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Why be afraid of the truth? Poverty, Inequality and Growth in India, 1983-2000

Nov 7, 2004

This version Dec 6, 2004

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* Oxus Research & Investments. We would like to thank Suman Bery for discussions, and Aditya Wali for excellent research assistance. This paper is a revised and extended version of a research report done for the Planning Commission, The Myth and Reality of Poverty in India.

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Abstract

This paper evaluates the performance of the Indian economy on five related dimensions: employment, wages, economic growth, inequality and absolute poverty. The findings run counter to some popular beliefs. For example, rather than a large slowdown as claimed by several recent studies, real wage growth in the 1990s nearly doubled its earlier pace. This finding contradicts the job scarcity conclusion. The two "facts", low job growth and high wage growth, are reconciled by noting that the growth in the potential labor force declined significantly in the 1990s. Instead of a job scarce economy, one strong conclusion is that India is transitioning from a labor surplus economy to one with lesser surpluses and emerging labor scarcities.

The growth in wages of the poorest (the agricultural workers) over the last two decades also allows one to estimate what the poverty decline in India would have been if the government's consumer expenditure surveys had matched the growth rates revealed by it's own surveys on wages and income. This yields an estimate of poverty in India in 1999/00 which is not 26 percent of the population as claimed by the Government, or 35 percent as claimed by the World Bank, but rather close to 13 percent.

Executive Summary

This paper is about economic growth in India since the early eighties, and the associated changes in inequality and absolute poverty. A re-examination of this trinity of issues is urgent because a large part of the present debate in India is about what can be deduced about the effect of economic reforms, both on economic growth and especially on inequality and poverty. The debate also has urgency because since the general elections in May 2004, the new government has issued a blueprint for policy, the Common Minimum Programme (CMP). Explicit in the CMP is a particular interpretation of what "actually" happened in India in the 1980s and 1990s, and this interpretation has guided it to make several policy recommendations.

Our road to examining "what happened" involves a somewhat detailed exploration of each of the three "debatable" issues. In particular, various sources of data (household surveys and national accounts) are investigated for the patterns they reveal. **The first major result of this paper** is that the pattern of growth revealed is *not* the usual divide between household surveys and national accounts i.e. that surveys reveal a significantly lower growth than corresponding data from national accounts. It is the case that consumer surveys (conducted by the National Sample Surveys Organization or NSSO) indicate a shockingly low average growth in per capita consumption for the last two decades, and the national accounts yield an estimate that is substantially higher. This divergence is to be expected since it has not only been observed for India but also in most parts of the developing world. What is surprising is that while the consumer surveys show low growth, income and employment surveys conducted by the *same* organization, using identical methods, sampling patterns and for several years even the same households, yield a magnitude and pattern of growth which is near identical to that revealed by the national accounts. This clearly has radical implications for what happened, and poses the question: which NSSO *survey* does one believe?

Analysts, and policy makers, have often combined the growth rate that emerges from national accounts and the distribution which emerges from household surveys. This is obviously incorrect. One methodological compromise is to combine the information from the different NSSO *surveys*, something that is attempted in this paper. When this is done, using extremely plausible and defendable assumptions, it emerges that poverty in

India in 1999/2000 (the last year for which a major consumption and income household survey was undertaken) was not 26 percent of the population as estimated by the government, nor substantially higher (28 to 35 percent is the estimate of different institutions and scholars, with the World Bank being at the top end of this estimate) but rather close to 13 percent i.e. half of the official level and a third of the World Bank estimate. **This is the second major result** of this paper.

While the level of poverty circa 2000 has been shown to be much lower than estimated, it does not necessarily prove that economic reforms, initiated by the Indian government in 1991-1993 and continued by successive governments since, were good for the poor. The answer is complicated and involves an examination of both the trends in growth acceleration (or deceleration) and the trends of change in inequality. Regarding the former, it is observed, in rather unambiguous fashion, that there was a significant acceleration in per capita growth after the institution of major economic reforms in the early 1990s. Though this acceleration is not apparent in overall GDP growth, which has stayed relatively constant between 5.5 to 6 % p.a., it is most apparent in per capita growth, which has accelerated from 3 percent in the 1980s to around 4.5 percent in the nineties. This acceleration is not only there in the national accounts data (seen by all) but also is present in the NSSO survey data on incomes and employment (as highlighted in this paper). **This is the third result** of this paper i.e. that there was a marked growth acceleration in the nineties. This result is the conventional wisdom, but what is "new" is the support for this result from NSSO household survey data.

Poverty decline, a major goal of Indian economic policy, is both a function of economic growth and change in inequality. "Growth" in inequality in the 1990s is a much debated subject with more than a few conclusions along the lines of a "pervasive" increase having been observed, and some scholars even asserting that inequality change in India was comparable to the large inequality increases observed in China. This raises two questions. First, if the inequality increase did occur, was it "inevitable" given the higher growth; second, was the large inequality increase that has been asserted, or even a lower inequality increase, even observed in India.

The answer to the first question is ambiguous. The association of change in inequality with economic growth is complicated. It could be that accelerated growth is *causally* accompanied by an increase in inequality, in which case the poor, the target of policy, may actually benefit. [Higher growth brings about an increase in inequality but in net terms there is extra income growth for the poor]. On the other hand, it could be that inequality change is large enough to matter, and eats away at the gains for the poor associated with higher average growth i.e. the incomes of the rich grow at a significantly faster rate than the incomes of the poor. Ultimately it is an empirical matter.

And empirically, the effect of inequality change on poverty decline has been observed to be very small; recent estimates suggest that less than 10 percent of poverty changes are a consequence of changes in inequality; most, that is, 90 percent or more, of poverty decline occurs due to growth. Inequality change associated with extra growth is equally small; recent estimates suggest that this elasticity (i.e. percentage change in inequality associated with a 1 percent change in per capita incomes) is very small, also around 0.1.

But these "low" effects of inequality change are average effects, and India might be an outlier. Consequently, the various studies of inequality change in India, the different methods, are all evaluated along with the change in inequality recorded by the official data. Most studies and methods indicate that inequality traversed a *very mild* V shaped pattern over the last 20 years or so (specifically, from 1983 to 1999/2000), with the left leg higher in magnitude than the right leg, and 1993/1994 being the year of lowest inequality and 1983 being the highest. The worst case "estimate" is that the worsening of inequality from 1993/94-1999/00 was a very mild affair, with a cumulative magnitude of the log variance of no more than 10 percent, and for the Gini, about 3 to 5 percent. Such a change is, in a worldwide context, considered to be to be very, very small. For example, the average cumulative change in inequality for a longish period (more than 4 years) in the developing world in the nineties is close to 3 to 5 percent (for the Gini). Just the use of different price deflators reduces this worst case estimate to less than half. **This is the fourth major result** i.e. that the *worst* estimated inequality change in India was less than 5 percent in the 1990s and that this change was less than the variation observed in the different definitions of per capita consumption in the same NSSO survey!

Collecting the major results presented, the following conclusions emerge: there was acceleration of growth in the nineties, that the level of growth was extremely high throughout (among the top ten economies in the world), and there was little inequality change. Thus, the Indian economy probably achieved a perfect double: high growth with near constant inequality. Normally, such a result would be occasion to look for what went *right* with economic policies, and proceed with doing more of the same i.e. accelerating reforms. But there is a notable “negative” feature of the Indian economy, particularly in the nineties: low job growth. In the 1980s jobs were being created at the rate of 3.1 percent per year; in the 1990s, the rate more than halved to only 1.4 percent. Despite all the results of good growth, low inequality change, and high poverty decline, this slow job growth is disturbing. How can a miracle economy produce so few jobs?

Government and analysts have noticed the low rate of growth of jobs, and concluded that something was wrong with economic policy. There is one explanation (not ours but a theoretical possibility) that fits a “pessimistic” scenario: economic reforms in India mattered for a privileged few, the economic growth rate was maintained by increases in productivity and wages of the rich, and that for the poor, unemployment and lack of work opportunities was the “answer”.

If something was wrong with economic policy in terms of job creation (or job creation primarily for the rich) then the fault lines should show up in unemployment rates. But no definition of unemployment shows any trend increase in unemployment in the 1990s: not for usual status, not for weekly status, not for males, not for females. In addition, participation of child labor has significantly declined, and educational enrollment in the 15-24 age group has significantly gone up, so the 1990s were seemingly better.

But the unemployment data is dismissed by the reform critics on the grounds that these data are an outcome of the “discouraged worker” phenomenon. The reasoning goes as follows: there are few jobs available (“just look at the data on job growth”) so people withdraw from the labor force i.e. they stop looking for work. This causes the unemployment rate to be artificially lower than the reality. In other words, the critics mention, a plausible and consistent interpretation of the job reality is that there has been a large increase in the percentage of discouraged workers in India.

