issues in monitoring world poverty for the Millennium Development Goals. Section 1 is concerned with the selection of poverty lines, whether they should be national or international, and if the latter, how the line should be converted between local currencies. It concludes with a proposal for modifying current practice. Section 2 is concerned with the data sources, and with whether monitoring should be done using national accounts data, with appropriate allowance for distribution, or from household surveys. I vigorously argue in favor of household surveys, and again end with a series of proposals for survey work. Section 3 summarizes some current priorities.

1. Drawing the line: poverty lines for monitoring the MDG

1.1 National or international lines?

The World Bank's world poverty counts use an international poverty lines of approximately \$1 and \$2 -a-day at 1995 international purchasing power parity (PPP) prices. The notion behind such lines is a simple one, that for the purpose of the world counts, or indeed for monitoring poverty reduction, we need a common international standard of some sort. While a common money standard is certainly not the only possibility, the rhetoric of a \$1-a-day standard has been enormously successful, particularly for the first-world and international agency audience to which it is primarily directed. Yet, as we shall see, the construction of such lines is a complex and difficult task, and the attraction of even the simple concept is apt to diminish once the details of its implementation are exposed to close inspection.

For most of its country work, the World Bank (like policymakers in each country) uses the *national* poverty lines that exist for many countries, although not all. The MDG target, which involves cutting poverty rates by half, could conceivably be applied to poverty measures based on either national or international poverty lines. So there is a choice.

There are strong arguments for an international line, if only because of the proven appeal of something like the \$1-a-day concept, even if the reality falls some way short of the rhetoric. To start with an area where there is a wide degree of agreement, no one would argue that the US poverty counts should be included in world counts nor in the MDGs. Of course, the counts do not apply to the rich countries, but the obvious interpretation of the exclusion is that the US poverty line is too high to be used in international comparisons, and that if a suitable international line were applied to the US, then there would be little or no poverty. Ravallion's survey of international poverty lines shows that the US phenomenon is quite general, Martin Ravallion (1994). Except among the poorest countries, poverty lines are higher in richer countries, and the evidence from surveys within rich countries shows that people's own notions of the poverty line rise as they get better-off. Which is one reason why some countries, such as Canada, have formalized the process by defining a poverty line in relation to the median of the income

distribution. These relative poverty lines clearly make sense for each country considered in isolation. But they hardly correspond to a universal notion of a state of extreme poverty and deprivation that is recognizable irrespective of average living standards in each country.

One possible argument is that the choice of line is not of much importance in practice. The poverty MDG is about poverty *reduction*, not about the poverty *level*, so that even if national poverty lines are not comparable, they do not prevent us from looking at poverty reduction in each country using its own individual poverty line. But this argument will not take us very far. Suppose, for example, that all countries have the same lognormal distribution of income, with the same degree of inequality, and differ only in the mean of log income. Then the amount of growth needed to halve the poverty rate is larger in a country with a higher poverty rate. For example, in a country where the poverty line is equal to median income, the poverty rate is 50 percent. To cut that to 25 percent, mean income has to increase by two-thirds of the standard deviation of log income; if the standard deviation is 0.5 (the Indian figure for consumption per head), we need income to increase by about a third. For a country that starts at 25 percent, cutting poverty by half requires that mean income increase by only a quarter, and if the country is fortunate enough to have a poverty rate of only one percent, cutting it by half needs only a 12.5 percent increase in mean income. Countries that are richer at the start, and have adjusted their poverty lines upward, have a stiffer task to meet the goal, but it is not a task that necessarily has much to do with eliminating the extreme poverty and deprivation with which we are concerned.

Which leads me to the position that *some* degree of international comparability is desirable, and that *some* kind of international poverty line is required. So we must either defend the \$1-a-day line, or come up with something better.

The \$1-a-day line has been criticized because it unrelated to any clear conception of international poverty, Sanjay Reddy and Thomas Pogge (2002). Their alternative is be to specify some minimum level of living in terms of "the resources necessary to achieve a salient set of elementary capabilities." Many national poverty lines, in both rich and poor countries, are constructed in a way that is consistent with such an idea. For example, both the Indian and US poverty lines were originally set with reference to the ability to buy a minimal food bundle or number of calories, see Chapter 3 of Angus Deaton (1997) for comparative discussions of those histories. However, a closer examination of these and other cases suggests that the link with food is more rhetorical than real. In both India an the US, the poverty line has been held constant in real terms, updated in nominal terms by a price index, and no attempt has been made to preserve the original link with food. And in both countries, updating according to the original methodology would generate poverty lines that are quite different from those currently in use. A more accurate interpretation of the history would be that the Indian and American poverty lines were originally accepted because they fell within the range of what was generally acceptable as a poverty line, a range that included, but was far from determined by, the incomes of those currently buying some minimally acceptable food bundle. These lines survived, not because of their link to any prescribed level of living, but because they have continued to fulfil and useful purpose for policymakers and in the policy dialog more broadly. Many other lines could

presumably have served the same function, at least for a time, and all lines are continually up for challenge by politicians, commentators, and expert panels. Proposals to reset the lines to a meaningful poverty standard are frequently advanced, but rarely carry the day, in part because the original justification of the lines tends not to be convincing on close inspection, but also because it is so difficult to come to agreement on what it costs to maintain a minimal standard of living. A committee of experts sitting in judgment on what poor people need to consume, or worse still, what they *ought* to consume, is a particularly objectionable mechanism for setting poverty lines. Instead, poverty lines need to match what people think it means to be poor in a particular society at a particular time, and the appropriate test of a suitable line is its usefulness and survival over time.

By these criteria, the \$1-a-day line has a good deal to commend it. On the one hand, it corresponds, at least approximately, to the national poverty lines of a number of the poorest countries. And this is surely the right place to look. Indeed, it is hard to think of a more appropriate definition for international poverty than being poor in the poorest nations, and we can rely on the political processes in these countries to throw up appropriate poverty lines. But just as important is that such lines are useful for the purposes to which they are put, in this case for monitoring by the international community of the number of very poor people in the world, and the \$1-a-day concept has proven itself by the extraordinarily extensive use to which it has been put in the decade since it was first constructed. Indeed, its use in the MDGs is itself a testimony to its value.