The **fifth major result** is that this discouraged worker hypothesis/explanation for the Indian labor market of the nineties is a myth, albeit a seductive myth. Examination of population data reveals the well known result of a decline in the growth rate from 2 % p.a. in the 1980s to 1.75 % p.a. in the nineties, and 1.6 percent in recent years. Also remarked upon is the slowdown in growth in the labor force, but this variable is confounded by the discouraged worker phenomenon. There is one variable that can help identify whether low labor force growth was due to demand (low number of jobs) or supply (low number of people available for jobs). This variable is the *potential* labor force, the number of people alive in the age group 15-59. Strikingly, and little remarked upon is the fact that the rate of growth of the potential labor force significantly declined in the 1990s: from a 1980s rate of growth of 2.7 % p.a. to only 1.9 % p.a.. Thus, low enough as the average job growth was, it may have been *caused* by low growth in "supply" i.e. the Indian economy produced as many jobs as "demanded". Thus, a major reason for the lower rate of growth of jobs is the considerably lower rate of growth of the *potential labor force*. This means that jobs became scarcer in the 1990s. This also implies, at a minimum, that the Indian economy in the 1990s witnessed a declining labor surplus economy, if not an emerging labor scarce economy.

This result is contrary to conventional wisdom, and certainly the prevailing political wisdom; the latter, in the name of social justice and a "progressive" with a human face slogan, is emphasizing a large expansion in "food for work" programs, and *employment generation* programs. Both interpretations of the labor market cannot be right. It cannot be that people are plentiful and also that people are scarce, or even that people are becoming less plentiful. Some light can be shed on this debate by examining the pattern of wage growth. If people willing to work have become more plentiful, (the conventional view) then one should observe that the growth in real wages slowed down in the nineties. Indeed, this "fact" has been asserted to be true by several experts. The **sixth major result** is that this is also a myth, though it has been propagated by no less an authority than the Planning Commission of India, the same institution, incidentally, responsible for the policy of creating more jobs. Real wages in India have not only not declined, they accelerated in the 1990s – from 2.4 % p.a. to 4.5 % p.a. Nor was this acceleration confined to the software sector, or the rich sector etc. Rural wages accelerated from 2.4 % p.a. to 4.3 % p.a.. As did the wages of agricultural workers – from 2.2 % p.a. to 3.1 % p.a. in the 1990s.

These wage growth numbers emerge from a detailed analysis of NSSO data on employment and income, and are supported by other sources of wage data. This acceleration in wages in the 1990s can be taken to be in the nature of an "incontrovertible fact".

Thus, the wage facts indicate is that the conventional, government endorsed "jobs are scarce" conclusion needs to be modified. For some categories of workers, jobs are indeed scarce and these are likely to be for the non-poor, for urban, and for individuals belonging to households with rising aspirations i.e. the revolution of rising expectations. Preliminary indications of our ongoing research is that frictional employment has also increased in the 1990s i.e. another sign that India is moving from an economy characterized with under-employment and labor surplus to one increasingly characteristic of considerably less labor surplus and even lesser underemployment and unemployment.

The **seventh major result** is the light shed by wage growth on the "great inequality debate" in India. High wage growth in rural and urban areas for the 1983-2000 time period (3.1 % p.a. for rural and 3.2 % p.a. for urban) is strongly suggestive of at least a *constancy in inequality*. And a high average wage growth is indicative of a sharp decline in poverty. This is the **eighth major result**: the conventional view of poverty levels and decline in India needs to be radically changed. The official estimate of poverty in India for 1983 is 44.5 percent i.e. almost half the total population of India was absolutely poor just twenty years ago. After years of stagnation in the sixties and seventies, this rate declined to (again officially) to 26 percent in 1999/00. Our estimates, based *exclusively on the NSSO surveys for expenditure, income and employment*, indicate that the level of poverty in the two years are 45 and 13 percent i.e. thus, rather than a 19 percentage point decline, there was a decline almost double that amount – a 33 percentage point decline. Looked at differently, the NSSO wage based data indicates a poverty level in India in 1999/00 which is exactly half the official estimate! In terms of the rate of decline, our estimates are that poverty dropped to 27.3 percent in 1993/94 and 13 percent in 1999/00 i.e. the pace of decline post-reforms was at a 40 percent faster rate (1.7 percentage points a year in the 1980s and 2.4 percentage points a year in the 1990s). Though the paper makes no attempt to causally link economic reforms to the pattern of economic growth and therefore poverty decline, it most likely is the case that a faster rate of poverty decline was associated with the economic reforms induced faster growth, especially since this faster growth was not accompanied by an increase in inequality. This is **the ninth result**.

The above nine results emphasize the need for Indian policymakers to revisit, and revise upwards, the poverty line, which has stayed constant for thirty years. India is *transiting* from an economy characterized by absolute poverty to one characterized by relative poverty. We need a higher fraction, and number, of poor people not because economic reform policies have failed but because they have ***succeeded***. There are significantly less absolutely poor people in India than the official data (and experts) would have us believe; there are significantly more relatively poor people in India than what the government of India (or experts) want to believe. This is the **tenth, and final, major result** of this paper.

Introduction

The status of poverty and inequality in India in the late nineties has been mired in extreme controversy. No one doubts that significant growth has taken place. This growth has averaged about 5.6 percent per annum (hereafter % p.a.) for the last *twenty-four years*. Per capita growth has accelerated to 4.5 % p.a. today from 3 % p.a. in the early eighties. Prior to this spurt of sustained growth, per capita growth in India had averaged only 1 % p.a., with the seventies being the truly dismal period, when per capita per annum growth was close to zero. Comparatively, India's growth performance for the twenty-three year period 1980-2002, has been bettered only by a handful of countries.[1] Less recognized, India's record on inequality change over the last two decades has been even more remarkable, even if inequality change in the nineties is assumed to be near the extreme *upper bound* of most estimates, say 10 percent.

These statistics place the Indian debate in a comparative perspective. India's record on growth, and inequality change, has been spectacular. *Which means that the record on poverty alleviation has also been spectacular*. This conclusion follows, by definition, since the change in the head-count ratio of poverty is a function (albeit a non-linear one) of growth and inequality change. If inequality change is zero, poverty decline is entirely a function of economic growth, mediated by statistical initial conditions i.e. the congestion of the poor near the poverty line in the initial time-period.

Given this presumed exceptional performance, why is there any controversy about poverty in India? Because poverty is not measured by gains in per capita consumption as measured by national accounts but by changes in per capita consumption as measured by household surveys. And the growth as revealed by these surveys has been considerably less than the growth as revealed by national accounts. That is it – the entire explanation.

The most widely used form of survey data, and indeed the official source for measurement of poverty, are the consumer expenditure (CE) surveys conducted by the National Sample Surveys Organization of India (NSSO), which conducted the first household survey as far back as 1950-51. The only other organization to systematically

[1] These are (with respective average per capita growth rate, in international PPP dollars, in parantheses: China (7.5), Korea (5.4), Ireland (4.7), Vietnam (4.4), Luxembourg (4.1), Thailand (4.0), and Mauritius (3.9) percent. Then follows India with 3.2 percent.

conduct household surveys (with the first income distribution survey being conducted in 1964-65) in India is the National Council for Applied Economic Research (NCAER).

The difference in survey and NA growth is not small. The NSSO large sample[2] surveys indicate a ten and a half year growth rate from 1983 to 1993/94 (hereafter the 1980s) of only 1.2 % p.a., followed by the mildest of acceleration to 1.3 % p.a. growth rate in the six-year period (from 1993/94 to 1999/00, hereafter the 1990s). The sixteen and a half year average growth rate: 1.25 % p.a. The corresponding growth rate according to the national accounts (GDP) data on private consumption: 2.4 % p.a. The NA growth rate of per worker private income is even higher – 3.2 % p.a. The growth pattern of per capita survey incomes (from the E&U survey) is radically different, in levels, and change, than the NSSO CE survey: it shows a per capita growth of 3.1 % p.a. in the 1980s, and an acceleration to 4.2 % p.a. in the 1990s – figures closely corresponding to the NA data.

The difference between the two estimates of growth is 1.2 % p.a. Compounded over the 161/2 years, this means that either per capita consumption in India was 1.2 times the level observed in 1983, or 1.5 times the 1983 level. What one believes to be true means a difference in the head-count ratio of poverty in India at around 25 percent of the population, or around 13 percent. This is the first part of the debate on poverty in India.

The second part of the debate is related to what it is that the NSSO consumer expenditure (CE) surveys actually reveal. Under the strict assumption that the NSSO CE data are accurate, there should be no debate about what happened to growth, poverty, or inequality. Each of these "numbers" or estimates are spitted out by the unit-level data, suitably weighted. So all researchers should obtain the same results from the same data.

The debate is there because of *changing* definitions of the major variable of interest: the monthly per capita expenditure (MPCE) of each sampled household. In particular, because of definitional changes, the **1999-2000 NSSO** expenditure survey is,

[2] Every five years, the NSSO conducts a survey which sample more than 120,000 households, or more than 650,000 individuals. These are known as the large sample surveys. Such surveys were conducted in calendar year 1983, and agricultural years (July 1 to June 30) 1987/88, 1993/94 and 1999/00. In the other years, a "thin" sample survey is conducted with sample sizes often less than half of the large samples.

incomparable with any earlier survey, and it will be *incomparable* with any future survey. Not as well appreciated is the fact that the **1993-94 NSSO** expenditure survey, the "benchmark" for all results on inequality and poverty change, is also *incomparable*!

The changed definition of this central variable (used in computations of both inequality and poverty) has forced researchers to "simulate" what the per capita expenditure would have been in 1999/00 if the definition of MPCE had stayed the same as in 1993/94. Since the "truth" cannot be known, (because the definition of per capita expenditure changed) there are as many estimates of expenditure growth, inequality change, and therefore poverty change, as there are researchers. How to sift through the various estimates, and, in particular, how to construct consistency checks on the different estimates, is the task that this paper attempts.

This paper is in two parts. The first part explores the methodological considerations, and examines various forms of NSSO and national accounts data to understand a consistent picture of what happened to growth, inequality and poverty in India in the 1980s and 1990s. The second part attempts to understand what actually happened in the Indian economy over the last two years, given that the NSSO expenditure survey (CE) paints a conflicting story – conflicting both with the results revealed by its "sister" Employment and Unemployment (E&U) survey, and conflicting with the results obtained by the NA data. The second part also involves a detailed examination of trends in wage data, as revealed by all the non-NSSO sources available. In particular, the Agricultural Wages in India (AWI) series, the Cost of Cultivation surveys, (both issued by the Department of Agriculture) and the data on rural incomes collected by the NCAER.

This examination of wages, along with examination of data on population and GDP growth, leads to an entirely different view of the Indian economy than conventionally assumed. In particular, the following results stand out. First, that the 1990s were characterized by a decline in not just the labor force growth (as noted by most observers) but also by a large decline in the rate of growth of *potential labor supply* i.e. people in the 15-59 age group. This decline had major "intended" consequences for the labor market – if the labor market was characterized by underemployment in the 1980s, and overall GDP growth was maintained, then the labor market would reveal much less underemployment in the 1990s. The flip side of a tightening (or less slack) labor market is an increase in real wages. And this is exactly what is observed.

Contrary to the popular belief (a view critically examined) real wages accelerated significantly in the 1990s and unemployment rates declined. Real wages per worker increased at a scorching 4.5 percent per annum pace in the 1990s, compared to a much slower and only a 2.4 percent pace in the 1980s. For rural India, where most of the poor are, wage growth increased from a 2.4 % p.a. pace to 4.3 % p.a. For rural agricultural workers, the acceleration was from 2.2 % p.a. to 3.1 % p.a. These NSSO based results are orthogonal to the popular view, repeated both by government agencies and development experts, that agricultural wage growth decelerated in the 1990s to only a 2.5 percent pace, and decelerated from a 5 plus rate of growth in the 1980s. (The levels are incorrect as is the change).

Which wage growth estimate (NSSO data supported by NA data, or non-NSSO survey data) is right has an obvious and large effect on conclusions about poverty levels, and poverty decline. An overwhelming amount of the recent poverty literature in India has been devoted to establishing the percentage by which the NSSO CE survey of 1999/00 overstated mean expenditures, and hence overstated the cumulative six-year growth between 1993/94 and 1999/00. The “error” being talked about is small (1 to 2 percent) and this will have only a very small effect on aggregate growth; however, the different cumulative NSSO survey estimates of growth (consumption and income based) diverge by about 10 to 20 times this amount! Thus, the significant “growth gap” between per worker wage growth and per capita consumption growth during both the 1980s and 1990s dwarfs any calculations of the over or under-estimate of mean per capita expenditures in 1999/00.

The different estimate of growth revealed by the NSSO E&U survey provides a perspective on how different the poverty estimates would be if the CE surveys had shown growth magnitudes consistent with all other data. All calculations, and estimates, agree that inequality in 1999/00 was less than that in 1983; if not better, than certainly not worse. If the wage *growth* that is achieved by agricultural workers (2.5 % p.a. on average for sixteen and a half years) is imposed on the *entire* economy (a conservative assumption) then the poverty level realized in 1999/00 is half the official level of 26 percent i.e. 13 percent.

This number, and its significant departure from the official number (26 percent poor), should be kept in mind as the paper traverses a mine-field of data and estimates of growth, poverty, and inequality. Section 2a is devoted to an examination of the policy trinity: growth, poverty, inequality: what does theory have to say on the inter-relationships. The rest of Section 2 evaluates the “background” evidence on these three policy topics. Section 3 examines various methodological issues related to NSSO survey data. Section 4 examines the various estimates of inequality that are present in the literature. Section 5 is devoted to a detailed examination of trends in wages as revealed by the different sources of data, but especially by NSSO data. Section 6 evaluates the various poverty, and poverty decline, estimates that are present today in the “marketplace”. Section 7 concludes and offers an alternative view of “what happened in India in the 1980s and 1990s”.

Section 2a: **Poverty, Inequality and Growth: The Relationships**

A perspective on the intensive data discussion that follows is provided by an assessment of the debate on the "big picture" of what happened in India in the 1980s and the 1990s. On ideological and emotive issues like poverty and inequality it is often difficult to separate fact from interpretation. We attempt in this section to present an overview of what theory says, what is widely believed, and what the available data shows. The sections that follow examine the conclusions offered by different authors on this wide ranging debate.

A comprehensive evaluation of the poverty-inequality-growth nexus requires that the following three questions be addressed. First, what is the relationship between growth and poverty reduction? This is the all-important policy question – for a given amount of growth, say 10 percent, how much reduction in the head-count ratio of poverty is to be expected? The answer raises an additional second question – what happens to inequality when growth increases by 10 percent? Third, is there a relationship between the growth that occurred and initial conditions prevailing with regard to inequality? Would growth have been lower if initial inequality was higher? Or would growth have been higher?

The expected reduction in poverty, given a certain growth rate and with the assumption of a zero change in inequality, is highly non-linear. The pace of reduction, *ceteris paribus*, depends most critically on the congestion of the poor close to the poverty line[3]. The expected pace of reduction, per unit of growth was termed the "shape of distribution" elasticity by Bhalla, and is called the "derivative with respect to growth" by Deaton-Dreze. For the NSSO consumption distribution, and poverty levels around 25 to 40 percent, the average all India value of the SDE is around 0.85. In words, this value of the SDE means that for each 1 percent growth in per capita expenditures, the head-count ratio should decline by 0.85 *percentage points*, *ceteris paribus*. Note that this semi-elasticity is different than the conventionally measured elasticity between (log) poverty decline and (log) expenditure growth.

[3] See Anderson (1964), World Bank(1991), Kakwani(1993), Bhalla(2000, 2002) and Deaton-Dreze(2002).

This relationship between poverty decline, dH, and growth in expenditures (income) of the poor, Y_P' , is (see Appendix I for details) is given by the following equation:

(1) $$dH = SDE^* Y_P' = SDE^*(Y'_M + Y_X')$$

In this equation, H is the head-count ratio of poverty and dH is the arithmetic change in poverty between time-periods t and t-1. SDE is what was termed the "shape of the distribution elasticity in Bhalla(2002): *Imagine there's no country: Poverty, Inequality and Growth in the Era of Globalization*", hereafter referred to as *Imagine*. Y'_M and Y_X' represent the growth of average expenditures, and the percentage change (or growth) of the share of total expenditures that accrue to the "poor".

Specifically, the SDE applicable at any point in time t is given by the initial conditions prevailing at time period t-1 i.e.

(2) $$SDE_{t-1} = P *f(Y_P)_{t-1}$$

where P is the poverty line (a given constant), and $f(Y_P)$ is the density of the cumulative distribution of per capita expenditures at time-period t-1, and Y_P represents the mean incomes of the individuals near the poverty line at time-period t-1.

The interest is in relating the growth in incomes at or near the poverty line, Y_P', to the poverty decline, dH. This can be written as *the sum of the growth in aggregate mean incomes , Y'_M , and the growth in the share of incomes of the fraction X close to the poverty line, Y_X'* i.e.

(3) $$Y_P' = Y'_M + Y_X'$$

Equation (3) highlights the simple fact that it is the change in inequality around the poverty line, Y_X' , that matters for poverty decline and not the change in aggregate inequality as represented by the Gini or the log-variance etc.

Collecting the terms, one obtains the two relationships of interest,

(4) $$\mathbf{dH = P * f(Y_P)_{t-1} * (Y'_M + Y_X')}$$

and (dividing both sides by the head count ratio in the initial time-period, t-1, H_{t-1}, and then dividing both sides by mean expenditure growth Y'_M) one obtains the much used elasticity of poverty decline with respect to expenditure growth, *ç:*

(5) $$ç = (dH/H_{t-1})/(Y'_M) = (P/ H_{t-1})* f(Y_P)_{t-1}*(1 + (Y_X'/ Y'_M))$$

or (5') $$\mathbf{ç = (SDE_{t-1}/ H_{t-1})*(1 + (Y_X'/ Y'_M))}$$

Analogously, the equation for the arithmetic change in poverty, dH, the variable of policy interest, is given by

(4') $$\mathbf{dH = SDE_{t-1}*(Y_X' + Y'_M)}$$

Equations (4') and (5') are the equations of greatest interest for this paper, and allow us to have a perspective on the relationship between poverty, inequality and growth. These equations clear up a lot of confusions. First, the commonly used elasticity *ç* is shown to be a function of the congestion around the poverty line in the initial time period, the change in inequality of people around the poverty line (given by $*(1 + (Y_X'/ Y'_M))$ *and the head count ratio in the initial time-period*. As equation (5') shows, one can obtain an artificially higher estimate of poverty decline if the initial level of poverty is *lower.* Thus, the elasticity cannot be used as an indicator of poverty reduction performance, as used by several World Bank documents and Sundaram-Tendulkar[4]. Second, equation (5') clearly shows that the much argued initial inequality has no role to play in determining the path of poverty change.[5] Third, and most importantly for the discussion in this paper, note the inequality variable in equation (4'). *What has an impact on poverty reduction is not a change in an aggregate index like the Gini, but rather the change in inequality of those close to the poverty line*. This change can be approximated by the change in the share of the decile or quintile of population close to the poverty line, Y_X'. In other words, for discussions about poverty, the use of an aggregate inequality index has less

[4] This explains how Sundaram-Tendulkar obtain the joint paradoxical result of both a greater poverty-expenditure elasticity in the 1990s and a greater increase in inequality.
[5] See Bhalla(2004c) for a detailed discussion.

relevance than an inequality index which proxies for the change in the share of expenditures of those with expenditures close to the poverty line.