If we accept the \$1 or \$2 a day lines as a reasonable starting point, then we need a method for converting them into the local currency equivalents that are required for measuring poverty in each country.

1.2 Purchasing power parity price indexes

One thing that we cannot do is to convert a common international poverty line using market exchange rates. In poor countries, because labor is relatively cheap (because people are poor) the prices of non-tradeable goods, especially those with a high labor content, are low relative to the prices of tradeable goods. In consequence, the market exchange rate between the poor country and the US (for example), which is determined by trade, is unlikely to be an accurate measure of the cost of living difference between the countries. A dollar buys 50 rupees, but food that would cost \$10 in the US can be purchased for only 100 rupees, so that the implicit exchange rate is not 50, but 10. An Indian consumption level of 500 rupees a month would convert to only \$10 a month at the official exchange rate, but would be worth \$50 at the "food" exchange rate. In consequence, using the official exchange rate to convert consumption or income would vastly exaggerate the difference in levels of living between the two countries, and more generally between poor and rich countries, systematically overstating poverty in the former.

This account also suggests the remedy, which is to take a bundle of goods consumed by poor people, and "price it out" in each country. If the cost of the bundle of goods in India (say) is the

poverty line, the cost of the bundle in the other countries gives the appropriate value of the common poverty line in those countries. Alternatively, we can select a base country and calculate Laspeyres price indexes for all other countries relative to that base, so that if we have something that is approximately a \$1 per person per day poverty line for the base country, we can convert it into other currencies by multiplying by the price indexes. Such price indexes are examples of the purchasing power parity exchange rates that we need if we are to have a common international poverty standard.

The construction of Laspeyres indexes by "pricing out" a poverty bundle can be formally linked to methods that rely on calculating the cost of a minimal standard of living, or indeed of achieving a minimal set of capabilities. Such cost-of-living price indexes can be approximated by pricing out the goods purchased by people at the relevant level of living, and such approximations are routinely relied upon by statistical offices who see their consumer price indexes in cost-of-living terms. The accuracy of approximation can be improved by calculating various "superlative" index numbers, W. Erwin Diewert (1976), of which Fisher's ideal index is one, and these are often used in one form or another by the statisticians who construct purchasing power parity price indexes.

1.3 General problems with PPP exchange rates

Because PPP exchange rates are price index numbers, we can use the extensive body of theory about price indexes to explore their strengths and weaknesses. In particular, it is important to separate the *general* problems that are common to all price indexes, and which are therefore inherent in *any* attempt to convert a common international poverty standard into local currencies, from problems that are *specific* to current PPP exchange rates that are used for poverty measurement, and which might be ameliorated by better practice.

If poor people the world over consumed a single commodity—rice, say—the construction of PPP exchange rates for poor people's consumption would be a simple matter. All that we would need would be the price in local currency. This is the case where the price index works the best, which is when it is unnecessary, because we have a *price*, not a *price index*. In reality, poor people consume a range of goods, and their patterns of consumption are different in different parts of the world, and often even in different parts of the same country. The theory of price indexes handles this by pricing, not a bundle of goods, but the cost of attaining the same standard of living. For price indexes appropriate for international counts of extreme poverty, this would be the living standard of the poorest, and there is nothing that stops us from thinking of that living standard in terms of capabilities. But the exercise is clearly a difficult one. We are asked to consider the conceptual experiment of how many pesos it would cost a landless laborer from Bihar to live as well (or as poorly) in Mexico as she lives in India. Once stated thus, it is not at all clear that we should expect any satisfactory answer.

The standard economic theory of cost of living indexes is cast in terms of utility theory, with

price indexes defined by comparing the cost of attaining the same standard of living (utility) at different set of prices. When people consume only one good, such as rice, the standard of living maps directly into rice consumption, and cost of living indexes are just the relative costs of a unit of rice. When there are many goods, but the relative prices are the same in all countries, the cost of a bundle of goods in country A relative to its cost in country B will be the same no matter what bundle we choose, and in this case too, there is an straightforward way to measure price indexes. But the reality is that the relative prices of goods are quite different in different countries. Consumption patterns are also quite different, presumably in part because of international differences in incomes and relative prices, but also because of differences in tastes. When tastes differ, the theory offers no consistent yardstick with which to measure the cost of living, and there is no reason why the relative costs of living in India and Mexico should appear the same to Indians as they do to Mexicans. For an international subsistence poverty line, we can perhaps (although only perhaps) ignore taste differences, specifying a basic level of human functioning, and calculate the minimum cost of achieving it in different countries.

There are a wide variety of price indexes. The simple Laspeyres index outlined in section 1.2 is not always feasible—goods consumed by the poor in one country may not be available in another country—and when it is, it is unclear why one country rather than another should serve as the base. We could consider using each country in turn as base, but we will generally get as many different sets of PPPs as there are countries. These multiple indexes can then be "reconciled" by various kinds of averaging, sometimes leading to "better" index numbers according to some criteria. The important point here is the one noted long ago by Irving Fisher, that there does not exist any index number that satisfies all the "reasonable" requirements that might be placed on it. For example, there is no guarantee that the price in A relative to B is the reciprocal of the price of B relative to A, or that if we go from A to B, from B to C, and from C back to A that we will finish up where we started. One desirable property can be guaranteed by choosing an appropriate formula, but only at the price of losing another. Price indexes are not prices. The constructors of PPP exchange rates, like those who are responsible for national price indexes, know this, and make compromises. But they are always open to criticism that some "obvious" or "elementary" requirement has been violated. There is no way of constructing a PPP index number that is immune to all such criticism.

These problems exist even in the conceptually easier case of within-country comparisons. Until the early 1990s, India had two poverty lines, one for rural and one for urban households. Over the last decade however, there have been different poverty lines for each state, and for each sector within each state. The methodology under which these lines were constructed and are updated is explained in Government of India (1993)and appears to be sound in principle. Yet the results have brought official Indian poverty measurement into something close to disrepute. Urban poverty lines are so much higher than rural lines that, in many states, the urban poverty rate is higher than the rural rate, something that few independent observers would accept. In addition to these unsatisfactory intrastate price indexes, the pattern of prices across states also appears to make very little sense. There is an internal Indian debate on price indexes that parallels the international debate about unsatisfactory PPP exchange rates. And at least part of the reason lies in the inherent difficulty of constructing such indexes.

Angus Deaton and Alessandro Tarozzi (2000) report the results of a project to recalculate price indexes for Indian states and sectors for 1987-88 and 1993-94. The conceptual problems are immediately relevant in practice. If, for example, we wish to construct a price index for Kerala relative to Uttar Pradesh, we can start by calculating the bundle of consumption at or around the poverty line in Kerala, and then price it out in UP. But this is immediately problematic, because some of the goods that are heavily consumed in Kerala (for example, fresh fish, coconuts, and coconut oil) are rarely purchased in UP. The few recorded purchases are at very high prices, because these goods are exotica in UP, and cater only to a small minority. Conversely, atta is a basic staple in UP, but rarely shows up in Kerala. The cost of the Keralan poverty bundle in UP is therefore not a very good measure of the relative costs of being poor in UP relative to Kerala. If we start from UP, we have the same problems in reverse, and we get an answer for the cost of living in Kerala relative to UP that is not the reciprocal of the cost of living in UP relative to Kerala. One answer is to average the two; indeed, the geometric average is Fisher ideal price index. There is a lot to be said for the Fisher index in this context; it moderates the extremes that we get from either of the two Laspeyres indexes and, because it is a superlative index in the sense of Diewert (1976), it captures at least some of the change in consumption patterns that would take place if the Keralan actually moved to the UP, or vice versa. But the Fisher ideal index is subject to criticisms of its own, see for example Sanjay Reddy and Thomas Pogge (2002). Another partial solution that is sometimes proposed is geographical chaining. We can compare Kerala with UP by passing through a sequence of neighboring states, from Kerala to Andhra, to Maharashtra, Rajasthan, and eventually to UP, calculating an index only for pairs of nearby states with consumption patterns that are more similar than those at the beginning and end of our journey, and then chaining the results. Such chained indexes also have many desirable properties. But they can also be criticized on the grounds that such an index has the odd feature that the price index of UP relative to Kerala depends on what happens to prices and consumption patterns in Maharashtra, as state that is adjacent to neither.

There are no general solutions to the conceptual problems of constructing PPP price indexes. Those who construct them know this, and make compromises at least some of which are undesirable. There is no alternative.

1.4 Specific problems of the PPP indexes used in the world poverty counts

The PPPs used in the world poverty counts originally came from the Penn World Tables, but more recent versions have been constructed under the aegis of the World Bank. Constructing PPP index numbers is a major undertaking, requiring each participating country to supply information that is not part of its usual activities and, not all countries have done so. The number of these "benchmark" countries has increased over time, from 60 in the 1985 numbers to 110 in the 1993 version, including all of the largest countries. Non-benchmark countries have PPP exchange rates imputed to them through a regression procedure that effectively adjusts their official exchange

rate by an amount that is similar to the adjustment for countries at similar level of development for which benchmark data exist. This procedure, while clearly sensible, can also be subject to substantial error, and in some extreme cases in the past, the Bank has published PPPs and associated poverty rates that were clearly incorrect. There are also concerns that country statistical offices do not always take these tasks very seriously; the ICP data do not feed into domestic policymaking and they have no domestic constituency, so that they tend to be assigned low priority and funding.

The latest World Bank poverty counts, detailed in Shaohua Chen and Martin Ravallion (2001) show some very large changes compared with earlier counts, even when computed for the same country in the same year, and the changes are mostly attributable to the switch to new PPP numbers. Some of these changes almost certainly reflect a move from worse to better data—it is surely better to have 115 countries than 65—but some is probably due to "noise," essentially random measurement error. Certainly the new figures paint a different picture of world poverty than do the old ones, see Angus Deaton (2001). And although it is hard to predict what might happen with a new round of PPP numbers—and it is possible that they will be much more stable from now on—it is surely undesirable to measure world poverty with methods that are so unstable and unreliable.

There are also concerns about the way the PPPs are constructed. Price indexes are essentially the product of *weights* and of *prices* and there are questions about both.

The PPPs in the current poverty counts are *consumption* PPPs, rather than the earlier *national product* PPPs, and this change is clearly an improvement. However, there remain questions about whether these consumption PPPs are really relevant for the poor. The consumption bundles of the poor are not the same as the average consumption bundle, and price movements in the latter can be different from price movements in the former, for example if the relative price of food increases. Furthermore, national price indexes are also computed on a *plutocratic* basis, with weights that are proportional to *aggregate* expenditures, and not on a *democratic* basis, in which each household's own price index is averaged to get a national index. As a result, the national price index tends to be representative of relatively well-off households—Angus Deaton (1998) calculates that the US CPI is representative of a household at around the 75th percentile—further reinforcing the divergence between what we get—the national consumer price index—and what we want—a price index for the poor.

There is also a concern about world commodity prices. Some foodstuffs that are important to the poor, such as rice, wheat, or sugar, have active world markets, in which prices are (in)famously volatile. If the world price of rice rises relative to the world price of wheat, for example, the consumption of countries where people eat rice will become more valuable at international prices relative to the consumption of countries where people eat wheat. With a constant real PPP poverty line, the rice-eating consumers will become less poor relative to the wheat eating consumers. If markets work well, so that the world prices are actually the prices that poor people face, this is what we want to happen; after all, the rice consumers always have the opportunity to

switch to other commodities. But it is not clear that these world price fluctuations are indeed relevant for the majority of poor people in the world. Most rice in the world is eaten within a very short distance of where it is grown, and for many rice consumers, the world price might as well be the price on Mars. The severity of this problem depends on just how the ICP goes about collecting local prices, and the extent to which the prices used to construct the benchmarks are affected by fluctuations in world prices. The instability of actual PPPs from one revision to the next suggests that commodity price instability may be playing a role, but instability may also come from other sources. In any case, the ideal PPP would not only use weights that are appropriate for poor people, but would collect prices that are appropriate for them. Statistical offices rarely collect different prices for different people, but work on the premise that all consumers face the same prices.