Heuristically, consider the following example. Assume that the average rate of growth of expenditures in an economy is X %, and that the bottom quintile has a rate of growth in expenditures of (X + y) %, the middle 60% has a rate of growth equal to (X –0.5*y) % and the top quintile of enjoys a growth of (X+0.5*y) %. In such an instance, aggregate inequality can show a significant increase, yet the poor clearly benefited disproportionately more from growth.

This is not just a theoretical point. Consider the inequality data presented in Table 1. These data document the trend in inequality in India according to quintile shares of per capita wage incomes yielded by the three NSSO E&U surveys. A considerable part of the discussion in Section 4 of this paper is on the question of "what happened to inequality" in the 1990s. Most (if not all) this literature has preoccupied itself with aggregate changes in inequality. Table 1 shows how this can be misleading. Note the annualized growth rates for the different quintiles for the 1990s – the fastest growth is achieved by the richest quintile (5.4 % p.a.), the second highest growth is registered by the poorest quintile, 3.7 % p.a.. Per capita incomes in the middle 3 quintiles increase at a lower rate, between 2.4 and 2.9 % p.a.. The Gini index finds this pattern to be highly unequal; its value increases by about 10 percent, from 0.4644 in 1993/94 to 0.5023 in 1999/00. On the other hand, the log variance index shows a steep fall in inequality in per capita incomes – it declines by more than 15 percent (from 1.03 to 0.87)! This divergent pattern among aggregate indices suggests that changes in such indices may indeed hide more than they reveal.

Table 1: Share and growth of consumption of per capita incomes, NSSO E&U survey

	% Share in expenditure			Growth (annualized) in expenditure		
Quintile	1983	1993/94	1999/00	1983 to 1993/94	1993/94 to 1999/00	1983 to 1999/00
1	3.7	3.9	3.8	3.3	3.7	3.5
2	8.1	8.9	8.1	3.6	2.4	3.2
3	13.0	14.1	12.7	3.5	2.4	3.1
4	20.3	21.9	20.3	3.5	2.9	3.3
5	54.9	51.1	55.1	2.1	5.4	3.3

Gini

	1983	1993	1999
Nominal	.5190	.4960	.5354
Real			
PC deflator	.4971	.4644	.5023
DT deflator	.5138	.4793	.5118

Log variance

	1983	1993	1999
Nominal	0.94	.112	0.98
G			
Real			
PC deflator	0.875	1.03	0.8733
DT deflator	0.932	1.06	0.8930

Source: NSSO Employment Unemployment Survey for years 1983, 1993-94 & 1999-00.

Section 2b: A broadly accepted view of what happened, 1983-2000

The conventional view, perhaps even universal wisdom, of what happened in the Indian economy in the 1980s and the 1990s is as follows. In the 1980s, economic growth averaged around 5.5 % p.a.. Population growth was around 2 percent, so per capita growth averaged 3.5 percent. This growth was a major acceleration from the past, and both the agricultural and the non-agricultural sectors of the economy shared evenly in it. Agriculture grew at a robust 3.7 % p.a., and non-agriculture grew at 6.6 percent. (Table 2a). Rural India, where most of the poor reside, benefited enormously and consequently absolute poverty declined at a fast pace in the eighties. The head-count ratio of poverty declined from 45 percent in 1983 to 37 percent in 1993/94. This decline is remarkable because both in the fifties and in the sixties, the poverty ratio had, in some year or another, hovered around 45 percent. (For the *same* poverty line, poverty in 1951 was 45.3, and in 1961, it was 46.5 percent).

In the early nineties, the Indian economy ran into a balance of payments crisis, a situation, which forced India (for the last time, as it happens) to go with a begging bowl to international agencies, namely the World Bank and the IMF. The government of India instituted major economic reforms in the short space of two years, 1991-93. The nature of these reforms is not in much doubt, or controversy. They were primarily oriented towards industry (e.g. abolishment of the licensing system; an industrialist just a decade ago needed the permission of the government to expand capacity, reduce capacity, or to initiate new investments), and external trade (the rupee was devalued in two super quick steps, over two days by 20 percent, and tariffs were reduced from outright bans and astronomical levels to open imports, and less astronomical levels.)

These reforms led to an acceleration of growth for a few years (particularly the three years from 1994/1995 to 1997/98, when GDP growth averaged over 7 percent). However, besides these years, growth acceleration was invisible, and GDP growth for the eleven years following the reforms (1992/93 to 2002/03) was exactly the same as ten years prior (5.7 % p.a.) to the reforms. However, because the reforms were oriented towards the rich, urban parts of the economy, this sector benefited disproportionately more i.e. inequality worsened, and worsened significantly.

According to this version of what happened, there are four important supporting pieces of evidence. First, the rate of agricultural growth collapsed in the nineties – from 3.7 % p.a. to only 2.6 percent. Second, and perhaps even reflecting this slowdown, *the rate of growth of real wages in agriculture collapsed to less than half the robust rate of the eighties* – from about 5 .0 to 5.5 % p.a. to only 2.5 percent. For considerations of poverty, this is considered clinching evidence because agricultural households constitute the poorest of the poor. Third, the ratio of per capita expenditures between urban and rural areas increased significantly. Fourth, poorer states lagged behind the growth of richer states. Thus, and not surprisingly, inequality worsened significantly. NSSO surveys also showed that the rate of poverty decline was definitely slower in the nineties. This is revealed by the fact that in the 1980s, the head-count ratio of poverty declined by eight percentage points, while in the 1990s poverty declined by only five percentage points. This slower decline in poverty is all the more shocking because it occurred with considerably faster growth i.e. a lower poverty decline in the nineties with a major acceleration in economic growth.

While no formal connection is made, it is nevertheless contended that the changed pattern of growth etc. in the 1990s was responsible for the changed pattern of poverty decline and inequality change. The changed pattern of growth was made possible by the institution of economic reforms in India in the early nineties. Hence, the "match-up" between 1980s growth and its *favorable* consequences, and the economic reforms and 1990s growth, and its *unfavorable* consequences.

According to the critics of reforms, there were two other side effects which make them somewhat unpalatable. First, the above four "facts" conclusively show that after decades of pro-poor growth, the situation in India paralleled that of China, a country where inequality deteriorated by more than 30 percent (on the basis of the Gini index) in the short space of six years. India, *in contrast*, at least until 1993/94, had witnessed constant to declining inequality! This puts into perspective how bad economic reforms were for inequality, and therefore the poor.

Second, and worse than the trend in inequality, the popular view continues, is the skewed nature of growth in the 1990s; it was "bad". And bad because it represented a period of (relatively) jobless growth. Organized sector employment after registering an

Table 2(a): Economic Growth in India: 1950/51 - 2003/04

		Growth In						
					NSSO			
Decades	Share of Agri.	GDP	Agri.	Non-Agri	Pop.	Pop. (15-59)	Workers (15-59)	Wages (15-59)
1950s	57.5	3.8	3.1	4.9				
1960s	49.5	3.6	2.3	5.0	2.3			
1970s	44.5	3.0	1.6	4.2	2.3			
1980s	38.5	5.5	3.7	6.6	2.1			
1983-93	**36.4**	**5.0**	**3.0**	**6.2**	**2.1**	**2.7**	**3.1**	**2.4**
1990s	31.2	5.5	2.6	6.7	1.8			
1993-99	**30.4**	**6.4**	**3.0**	**7.9**	**1.9**	**1.9**	**1.4**	**4.5**
2000s*	25.3	5.8	3.4	6.6	1.6			

Source: Economic Survey, Govt. of India (2003/04)
Notes:

1. Growth figures for 1950s represent annualized growth in respective variables from the 1st year of the decade (1950-51) to the first year of the next decade (1960s), so on and so forth.
2. *Growth figures for 2000s are till the year 2003/04 for which advanced estimates reported in the Economic Survey are used.
3. Decadal share of agriculture is estimated by taking simple mean over the respective decade(s)
4. Wages are real, for all workers, with poverty line as a deflator, and NSSO data for 1983, 1993-94, and 1999-00
5. Rural wages, non-NSSO data, are from Dreze-Sen and Deaton-Dreze.

Table 2b: Employment in India: 1981-2001

	Growth In Sector		
Decade	Public	Private	Total
1980s	2.08	0.37	1.56
1990s	0.04	1.20	0.39

Source: Economic Survey (2003/04), Govt. of India
Notes: Growth estimates are annualized with 1981 to 1991 representing the 1980s, so forth for the 1990s.

annual growth of 1.6 percent in the 1980s, barely grew in the 1990s (0.4 per cent per annum). (Table 2b).

There are debates within this overall story, but the broad parameters are accepted by most observers. While the GDP growth numbers are universally accepted, this is not the case with the numbers on inequality and poverty.

It is important to critically examine such evidence, since the raw, unadjusted numbers on shares of expenditures of the poor, do not show much difference between the 1980s and 1990s. A cumulative decline of a few percent in the share of consumption accruing to the poor may have more to do with measurement error than a genuine change in inequality. And even if it is a genuine change, it hardly constitutes a "pervasive" increase in inequality as argued by some observers. Further, as will be demonstrated, wage growth, the important supporting evidence, went in a direction completely opposite to that which has been asserted i.e. instead of halving between the 1980s and 1990s, the rate of growth of wages near doubled. This direct evidence is the most revealing about the likely pro-poor nature of the growth process in the 1990s.

Finally, the need to critically examine conclusions about rates of wage growth, inequality etc. arises because such assertions affect government policy, and according to some, even affect electoral outcomes. So much so that many have argued (and many within the new Congress government) that the Congress party won the recent general election, May 2004, or that the Vajpayee led NDA government lost, because of the political economy of poverty – the poor getting at least relatively worse off, and thereby kicking out the pro-rich urban-biased reform oriented NDA government[6]. This is not the forum to discuss the veracity of these claims; however, we want to emphasize that the view of the Indian economy outlined above is accepted by virtually everybody: economists, sociologists, policy makers and politicians.

[6] It is ironically strange reasoning i.e. the NDA government was defeated because it was too reform oriented so the people preferred the party that first launched economic reforms in India.

Section 2c: Deconstruction of the numbers on poverty, inequality, and growth

It is useful to compartmentalize the above version of events into two components: the facts that most participants in the debate agree on, and those that are subject to reconstruction, and therefore debatable. There are seven facts that have near universal agreement, facts which have agreement by all parties to the debate. (see Table 2). Unless otherwise stated, *the 1980s refers to the 10.5 years NSSO survey period 1983 to 1993/94, and the 1990s refers to the six-year period 1993/94 to 1999/00.*

Fact 1: Economic growth in India has averaged above 5.5 percent over the last 24 years. Per capita growth averaged 2.9 % p.a. in the 1980s, and markedly accelerated to 4.9 % p.a. in the 1990s.

Fact 2a: Consumption growth revealed by the NSSO consumer expenditure (CE) surveys has significantly lagged behind the growth as revealed by national accounts (NA). The latter (NA) shows an acceleration from 1.7 percent in the eighties to 3.6 % p.a. in the nineties. The NSSO CE survey reveals an extremely mild acceleration – rather than 1.9 percentage points per year increase as shown by the NA data, the acceleration was 0.1 percentage points per year – from 1.2 % p.a. to 1.3 percent. So there is no disagreement on the fact that per capita growth was higher in the 1990s than in the 1980s.

However, the acceleration in NSSO consumption growth rates was relatively minor. Which growth rate one believes therefore significantly affects one's perception of poverty decline and welfare improvement. The difference in absolute levels of income, dictated by different "sources" of growth rates, is not minor. For example, if a person had an expenditure level of Rs. 100 in 1993/94, and if she shared evenly in the growth process, then her income would either be Rs.108 in 1999/00 (NSSO growth) or Rs. 124 (NA growth). In terms of poverty decline, either one should expect a reduction in the HCR of 6.5 percentage points ***(hereafter ppt)*** or 14 plus ppt.

Table 2c: Different estimates of annualized growth in India, 1983-2000

	10.5 years **1983 to 1993-94**	6 years **1993-94 to 1999-00**	16.5 years **1983 to 1999-00**
Income related growth rates			
GDP Per capita	2.9	4.9	3.7
Private Income (NA)	5.0	7.0	5.8
Work force (reporting positive wages, NSSO)	3.1	2.3	2.8
Work force (All workers, NSSO)	3.1	1.4	2.5
Per worker private income (Positive wage)	1.9	4.7	2.9
Per worker private income (All workers)	1.9	5.6	3.2
Per capita Income (NSSO)	3.1	4.2	3.5
Wage data (NSSO, E&U)			
Rural	2.4	4.3	3.1
Agricultural workers, rural	2.2	3.1	2.5
Urban	2.3	4.9	3.2
All India	2.4	4.5	3.2
NCAER surveys; rural; 1981-99			3.3
AWI wages, agriculture	4.1	2.8	3.6
Cost of cultivation survey, agriculture	2.8	4.5	3.5
Consumption related growth rates			
Consumption per capita (NA)	1.7	3.6	2.4
Consumption per capita (NSSO)			
CE survey - MPCE5y definition	1.2	1.3	1.2
CE survey - MPCE5m definition	0.8		
E&U survey - MPCE5y* definition		0.0	

Source: National Accounts, NSS Consumer Expenditure and Employment Unemployment surveys for years 1983, 1993-94 & 1999-00.

Notes:

1. MPCe5m is based on a 30 day recall period for selected non-food items, while MPCE5y is based on a 365 days recall period for these items. See Table 2 and text for exact definitions.
2. * Stands for Employment Unemployment Survey estimates Work force reporting positive wages excludes the self-employed.
3. For national accounts, the time gap between the surveys is 10 and 6 years respectively.

Fact2b: Economic growth according to the NSSO E&U surveys

The NSSO E&U survey provides an estimate of income growth experienced by the same households for whom consumption was measured in the 1983 and 1993/94 surveys. These households reveal that their income growth rate was three times higher – at 3.1 % p.a. rather than 1.1 % p.a. – in the 1980s. For the 1990s, the income growth accelerates (and in tandem with national accounts data) to 4.2 percent per annum. As stated above, during this latter period, survey consumption growth was only 1.3 % p.a.

Fact 3: *different estimates of growth revealed by NSSO surveys and national accounts*. Which of the *two survey, CE or E&U, growth rates* is right? Since such radically different growth paths are indicated by the two surveys, it is important to establish their relative accuracy. Note that what is of interest is not the absolute level of incomes or expenditure yielded by the surveys – these will be different than their comparator (national accounts data) because of differences in definition, coverage, data collection, etc. What is likely to be unaffected by these considerations of definition etc. is the *divergence* in the respective growth rates. And these growth rates reflect different worlds, and different truths. So which survey is more correct? Or can both be right?

There are several methods by which the two growth rates can be compared. One is to assume that the national accounts growth rates are correct, especially since such growth rates evolve after several consistency checks. But purists object to this acceptance. Second, the two sets of growth rates can be filtered through consistency checks, also called "smell" tests or duck tests in *Imagine*.

Such tests were offered in *Imagine* for verifying the claims of several authors that despite rapid gains in average growth in two large economies, namely India and China, world inequality had in fact deteriorated; or that world poverty had declined only marginally over 12 years of rapid developing country growth. Smell tests[7] are analogous

[7] *Imagine*, p.78: What are duck and smell tests? They are tests that immediately tell you that something is wrong with the observed "reality". For example, if someone drives a BMW, and it is claimed that she is poor, then she fails the duck test; that is, she may walk like a duck, but she certainly does not run like a duck! Or, if it is claimed that world inequality changed by 5 percent in 5 years, or almost 4 standard deviations away from the norm, well, that fails the smell test. Or if poor Indians are spending a considerably larger fraction of their expenditures on fruits and vegetables, and on education and health, and yet have not crossed the poverty line, well, that fails the smell test as well.

to the test offered by a US Supreme Court judge for verifying pornography: "I know it when I see it". The smell test can be used to determine which of the two growth rates is correct.

One such consistency test is via an examination of savings rates, something that all participants to the debate broadly agree on. The implicit savings rate in the NSSO surveys can be computed[8] and compared to the national accounts data on household saving. In the early 1980s, the NA household savings rate was close to 15 percent, and this increased to 23 percent by 1999/00. If both consumption and income grew at the rate indicated by the respective surveys (a cumulative 18 percent for the expenditure survey and a cumulative 48 percent for the income survey) then the implicit survey savings rate would jump to 33.5 percent i.e. possibly the highest *household* savings rate in the world, and incidentally, considerably higher than even China! The only way to reconcile the two survey data is to admit to the large possibility that the CE surveys are severely under-reporting the growth in average consumption. If this is the case, then it means that the debate on whether the 1999/00 CE survey *overstated* average expenditures is somewhat misplaced.

Thus, among all the data sources (survey data of the NSSO, NCAER and national accounts data) the real growth outlier is the NSSO *consumer expenditure survey,* and an outlier by a somewhat large margin. This is unfortunate, since it is the unadjusted CE survey that provides the all-important poverty numbers which affect policy.

Fact 4: There are at least *two separate* definitions of NSSO survey based consumption *inequality* that are available for the 1980s. The reason there are two (and more) estimates is because of inconsistency in definitions, a topic explored in some detail in the next sub-section. For each definition, there are at least two price deflators that have been used: the official Planning Commission price deflator (hereafter referred to as the PC deflator) and the price deflator developed by Deaton-Tarozzi on the basis of price information contained in the NSSO CE surveys (hereafter referred to as the DT deflator). Thus, there are at least four real survey expenditure series that can be estimated.

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[8] The two different surveys, consumption expenditure and employment/unemployment can literally be merged for the large sample years prior to 1999/00 because they sampled the same households. For 1999/00, the savings rate cannot be calculated for each household (different households) but can be estimated for groups of households.

Consequently, there are four inequality change, and poverty decline, estimates that can be computed for the 1980s and the1990s.