1.5 What to do? A proposal

The PPPs currently used to covert the \$1-a-day lines into local currencies suffer from all the generic problems of price indexes. More specifically, there are questions about their relevance to poor people, and the quality of their construction. A constantly shifting, erratic, and doubtfully relevant yardstick seems like a poor instrument for monitoring the MDG.

Yet it is clear that we cannot do without PPPs, at least not without abandoning altogether the idea of a genuine international poverty line. In Deaton (2001), I proposed a procedure that combines the PPP method with the idea of national poverty lines. It works something like this:

- 1. start from the PPP lines from Chen and Ravallion
- 2. ask UNDP and World Bank offices in each country to check and debate these lines
- 3. modify the lines to correct serious errors, and in some cases, to bring them closer to domestic poverty lines
- 4. update the lines over time using domestic price indexes, without further reference to PPP exchange rates.

Step 1 recognizes the rhetorical value of the \$1-a-day concept, retaining the trademark. Although these lines are clearly not perfect, I doubt that they can be improved by better PPP numbers, except as under 2. It is always worth keeping in mind that poverty lines always have a degree of arbitrariness, which we should use to our advantage. The hope is that within that range of arbitrariness, we can find a poverty line for each country that is (a) reasonably close to \$1-a-day, and (b) is meaningful and politically acceptable to people in the country for the purposes of the international (not domestic) counts.

Step 2 recognizes that it is inappropriate for these lines to be exclusively set by "experts" in New York, Washington or Geneva. Poverty lines work because they seem reasonable to the people who use them. The \$1 a day poverty line should not be very far from national poverty lines in the poorest countries (as is in fact the case in India), although they will (and should) appear

ungenerous in better-off countries. At the minimum, the local consultations will check that the PPP starting point poverty line is not absurd in terms of local purchasing power and local expenditure patterns. Better still would be more extensive local consultations, always informed by the fact that the line is supposed to be an *international* line, identifying people who are poor by (say) Indian standards, not necessarily local standards.

Step 3 should be done centrally to ensure some sort of conformity across countries. The concern is that in better-off countries, people may wish to tie the poverty line to their local standard of living, with the result that we overestimate their poverty by the standards that are required for this work. If Step 2 works well, few alterations should be made at stage 3.

Step 4 means that the international lines, although different from the local lines, are updated in exactly the same way. The desirable feature of this scheme is that we avoid the possible effects of fluctuations in international commodity prices as well as possible errors in PPP revisions. The undesirable feature is that we may lose touch with whatever components of the PPPs capture real changes in the living standards of poor people in one country relative to another. Yet, such genuine changes should also show up in domestic prices, and will be captured by local updating. Because domestic price indexes are important statistics that are carefully scrutinized and debated by politicians and interest groups in each country, they are much more likely to be accurate than PPPs that have little domestic salience.

2. How to monitor poverty for the MDGs

2.1 Two Methods

There are **two possible methods** for calculating poverty and monitoring its trend over time and both have been widely employed. They give quite different answers.

- Use either *consumption* or *GDP* data from *national income and product accounts* (NIPA), together with *some measure(s) of distribution*, to infer poverty rates and their trends over time. The measures of distribution typically come from *household survey data*.
- Ignore the NIPA information, and use household survey data to estimate the fraction of people living in households whose income or consumption is below the income or poverty line for the corresponding household type.

The survey method is used by the World Bank for the dollar-a-day counts, though it makes some use of the NIPA method for projections, for example those in its *Global Economic Prospects*. The survey method is also used by many governments, as diverse as the United States, based on data from the March round of the monthly Current Population Surveys, and by India, based on data from the quinquennial large National Sample Surveys. The NIPA based method was once used in India, until the Planning Commission was heavily criticized for doing so—on grounds

similar to those reviewed below—by an Expert Group Report, Government of India (1993). Recently it has been used by Surjit S. Bhalla (2002), Xavier Sala-i-Martin (2002) and UNCTAD (2002) based on work by Massoud Karshenas (2001; (2002).

The methods give different answers. In the 1990s, according to the all-survey method, there was a good deal less poverty reduction than might have been expected from the rate of economic growth, in the world as a whole, and in some large countries, most notably India. Bhalla and Sala-i-Martin "resolve" the puzzle in favor of growth by using the NIPA method, which ensures that the growth in the NIPA statistics is incorporated into the poverty measures. According to these authors, the poverty MDG has already been met. UNCTAD uses the NIPA based measure to argue, not so much that the trend of poverty reduction has been understated, but to note that the geographical pattern of poverty reduction is different on a NIPA basis than on a survey basis, with Africa notably poorer. The poorest poor countries are relatively poorer using NIPA-based poverty measures. The two outcomes, over time and over countries, are of course related. In both, poverty declines more rapidly with economic growth using the NIPA methodology.

Each method has variants. For example, Bhalla and UNCTAD both use consumption data from the NIPA as their base, while Sala-i-Martin uses GDP. The distributional measures that are required to calculate poverty from the NIPA averages sometimes use many points on the distribution, e.g. quintiles or deciles, or even a distributional assumption coupled with a single summary measure such as the gini, see below. Within the survey method, there are different survey designs and different measures. The resource measure can be income or consumption. Questionnaire design differs from country to country, and sometimes over time within a country. For example, Indian poverty estimates from the latest large survey in 1999–2000 come are not compatible with those for earlier years. Official US inequality measures are not comparable before and after 1993.

For reference and to illustrate, it is useful to record some of the formulas that might be used. In the NIPA method, suppose that mean consumption is estimated from the national accounts, and we have a single inequality measure, for example the gini, from a household survey. Suppose that consumption is approximately lognormally distributed in the population. The poverty rate is given by

$$P_0 = Pr(x \le z) = Pr(\ln x \le \ln z) = \Phi\left(\frac{\ln z - \mu}{\sigma}\right)$$
(1)

where x is the measure of resources, typically income or consumption per head, z is the poverty line, μ is the mean of the logarithm of x, σ is the standard deviation of the logarithm of x, and Φ is the cdf of the normal distribution. Under the lognormality assumption, the mean of logarithms can be calculated from the logarithm of the means $\ln \bar{x}$ using the formula

$$\mu = \ln \bar{x} - \frac{\sigma^2}{2} \tag{2}$$

and σ can be calculated from

$$\sigma = \sqrt{2} \Phi^{-1} \left(\frac{g+1}{2} \right)$$
(3)

where *g* is the gini coefficient, See pp 13 and 113 of Aitchison and Alan Brown (1969). These formulas are only one way of turning means into poverty counts. Another way, which obviates the need for the lognormal assumption, is to use points on the empirical distribution, for example the quintiles provided by Klaus Deininger and Lyn Squire (1996). These are used to construct approximations to the cumulative distribution function, sometimes with supplementary functional form assumptions, from which the headcount ratio can be read off. (Incidentally, it is unclear to me in what sense these support UNDP's current monitoring procedures, UNDP (2001)which are based on the proposition that the effects of growth on poverty reduction are less when the gini is larger. Although it is true that growth reduces poverty more if the distribution is less spread out, the derivative also depends on where we currently are in the distribution.)

In a household survey, the quantity x is measured directly, typically at the household level. The headcount ratio is then estimated by the number of persons in the population who live in households whose x is below the poverty line. This version of the headcount ratio would be estimated by

$$P_{0} = \sum_{1}^{n} w_{h} n_{h} 1(x_{h} \le z) / \sum_{1}^{n} w_{h}$$
(4)

where w_h is the survey weight or inflation factor for household h, and n_h is the number of household members.

2.2 Are National Accounts or survey data more reliable?

There is a longstanding prejudice, at least in the west, that national accounts data are more reliable than data from household surveys, UNCTAD (2002) uses two arguments, that NIPA construction is more likely to be standardized across countries, and that NIPA data are available for virtually all countries. Average consumption from household surveys is often lower than average consumption from the NIPA, something that is as true in India as it is in the US, and this is interpreted as evidence that household surveys systematically understate consumption. And indeed, there are well-known difficulties with surveys. Rich households are often hard to reach; rich people who live in gated communities cannot be reached by surveyors, and when rich households are approached, they often refuse to cooperate, or send servants to speak to the surveyors. Miguel Szekely and Marianne Hilgert (1999)show that, in a number of Latin American surveys, the highest recorded incomes are around (and often less than) the typical salaries of a typical manager, as supplied by an international consulting firm.

There are also problems at the bottom. People who do not live in households—street people, or beggars—cannot be reached by household surveys. It is often expensive, difficult, or even dangerous, to travel to remote areas or to areas where there may be a concentration of poor

people. Statistical offices typically undersample poor areas, if only on cost grounds, and though this can be corrected for, accuracy is likely to suffer. Indeed, in some countries (such as Korea for many years) surveys are confined to urban areas. Because income, and to a lesser extent consumption, is unevenly distributed throughout the year, particularly in agricultural economies, the measured distribution of resources, and sometimes its mean, can be sensitive to the timing of visits throughout the year. Other issues concern the sensitivity of results to apparently minor changes in design; for example, when the Indian NSS experimentally changed the recall period for food from 30 to 7 days, the estimated poverty rate was cut by a half. Only part of this is to do with the sensitivity of measured consumption to questionnaire design. At least as large a part is played by the sensitivity of headcount ratios to measurement (and to the choice of poverty line) when the poverty line is near the middle of the distribution of resources. This is one of the many arguments for not relying too heavily on headcount ratios.

Although it is true that the conceptual framework of the NIPA is more *standard* across countries and over time than is the design of household surveys, that does not imply that NIPA data are necessarily more accurate. NIPA consumption estimates are typically constructed following a "commodity" approach. Consider, for example, consumption of a cereal, such as rice. In well-run offices, the statisticians have data on production, for example from crop cutting, aerial, or farm surveys, as well as data on imports and exports. In less fortunate cases, production is often projected from the rural population, on the assumption that output per head is roughly constant. From the estimates of production, imports, and exports, the statistical office computes net domestic consumption. From this, it must deduct business (including cereals fed to animals) and government consumption; data on the latter may be quite good, but on the former it is typically very poor. The best that can be done is often an allowance using some "multiplier" or "fudge factor" which, ideally, would be based on a business survey, but is often seriously outdated. An allowance also has to be made for investment uses, in this case inventories, which are also difficult to measure. The final total is a *residual*; there is no direct measurement of consumption. As a result, errors in measuring exports, or production, or inventories, or animal feed, are all cumulated into the estimate of consumption.

Other problems with the NIPA are documented by Dudley Seers (1983), who notes that, in many countries, less than half of the national income estimate is derived from primary sources. More important for my current purposes, he points to the difficulty of capturing "informal" (including, but not confined to, illegal) income generating activities, and argues that the share of such unmeasured activities in GDP falls as the economy develops. In consequence, the growth rate of GDP as measured will be overstated, as more and more "informal" activity becomes formal and is captured in the statisticians' nets. To the extent that household surveys measure consumption, as opposed to income, at least some of the informal and illegal activity will be captured. This could be thought of as the "Al Capone" effect; although the authorities could not prove that Capone was engaged in illegal activities, they could show that his expenditure was greatly in access of his reported income, and convict him on charges of income tax evasion. As a result, not only will the measured growth rate of GDP be too high, but the ratio of survey-measured consumption to NIPA-measured consumption will decline with economic development, and may

well be greater than one in the poorest countries. That this is in fact the case is documented by Massoud Karshenas (2001) and is at least in part responsible for the finding in UNCTAD (2002) that the poorest countries are relatively poorer when poverty is measured using NIPA method in place of the World Bank's survey methods.