Fact 5, inequality change: Regardless of the definition of per capita expenditure that is employed, a “safe” fact is that consumption inequality did not worsen between 1983 and 1993/94 and if it improved, it improved *marginally*. The mean estimate of the various definitions for the cumulative *change* in inequality for the 1980s is (log) -3 percent (Gini), and –8 percent (variance of logs). This is a very small improvement, both in absolute terms, and in comparative terms (with other countries). Further, it is important to note that the various definitions of inequality show greater variation *at any point in time* than the highest estimate of decadal change in inequality! For example, according to the 1980s NSSO definition (MPCE5m, see below) of per capita consumption, the Gini was around 0.33 in 1993/94; according to the 1990s definition (MPCE5y), it was 0.30, or about 10 percent lower.

Fact 6, poverty decline in the 1980s: The head-count ratio (HCR) of poverty declined at a rate of either –0.8 ppt per year or –1.06 ppt a year in the 1980s (the numbers depend on the definition of consumption used). These declines mostly reflect the respective annual growth rates in the parent definition of expenditure – either 0.8 or 1.0 % p.a., respectively. The two definitions differ in the degree to which they capture the purchase of “low frequency” items. Thus, each 1 percent growth, *ceteris paribus*, brought about approximately 1 ppt decline in poverty.

Fact 7, the 1990s debate: None of the facts stated above contain any information about any survey related data for the 1990s. This is because the definition of per capita expenditure changed in the 1999/00 CE survey. The definition changed in two major ways. First, questions on food expenditures (which account for more than half of the total) were asked on both a 7 day and 30 day recall basis in 1999/00; in earlier large sample years, it had only been asked on a 30 day basis. It is reasonable to believe that this alteration would affect the mean and the distribution of food expenditures, and therefore, both the mean and variance of total expenditures. The effect on the mean, the bias in the 1999/00 survey, has been the central focus of most attempts to “adjust” the CE 1999/00 survey and make it comparable to surveys conducted before.

The second alteration was in the method of recording of the expenditures on 5 major non-food expenditures: clothing & textiles, footwear, durable goods, education, and institutional (e.g. hospitals) medicine. Before 1999/00, these items were asked on *both* a 30 day and 365 day recall basis, and the expenditures for 30 day recall were recorded. In 1999/00, questions were not asked on a 30 day basis, and questions on 365 day expenditures were recorded (and pro-rated to reflect 30 day expenditures).
This change in the definition of consumption per capita (more accurately, the change in the definition of variables that constitute total consumption) has at least two implications for any study on growth, inequality and poverty. First, the inequality of expenditures will decrease with the 365 day questions on the recording of 5y expenditures as opposed to 5m expenditures (m for monthly, y for annual and 5 for the number of items). This occurs because of the "low frequency" of purchase of these five items – they are more likely to get recorded on an annual, rather than monthly, basis. Consequently, at any point in time, per capita consumption will be higher and the poverty level lower with the use of the 5y definition of consumption, rather than with the 5m definition. Second, and not emphasized enough, is that this changed method of collecting information on low frequency items most likely affected the variance of per capita expenditures.

The example of education expenditures is illustrative. Such expenditures are mostly undertaken (and remembered) on a monthly basis, and thus the 365 day expenditures may be "helped" along with the asking of 30 day expenditures. The lack of 30 day questions may mean that 365 day expenditures might get understated in 1999/00 relative to 1993/94 (when both 5m and 5y questions were asked). This implies that contrary to most assumptions, mean 1999/00 per capita expenditures might actually be biased *downward*.

The rest of the first part of this paper is about the resolution, and estimates, of the various biases that are present in the 1999/00 survey, and thereby interpreting what actually happened to growth, inequality and poverty in the 1990s. Besides the above facts, everything else is debatable, sometimes hotly so. It will be our attempt to try and bring to the readers attention all the evidence that exists in the "marketplace" including some new evidence on the bias in total expenditures; inequality in 1999/00 adjusted exclusively for the 7 day questions on food; the direction, and magnitude, of bias caused by movement from 5m to 5y; intensive use of the NSSO surveys on wages, employment and unemployment; and intensive use of the information on consumption expenditures contained in the 1999/00 E&U survey.

Section 2d: Which growth rate is correct

This section summarizes the evidence for the simple question: what happened to economic growth in India during the last 20 years? Normally, this is not a contentious question, as one just looks at the data on economic growth yielded by the system of national accounts. But for the study of poverty, one cannot make this simple calculation because as shown in Table 2c, the rate of growth obtained from national accounts (zNA) can substantially vary from the rate of growth obtained from household surveys (zSU) [9].

It has been a common practice to discuss the growth-poverty relationship through the joint examination of growth as yielded by national accounts and poverty as yielded by household surveys. This was termed the Peter-Paul problem in Bhalla(2000) i.e. one was using Peter's (NA) income to infer about Paul's (NSSO) poverty! If the growth rates are different, and if the NA growth rates are significantly above the survey growth rates, then one would artificially obtain the popular conclusion of "growth, growth everywhere and not much drop in poverty". This is because the survey growth rates, being lower, also estimate higher rates of poverty and lower rates of poverty decline[10].

The correct approach is obviously to either use survey means and the survey distribution or to use adjusted national accounts means and if need be an adjusted survey distribution (adjustments may be necessary for both the mean and the distribution). What is inappropriate is to use NA means and survey distributions.

If the correct procedure of using both survey means and survey distribution is adopted, there remains the problem of whether zSU is a "correct" estimate. If household surveys are capturing less and less of the underlying consumption, then zSu would be considerably less than zNa; and if the latter is closer to the "true" rate of growth, then poverty would be overestimated.

Thus, an understanding of what happened to growth is vital for interpreting trends in poverty. The survey capture ratio, i.e. the ratio of the survey to national accounts means

[9] See *Imagine* for a discussion about how widely the two rates of growth differ not only for India but for most countries of the world; the book further documents how, like India, this variation is a near-universal phenomenon, and especially so for the nineties.

[10] See *Imagine* for a detailed discussion of how this simple mistake has been made by several leading economists, and especially by the poverty reducing institutions e.g. World Bank, Asian Development Bank, etc.

for per capita consumption indicates how the respective growth rates are different, If the ratio stays constant, then the growth rates are the same; if the ratio increases, then the survey growth rate will be higher; if it decreases, then that is equivalent to the statement that the survey growth rate was lower than the NA growth rate.

The S/NA ratio in India has charted a significant decline. (See Figure 1). After capturing a high proportion (over 85 percent for the 1980 base and 70 percent for the 1993/94 base) of consumption in the sixties and early seventies, the S/NA ratio collapsed in the early eighties to about 65 percent i.e. a third of the NA consumption was missed by the surveys[11]. Some of this “missing” consumption is to be expected, but a large 35 percent or so cannot be accounted for by changes in definition, or coverage, or prices. This ratio has further fallen to 55.5 percent in the latest large sample survey year, 1999-2000, and fallen below 50 percent in the 2002/03 survey.

This was the “fact” alluded to in the previous section. Sen(2000) and Sen-Himanshu(2004) state that the choice of the base year in the national accounts[12] affected the prominent decline in the S/NA ratio[13]. Repeating this exercise for the 1980/81 base does not show any difference in the pattern, or rate, of decline of the S/NA ratio (see Figure 1). Indeed, the rate of decline in the S/NA ratio is higher if one uses the 1980/81 base!

But, as several scholars have stated, this decline in the S/NA may not be indicative of the CE survey under-estimating growth but rather be a reflection of the different definitions employed (e.g. consumer surveys do not include NGO expenditures, but national accounts do). Bhalla(2004a) offers several tests of this hypothesis; the overwhelming conclusion: the CE survey is genuinely under-estimating growth.

The most convincing answer to the question “Is the low *consumption growth* indicated by the CE survey accurate?” is provided by noting the *income growth* registered by the

[11] If the S/NA ratio had stayed constant at 65 percent, then even though at any point in time a considerable portion of NA consumption is not being recorded in the survey, it would still be the case that the growth rates registered by the two sources would be the same.
[12] The national accounts were revised to a 1993/94 base from a 1980-81 base in the late nineties.
[13] Ravallion(2002) and Sundaram-Tendulkar(2002) both concur with Sen’s(2001) erroneous conclusion that this ratio did not decline in the nineties in India. As shown in Figure , this is a very erroneous conclusion.

same households and recorded in the parallel E&U surveys. Income growth is a reasonable proxy ("instrument") for consumption growth, and especially so for the poor. As Table 2c indicated, the average NSSO E&U survey income growth rate for the 1980s was 3.1 % p.a., for the 1990s, 4.2 % p.a., and for the entire 16.5 year period: **3.5** % p.a. The levels of these growth rates match the NA data, as does the acceleration. There are marked differences in coverage between the survey and national accounts e.g. the NSSO E&U surveys do not collect income data for the self-employed. Yet, the NA *pattern* is mimicked by the NSSO E&U survey, and *not* by the NSSO CE survey. What is even more telling is the fact that the NSSO canvassed the same households for the two surveys in 1983 and 1993/94 (not over time but at each point in time). So the "same" households (split almost evenly between the poor and the non-poor) are indicating a growth rate of 3.1 percent in their incomes, but only 1.1 percent in their consumption. While some divergence is expected for the rich, it is not expected among the poor; for the latter, increases in income are likely to go towards consumption rather than income.[14]

Other data reject the NSSO CE growth results and instead support the results of the NSSO E&U surveys. For example, the NCAER has conducted a panel survey of rural households in 1970-71, 1980-81 and 1998/99. Foster-Rosenzweig have analyzed these data and report that the average growth rate in total rural incomes (wages and non-wages) increased at an annualized rate of 3.3 percent between 1981 and 1999.