In India there has been a vigorous debate about poverty in the 90s with much attention given to the discrepancy between the NIPA and the surveys. According to some accounts, the ratio of NIPA to NSS consumption fell from around 90 percent in the 1970s to less than 60 percent in the late 1990s, and this discrepancy was attributed by some to the failings of the NSS data, more or less in the terms outlined above. But recent work within the government has revealed serious problems with the accuracy of the NIPA measures of consumption. Recent revision and re-basing of the accounts generated large differences in some categories of consumption expenditures, even in nominal terms, and even though there was no conceptual change. A. C. Kulshreshtra and A. Kar (2002) and K Sundaram and Suresh Tendulkar (2002), reviewing this work, show that the NIPA estimate for consumption of fruits and vegetables in 1993–94 in nominal rupees more than doubled between the 1998 and 1999 versions of the national accounts. The estimate for clothing fell by about a half, and that for rent, fuel, and power rose by more than 40 percent. Even with some canceling out of pluses and minuses, total consumption was revised upwards by 14 percent. (This revision, if it were carried through to poverty construction using the NIPA methodology, would cut the Indian poverty rate by a little less than a half.) These revisions reflect changes in data collection practices, in many cases from firm or farm surveys, which are evidently no more reliable than the household surveys. Indeed, it is hard to see why we should expect them to be.

Household surveys vary a great deal in design and reliability from one country to another. In some countries, India and Indonesia being two notable examples, there is a long tradition of high quality survey practice. The author has spent a great deal of time working with Indian NSS data and with Indian NSS officials, and there is no doubt that tremendous care and expertise goes into the work. Interviewers are well trained-for example, all have masters degrees in statistics-and devote great efforts to ensuring cooperation, returning many times to households who at the first visit are unavailable. The NSS claims it has essentially zero refusal rates, and while this claim is probably not literally correct, it is clear that refusals are low. In India, the perception is the opposite of that in the west, with the survey data typically thought to be of higher quality than the NIPA accounts. I see no reason to disagree with that supposition. Of course, India is not everywhere, and there are undoubtedly countries where the surveys are a genuinely less reliable than the NIPA. Indeed, there are several examples of "failed" household surveys, where the results are simply not credible, see the examples in Karshenas (2002) and Deaton and Margaret Grosh (2000). But I do not believe there should be any general presumption that NIPA accounts are more reliable nor more accurate than are surveys. Indeed, as I shall argue below, there are some general reasons to suppose that surveys are likely to be *more* accurate when it comes to the measurement of poverty.

Although there has been little formal work in poor countries, there is a literature for the US and the UK, for example, documenting the characteristics of those who refuse to cooperate with

surveys, see for example Robert Groves and Mick Couper (1998). The findings are consistent with the view that is the rich, not the poor, who are more likely to be missed. I suspect that this result carries through to poor countries; it is certainly consistent with the results for Latin America in Szekely and Hilgert (1999). Another example comes from Jamaica where the statistical office faces the problem of collecting survey data in cities which contain no-go areas controlled by political warlords. Yet the statistical office goes to such places, with whatever protection it needs. But armored surveyors do not help penetrate the walled-off estates and gated communities of the rich. The South African statistical office tells similar stories. So if there is a systematic bias in household surveys, it is most likely to come from understating the consumption and income of the rich.

2.3 NIPA consumption/income is not the same as survey consumption/income

There are a number conceptual differences between NIPA and survey measures.

Items that are included in the NIPA, but not the surveys are

- consumption in the form of imputed rents of owner occupied housing
- consumption in the form of imputed charges for financial intermediation
- consumption by non-profits
- residual business consumption
- incomes from employer's contributions to pension funds

Items included in the surveys, but not in the NIPA

• the component of annuity incomes that represents rundown of assets, as opposed to income from assets

Neither NIPA nor survey consumption measures include those components of government expenditures that contribute to living standards, such as education or health, nor more arguable items, such as defense. If the NIPA methods uses GDP, not consumption, all of these items are implicitly included, as well as the net savings of firms, government, and foreigners; as far as I am aware, only Sala-i-Martin uses this methodology explicitly.

The first two items in the first list are conceptually appropriate, so that NIPA is correct and the surveys wrong. Consumption of financial intermediation is the difference between the interest charged by financial institutions to consumers as borrowers and the interest paid to them as depositors, a difference that is accounted as the implicit value of the financial intermediation, UN (2000, p. 47). That same source says that "the allocation of this imputed service depends on the country's policy," suggesting that the treatment of this item is not consistent across countries. In India, this item alone has grown rapidly over time, accounting for half of one percent of consumers expenditure in 1983–84, but more than two percent in 1993–94, see Kulshreshtra and

Kar (2002).

If expenditures by non-profits are on behalf of households, the third item could also be arguably correct. (The argument is better for an NGO running a soup-kitchen than for a political party.) If we are using the NIPA to compute household disposable incomes, the last item in the list will be included (again correctly), and it is missed by the surveys. Because household respondents cannot typically parse out the asset and income components of annuities, households will almost certainly misreport the total amount as income, which overstates income and saving, and understates dissaving.

There are two important points to note:

- The items that are missed by the surveys, and captured by the NIPA, are likely to be more important for richer than for poorer households. Poor households in India rarely have bank accounts, yet the implicit financial intermediation turns out to be the largest single item in the discrepancy between NIPA and survey consumption.
- If the items missed by the surveys are income elastic, as is plausibly the case, economic development, over time or over countries, will tend to widen the gap between NIPA and survey consumption. This is in addition to the similar effect caused by the gradual incorporation of more and more informal activity into the national accounts.

By the first point, and even if the survey mean is an unreliable underestimate of total (or average) consumption, it may nevertheless be accurate for the poor. Even if we believe the NIPA, there is no implication that underestimation of the survey mean implies overestimation of poverty. This has been repeatedly noted in the Indian debate. If we also accept the second point, then the growth of average NIPA consumption will overstate the growth of consumption of the poor, and use of the NIPA method will risk overestimating the rate of poverty decline. Ideally, this would not happen if consumption on poverty reduction would be moderated by widening consumption inequality. *But this cannot happen in practice.* The only source of distributional information is the surveys, which exclude the items in question, so that their faster growth than average *cannot* be recorded, *even in principle.* The situation is almost certainly worse still if we were to apply the NIPA method using broader measures, such as GDP. Furthermore, the most likely reason for surveys to underestimate mean consumption, the under representation of the rich, means that the surveys will also understate inequality.