Thus, several results stand out. First, that the NSS CE estimate of growth, and one used by all to estimate poverty, is the *smallest* estimate of growth – only 1.3 percent per year. Second, that the NSS E&U survey estimate of per capita income growth is very, very close to growth in GDP per capita (3.5 vs. 3.7 % p.a.). Third, that there are five different estimates of growth in the rural areas of the economy: the two NSS survey estimates (consumption and income), two based on wage indices, and the fifth the NCAER survey mentioned above. These rural growth rates are in the range of 3 to 4 percent. Given that the alternative to NSSO CE rural growth estimates match to such an overwhelming degree, it seems reasonable to conclude that the NSS CE surveys have vastly understated both average consumption growth and the growth rate in consumption of

[14] Indeed, an alternate definition of absolute poverty could be as follows: the "poverty line" is the expenditure level beyond which "permanent" savings become possible. The absolute poor are , by definition, those who do not have income to save.

the poor. It is easier to believe that the CE surveys have estimated mean consumption growth inaccurately than to believe that all other surveys. However, it is the case that what estimate of growth one believes has strong implications for poverty, and policy.

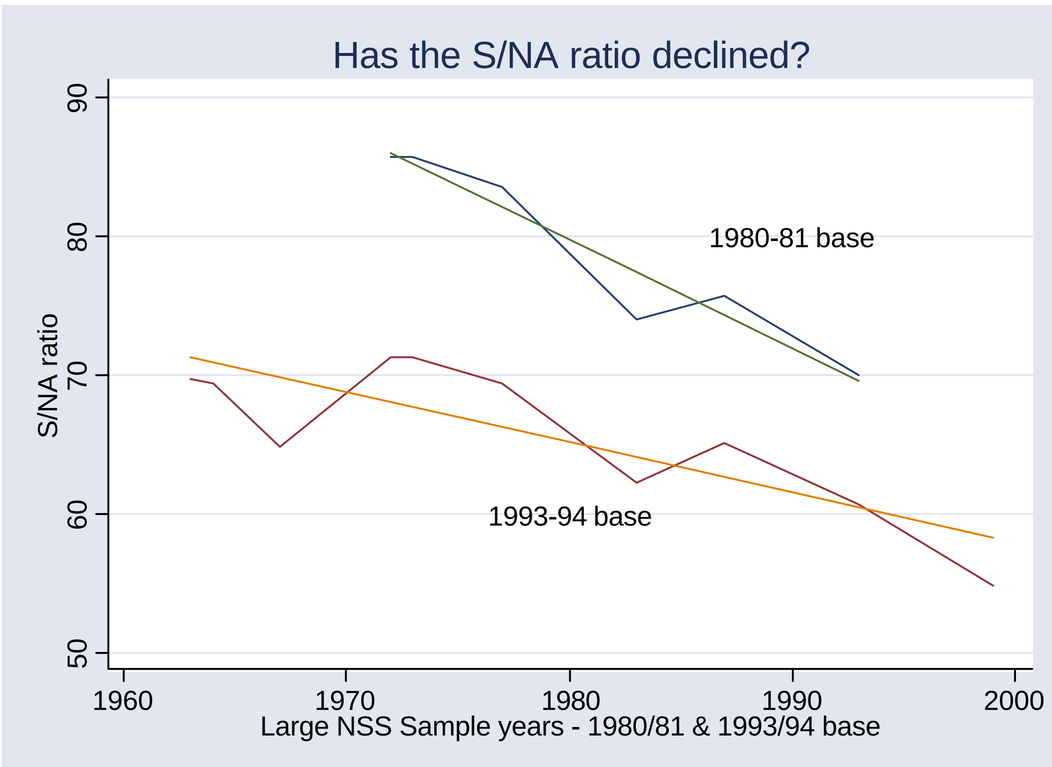


Figure 1

Notes 1): S represents the NSSO consumer expenditure mean and NA represents the mean of private final consumption expenditure in national accounts. Both means are in nominal terms; the “base” represents which NA series is being used.

Section 3: Evaluating the NSSO CE & EU surveys

3a: *Methodological Considerations*

A major reason for the controversy on "what happened to poverty in India in the nineties" is because of marked changes in the National Sample Survey questionnaire design for its CE survey; in particular, on the changed, and changing, *recall* periods for different items. Prior to (and including the 1993/94 survey) there is virtually no change in the questionnaire[15]; since then, and starting with the 1994/95 small sample survey, there are multiple changes which make any comparison with earlier years difficult, and controversial.

There are three broad expenditure items that can be classified. First, questions on the consumption **e**xpenditure per capita per month (**E**) of food items (including the category *paan* and intoxicants) asked on *both* a 30 day and 7 day basis as well as food asked individually for each recall - $\mathbf{Efood30_7}$ and $\mathbf{Efood7_{30}}$, and **Efood30** and **Efood7**, *with subscripts indicating the presence of identical questions for a different recall period*. When a subscript is present e.g. $\mathbf{Efood30_7}$, the notation means that while the subscripted recall period was asked (e.g. 7 day questions on food), the non-subscripted recall period is the one for which the expenditure is recorded.

There are two kinds of non-food items: non-food items which are always asked *only* on a 30 day recall basis (fuel and light, rent, consumption of services, toiletries, amusement, etc), and hereafter referred to as **Enfes30** (for monthly expenditures asked only on a monthly basis; hence no subscript). The second kind of non-food expenditure is on non-food items on a mixed recall basis; e.g. consumption of durables, asked either for both a 30 day and 365 day basis, or just a 30 or a 365 day recall period e.g. **Enonfood30** or **Enonfood365** or, $\mathbf{Enonfood30}_{365}$ and $\mathbf{Enonfood365}_{30}$.

There is one further complication. The category **Enonfood** was sometimes asked for five items (clothing, footwear, durables, education and institutional medical services), sometimes for only the first four (in 1983) and at other times for only the first three (in

[15] There are two small differences. First, both the 1983 and 1987 questionnaires did not ask for a response for 365 day expenditures on institutional medicine, a category that accounted for less than 1.5 percent of total expenditures in 1993/94; second, the 1987/88 survey did not ask for a response on 365 day expenditures on education, a category that accounted for about 2.5 percent of total expenditures in 1993/94.

1987/88). The number of categories for which questions were asked on a 365 day basis (3, 4 or 5) is used in the notation on how total per capita expenditures are designated.

Thus, there are at least eight different combinations that are possible, and in the short-space of seven years (1993-2000), the NSSO has tried out *six* of them. Table 3 reports on the various combinations of survey design that have been experimented with by the NSSO, and provides a useful glossary of complications inherent in any use of these data for inter-temporal comparisons.

The table indicates that there are several "sources" of non-comparability. First, note that food consumption was asked on a simultaneous 7 and 30 day basis in 1999/00 (and only in 1999/00). In other words, we have data on $Efood30_7$ and $Efood7_{30}$ in 1999/00 but not Efood30 as available in other years. It has also been recognized (especially see the important paper by Sundaram-Tendulkar (2003a), hereafter referred to as Sundaram-Tendulkar) that data for $Enonfood365_{30}$ are not available in 1999/00 but data for Enonfood365 are. Here, the conventional assumption has been to assume that Enonfood365 is *not* a biased estimate for $Enonfood365_{30}$. This assumption is likely to be incorrect for the mean, (likely to be biased downward) and may also be incorrect for the variance (effect is *a priori* ambiguous).

While undoubtedly such experimentation will, and eventually does, lead to a better sampling design, and while such experimentation makes a multitude of answers seem plausible, it is a bit unfortunate that such experimentation had to come at a time when major economic reforms occurred in India. We in India, and the world development community in general, need to know what happened, what worked, and what policy changes are needed. It is not the case that if the NSSO had not experimented we would know the answers. But it is the case that there would have been a more meaningful controversy. As the rest of the first part of this paper documents, the NSSO experiments have left the research community richer (in monetary terms), and the policy makers poorer (in terms of wisdom). The data experiments have allowed a multitude of interpretations to be both plausible and convincing – our objective in this paper is to help sift through the various "experiments with truth".