The evidence in UNCTAD (2002, 48, Chart 8) and Massoud Karshenas (2001)shows that, compared with survey measures, NIPA based poverty measures are relatively higher for the poorest of poor countries, and relatively lower for the less poor countries. This presumably comes from the fact that the ratio of NIPA to survey consumption increases with the level of economic development, which is consistent with the argument above and militates in favor of using the survey estimates for poverty measurement. Because the items that are both included in the NIPA and excluded from the surveys are not consumed by the poor, this evidence is an

argument, not for the use of the NIPA, but for the surveys. As we have seen, the changing ratio of NIPA to survey consumption can also come from the fact that the surveys capture more informal activities, if not their proceeds, at least the consumption based on them. Once again, the surveys give the right answer, and the NIPA the wrong one.

2.4 From NIPA means to poverty: using distributional information

In the NIPA method, mean consumption (or income) is used to yield a poverty measure using data on the distribution of consumption (or income), for example using formulas such as (1), (2) and (3). The measure of distribution, σ , the standard deviation of logs in (1), the gini coefficient *g*, or more comprehensive information on distribution, come from household surveys which are essentially the *only* source of distributional information. In principle, if NIPA consumption (say) could be "corrected" to place it on a survey basis, and if the distributional information were up to date, and the NIPA information consistent with that from the surveys, it would not matter whether the NIPA or survey method were followed. It should be noted that those who endorse the NIPA method and reject the accuracy of the surveys, are nevertheless prepared to accept and use the distributional information that they contain. It is quite unclear whether there are any reasonable assumptions under which such a schizophrenic treatment will be valid, Ravallion (2002). Indeed, the most frequently cited reason for survey failure is that the consumption of the rich is missed. If so, both mean and inequality will be understated, and the use of the latter to calculate poverty rates based on NIPA estimates of the former will bias down the estimates of poverty.

In many cases, survey data are only irregularly available—which is one of reasons why the NIPA method is used—so that the information on distribution is out of date. It is often argued that this matters hardly at all, because income distributions change only slowly, and because studies, usually based on the Deininger-Squire data, have shown that there is little or no systematic relationship between growth and changes in distribution. In terms of the formulas (1) through (3), we can take a fixed value of σ or g, and simply use the trend in NIPA consumption (or in GDP if consumption is a fixed proportion of GDP) to update the poverty numbers.

These arguments are dangerous. At least some of the debate on globalization, growth, and poverty hinges on whether or not growth is equally shared, and whether it is indeed true that incomes of the poor grow at the same rate as the economy as a whole. We do no justice to that debate if we wish it away by *assuming* that the poor proportionately share the benefits. And we can hardly expect our poverty measurements to be taken seriously if the way in which the measurement is done *assumes away* the possibility that the benefits of growth are unequally shared. Whether or not it is true that growth increases inequalities, we need a method of keeping score that does not assume the answer. We cannot argue that growth reduces poverty without having demonstrated the fact.

That distribution is impervious to growth is somewhat implausible. It is also implausible that the

errors that cause the discrepancies between the NIPA and surveys contaminate only the survey mean, while providing an accurate measure of the distribution of resources. By distributing NIPA consumption to people in a fixed way, while ignoring the survey mean, we are assuming that the discrepancy between the survey and NIPA estimates can plausibly be assigned proportionately to everyone. That is inconsistent with the supposition that the rich are less likely to be covered in the survey. It would also require that the discrepancies are proportionately the same for the different goods and services that comprise consumption, as well as between rural and urban sectors as well as the different regions of the country. In many cases, we have good evidence that these assumptions are not true. Drawing again on India, Sundaram and Tendulkar (2001) have shown that the differences between NIPA and survey estimates differ systematically across commodity groups so that, because the poor spend their budgets in different ways than the rich, it is certainly incorrect to assume that the discrepancies are distributionally neutral. It is also clear than there were real changes in the pattern of consumption inequality in India in the 1990s, changes that are consistent with what might be expected from an economy under gradual reform and where the new opportunities are far from equally available to everyone. Already successful states in the south and west grew the fastest and systematically diverged from the poorer, less successful states. There were also increases in inequality between urban and rural sectors, and within urban sectors, Deaton and Jean Drèze (2002). To assume that distribution is independent of growth is not to measure poverty, but to assume it.

Finally, it needs to be reiterated that the inequality measures from household surveys do not match the quantity that is being measured in the NIPA and which they are being used to redistribute. The measure of resources whose distribution is being measured in the surveys is not the same as the measure of resources that goes into the NIPA, and cannot therefore be used to spread it. The goods that are included in the NIPA, but excluded from the surveys, are not taken into account in survey measures of distribution, so that if they grow more rapidly than GDP as a whole, and accrue mostly to the rich, there is no possibility of the fact being picked up in the inequality estimates. The measures listed by Deininger and Squire, for example, relate to survey consumption, or survey income, sometimes on a household and sometimes on an individual basis. Only under very strong conditions do they tell us anything about the distribution of GDP, or even the distribution of NIPA consumption. And as we have seen, the distribution of NIPA consumption over individuals could be widening, even as the distribution of survey consumption over individuals is constant. So that results that show no relation between growth and inequality, quite apart from the fact that they are true on average, not for individual countries, are not informative about what we need to know, which is what is happening to the distribution over persons of NIPA consumption, or NIPA disposable income.

2.5 Inequality and the discrepancies between NIPA and survey estimates

It is useful to combine some of the elements of the foregoing discussion into a simple formulation of NIPA and survey accounting that allows for the fact that measurement errors are likely to work in different ways on the two estimates. To see how this might work, write

$$Y = \sum_{1}^{N} y_{h} \tag{5}$$

where y_h is the consumption (or income) of each of N households in the economy. Using surveys, we estimate y_h from a random sample of households, and add up to get Y. In the NIPA, by contrast, we add up over all transactions. To make the argument simple, suppose that each household makes a single transaction each year, in which it spends its entire income or consumption at once. In practice, (5) is measured with error, and one way to think about it is to rewrite (5) as

$$Y = \sum_{1}^{N} p_n(y_h) y_h \tag{6}$$

for the NIPA, and

$$Y = \sum_{1}^{N} p_s(y_h) y_h \tag{7}$$

for the surveys. The functions p_n and p_s are the probabilities that the "transaction" y_h is captured in the NIPA and in the survey respectively. These two types of probability are likely to behave differently from one another. In particular, because the NIPA picks up formal sector transactions, and misses many informal transactions, the probability of picking up the transaction is likely to be *constant* or even *increasing* in its size. In India, the consumption of the Maharajah or of the software millionaire is more likely to show up in the national accounts than is that of a farmer or street trader. By contrast, the surveys are much less likely to capture the Maharajah or the software millionaire, so that the survey probabilities are *decreasing* in the size of each income. Under these assumptions, both NIPA and surveys will underestimate total consumption. However, making the distribution of income less equal will increase measured NIPA if large transactions are more likely to be recorded than small ones, while simultaneously decreasing aggregate income from the surveys, because the larger the share of income received by the rich, the less will be captured by the surveys.

If there is any truth to this story, increases in inequality will cause the gap between NIPA and survey aggregates to rise. Clearly, this is a testable proposition, and is needs to be examined in the data. If borne out, the implications for measuring poverty are important because the survey estimates, which miss the rich but not the poor, are the right ones to use. The NIPA, on the other hand, by following the money, is more focused on those who spend most money. Using the NIPA method in these circumstances will overstate the rate of decline of poverty by attributing to the poor the growth rate that is, in fact, a characteristic of those nearer the top of the distribution.

2.6 Conclusions and recommendations

For all the reasons discussed above, NIPA based poverty measures may be misleading, and are quite likely, in practice, to overstate the reduction in poverty over time, as well as to exaggerate

poverty differences across countries. Instead, household surveys should be used to estimate the extent of poverty by directly measuring the living standards of poor households, rather than by imputing them through a long chain of inference, many of whose links are weak or absent. In some cases, surveys do not exist, or are seriously outdated, so that there is little option to measure GDP, or aggregate consumption, and to make the best possible guess about what is happening at the bottom of the distribution. But such measures should be recognized for what they are—guesses—and should not be treated on the same footing as direct estimates of the living standards of the poor.

What needs to be done is to commit to the construction of an adequate data base of household surveys for poverty analysis, and to work on improving it. Some useful steps along that process:

- Make an inventory of where we are now. Some of that is currently available through the World Bank's poverty website, but we need better documentation of existing surveys. For example, for each survey we need to know the coverage (for example, national or urban), the resource concept that is collected (consumption or income), and relevant aspects of the questionnaire design, such as the degree of detail in reporting (200 consumption items versus 30, for example, or a single income question versus a dozen), the recall periods over which data are collected, and how seasonality is handled. Changes in survey practice need to be documented; they are known to a few insiders, but are hard to find for almost everyone else. Surveys that failed or are otherwise clearly unsafe need to be identified and labeled as such.
- The Bank should issue an annual poverty CD-ROM containing the above information. When household surveys are publically available, the relevant data can be included, or links provided on the website, but even when they are not, percentiles of the income or consumption distribution can be provided. China is a good example of a country where the surveys are not available, even to the World Bank, but where the Chinese statisticians provide (to the Bank) percentiles of the distribution of income per head which can then be used to calculate poverty or inequality measures. I have recently calculated "corrected" percentiles for the latest Indian survey, and these will could be made available in the same way.
- The better documentation and availability of either the surveys themselves, or of summary statistics derived from them, will open up the process of poverty calculation. Currently, world poverty figures, and poverty figures for many countries, can only be calculated by the Bank. The process needs to become more open if the estimates are to carry more credibility. Currently, the (flawed) methodology of Bhalla and Sala-i-Martin is the *only* way that outsiders can calculate world poverty estimates
- Given a good inventory, it will be possible to know where surveys are seriously lacking. This is not just a matter of identifying countries that have no, or at least no current survey. We also need to know which surveys correspond to reasonable best-practice standards to

the extent that they could be accepted as a part of a poverty monitoring system. The UN and the World Bank should consider whether it would be helpful to revisit the original goals of the Living Standards Measurement Surveys, which was to develop a *common* survey instrument that would support internationally comparable estimates of poverty and inequality, doing for surveys what the SNA does for the national accounts.

• Until common surveys are available, we need more work on methods of adjusting the existing surveys to make poverty measures more comparable than is currently the case. An important current difficulty is that some surveys, for example from China and from all of Latin America, measure *income*, while most other surveys measure *consumption*. The last set of \$1-a-day poverty counts scaled the data in the income surveys by the ratio of consumption to income in the NIPA. It should be clear from the foregoing arguments that this is not likely to be satisfactory, though it is a good deal less obvious how to do better. These and other comparability issues will only be sorted out gradually over time, and it is here that academics can make a useful contribution. But we can only do so if provided with more of the raw poverty data than is currently the case.

3. What to do now?

In the short run, I believe that the World Bank's survey-based monitoring procedures for the \$1 a day and \$2 a day counts should be endorsed, with the emendations discussed in Section 1. To reiterate, this would involve accepting the local poverty lines from the Bank as a starting point, "editing" them using local knowledge, and updating them using local price indexes, all of which would be feasible for the UN system in the short run. The key flaw in the Bank's monitoring is not the work itself, but its lack of replicability and transparency. When other agencies and scholars can do the monitoring for themselves, we will understand much better than we currently do how the numbers vary with different assumptions and different data, and the assessment of progress toward the MDGs will not be the preserve of a single institution.

An alternative, or perhaps a complement, would be for the UN and the Bank to accept joint responsibility for creating and publishing the poverty numbers. Whatever agency is charged with the task needs to be insulated from policy making, particularly policymaking in the Bank. National statistical offices are carefully insulated from political interference—for example by giving their directors substantial terms of office, and by sets of protocols that prevent politicians or policymakers interfering with the production of the data—but such safeguards on the poverty data have been notably absent within the World Bank. High profile Bank publications have proclaimed quite different trends in world poverty, see Deaton (2002)and such disagreement undermines respect for the estimates.

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