Table 3: Questionnaire Format: National Sample Survey 1983 - 2001

Year	Efood30	Efood7	$Efood30_7$	$Efood7_{30}$	Enfes30	Enonfood 30	Enonfood 365	Enonfood 30_{365}	Enonfood 365_{30}
1983	✓				✓			✓	✓
1987	✓				✓			✓	✓
1993	✓				✓			✓	✓
1999			✓		✓		✓		
1994 – 98*	✓	✓			✓	✓	✓		
2000 – 01*	✓				✓		✓		

URP	MRP	MRP_a	MRP_b	MRP_{99}	MRP_{00}
Efood30 + Enfes30 + $Enonfood30_{365}$	Efood30 + Enfes30 + $Enonfood365_{30}$	Efood30 + Enfes30 + Enonfood30	Efood7 + Enfes30 + Enonfood365	$Efood30_7$ + Enfes30 + Enonfood365	Efood30 + Enfes30 + Enonfood365

Notes:

Efood30 = Questions asked on consumption of food for 30 days.
Efood7 = Questions asked on consumption of food for 7 days.
$Efood30_7$ = Questions asked on consumption of food for 30 days and 7 days in the same questionnaire.
$Efood7_{30}$ = Questions asked on consumption of food for 7 days and 30 days in the same questionnaire.
Enfes30 = Questions asked on consumption of Non-Food (Fuel and Light, Consumption Services, Rent, Consumption tax, Medical Non-Institutional) items for 30 days
(These questions are asked in every survey).
Enonfood30 = Questions asked on consumption of Non-Food (Durables, Institutional Medical, Education, Clothing, Footwear).
Enonfood365 = Questions asked on consumption of Non-Food (Durables, Institutional Medical, Education, Clothing, Footwear) items for 365 days.
$Enonfood30_{365}$ = Questions asked on consumption of Non-Food ((Durables, Institutional Medical, Education, Clothing, Footwear) items for 30 days and 365 days in the
same questionnaire (i.e. 30 day questions when 365 days are also reported).
$Enonfood365_{30}$ = Questions asked on consumption of Non-Food ((Durables, Institutional Medical, Education, Clothing, Footwear) items for 365 days and 30 days in the
same questionnaire. (i.e. 365 day questions when 30 days are also reported).
MRP_a and MRP_b (Mixed Recall Period) are constructed from NSS for 1994-1998.
* = NSS Thin Samples collected from 1994-1998, and from 2000-01.

Section 3b: Definitions of per capita expenditures

An appreciation of the changes in the methods of estimating per capita expenditures can be gauged by noting the different combinations of the broadest monthly per capita totals that are possible for the different years.

For 1983:

$$MPCE4m' = Efood30 + Enfes30 + E4nonfood30_{365} + E1nonfood30$$

where E4 refers to the fact that there are four non-food categories for which data on both a 30 day recall and 365 day recall are available. The m (or y) at the end of MPCE4 is short-hand for the fact that when the Enonfood expenditures were asked on both a 30 and 365 day basis, only the m (monthly) or the y(annual) estimates were used in the computation of total per capita expenditure.

For 1987/88:

$$MPCE3m = Efood30 + Enfes30 + E3nonfood30_{365} + E2nonfood30$$

For 1993/94:

$$MPCE5m = Efood30 + Enfes30 + E5nonfood30_{365}$$

In the literature, the above methods of computing MPCE are referred to, somewhat misleadingly, as expenditures according to a uniform reference period or URP. In **1993/94**, another definition of MPCE is possible, this according to what is termed in the literature as the mixed reference period or MRP:

$$MPCE5y = Efood30 + Enfes30 + E5nonfood365_{30}$$

For **1999/00**, the questionnaire changed in a radical fashion; now the broadest estimate of MPCE that is possible is:

$MPCE5y' = Efood30_7 + Enfes30 + E5nonfood30$

Computation of MPCE5m, the variable that did exist in the years prior to and including 1993/94, is no longer possible and therefore comparison of growth rates, inequality, and poverty on the basis of URP (MPCE5m) are not possible for the 1990s. Hence, the 1980s and 1990s cannot be (strictly) compared with any NSSO CE survey.

Some brief conclusions that emerge from a quick review of the various MPCE combinations are as follows. First, that all survey years involve a mixed reference period for at least two broad expenditure items (hence the misnomer of URP for any definition of expenditures). Second, that since all inter-temporal comparisons center on the 1993/94 survey year (the years prior to it have been dubbed as the pre-reform years, and the years after 1993-94 have been classified as the reform years[16]) it is unfortunate that that year is *unique* in NSSO consumer expenditure survey history. The only post 1993-94 large sample survey that is available is 1999-2000 – and it turns out that this survey is *also* unique. It cannot be compared to 1993-94 or any other year.

Attempts, however, have to be made to gauge what happened in the nineties, and to compare such performance with what happened in the eighties. The major problem with the 1999/00 survey, and emphasized by all the research on the subject, is the "confounding" caused by the presence of simultaneous questions on expenditure of food on both a 7 day and 30 day recall basis i.e. there are *two* estimates on how much cereals a family consumed in 1999-00. One estimate is on what the family said it consumed on a recall of 7 days, and another estimate based on the recall of 30 days. The two need not be identical, and they are not. But the presence of an "easier to recall" 7 day expenditure may affect the estimate provided on the 30 day basis; in particular, it would not be unusual for the 30 day expenditure to be a simple multiple of the 7 day expenditure. As Sundaram-Tendulkar point out, some of this bias may have been reduced, if not eliminated, by the NSS instructing its field operations staff to ask the 30 day questions first. This happened when the survey operations were just a few weeks old i.e. what is plausible is that only 10 percent of the questionnaires contain a bias, and

[16] And neither classification is correct. GDP growth in India took off in the early eighties with minimal reforms, and major economic reforms were initiated in 1991-92.

the other 90 percent contain, at best, a severely reduced bias. In other words, Efood30 is likely to be close to $Efood30_7$.

Two adjustments are possible. Either the 1999/00 MPCE estimate can be adjusted to parallel the MPCE5m estimate of 1993/94 (the method preferred by Deaton-Dreze) or the MPCE5y estimate for the two years can be used, the method preferred by Sundaram-Tendulkar and Sen-Himanshu. Both have problems. The Deaton-Dreze MPCE5m estimate just does not exist for 1999/00; and the MPCE estimate that is possible to compute for 1999/00, MPCE5y', *looks* similar to MPCE5y (of 1993/94). But this "equivalence" involves one additional assumption: that $Enonfood365_{30}$ will yield the same answers as Enonfood365[17] (the former has a 30 day subscript, the latter does not). This assumption is tested below and the equivalence hypothesis strongly rejected. There is a plethora of evidence to suggest that E5nonfood30 is downwardly biased estimate of $E5nonfood365_{30.}$ Which means that if the bias caused by food is small (the finding of Sundaram-Tendulkar, and supported by our own computations), then MPCE5y' in 1999/00 is a downwardly biased estimate of MPCE5y in 1999/00 i.e. contrary to the assertions of many, the measured per capita consumption level in 1999/00 is an underestimate of the "true" level.

For the 1983-93/94 period, Sundaram-Tendulkar compare the MPCE5m in the two end-years. This also has problems because in 1983, questions on 365 day medical institutional expenditures were not asked. But the effect of this "omission" is likely to be minor. More importantly, the two periods cannot be directly compared because the congestion around the poverty line for MPCE5m would be different than the congestion around the poverty line for MPCE5y.

Section 3c: **Estimating the three biases in mean expenditures in 1999/00**

There are three possible biases, in *both* the mean and the variance (inequality) in the 1999/00 CE survey. There is the bias caused by the presence of 7 day questions on food (bias is towards higher expenditures and larger variance); bias caused by the shift to only 365 day questions on non-food items (bias towards mean expenditures

[17] This under the strict, but reasonable, assumption that the nature of questions in one section of the questionnaire does not affect the responses in another section.

uncertain; bias towards lower variance); and third, bias caused by the *absence* of 30 day questions on non-food items for which 365 day recall period questions were asked (bias towards lower mean expenditures; effect on variance is ambiguous).

Several methods have been offered to test for the magnitude of the (total) bias in per capita expenditures, inequality and poverty in the NSSO CE survey for 1999/00. The most prominent method, and likely to be the most accurate (or the least inaccurate), is the one by Deaton(2003a). He noted that Enfes30 (m goods in his paper) was the *only* group of expenditures whose definition had stayed the same for *all* the survey years. If the information contained in these expenditures (accounting for about 20 to 30 percent of total expenditures) could be suitably "blown up", then one could compare mean expenditures, poverty and inequality in 1999/00 with the earlier years.[18] It is important to recognize, and appreciate, the central role played by the Enfes30 goods in a person's expenditure pattern, and in Deaton's method. These items are in virtually every consumer's budget, the goods are heterogeneous (fuel and light, non-institutional medical services, toilet articles, rent, consumption of miscellaneous goods and services etc.), and they have a high income elasticity (about 1.2 for the sample as a whole during 1993/94, based on MPCE5y).

As Deaton states, use of Enfes30 expenditures is theoretically justified if the following two conditions hold. First, that the magnitude and distribution of Enfes30 expenditures are not affected by the presence/absence of questions in other non-Enfes30 parts of the questionnaire. Second, that the relationship between Enfes30 expenditures and total expenditures (strictly, the log of these expenditures) is the same in the year of adjustment (e.g. 1999/00) as the base year used for adjustment (e.g.1993/94). These are reasonable assumptions provided tastes and relative prices do not change. If these two assumptions hold, the relationship observed between total and Enfes30 expenditures can be *imposed* on the data for Enfes30 goods in other years. Data for

[18] Bhalla(2004a) makes the point that inter-temporal comparisons should not be between the actual estimates in a given year and the synthetic or adjusted estimates in another year i.e. since the adjusted estimates are derived by non-linear transformations, involving significant errors or error variances, they cannot be "equivalent" to unadjusted estimates. Comparing like with like means a comparison of adjusted with adjusted or unadjusted with unadjusted. Deaton-Dreze(2004) claim that their method of comparing unadjusted *variances* in 1993/94 to the transformed synthetic variances in 1999/00 is akin to obtaining temperature in centigrade in 1993/94 and transforming *exactly* the Fahrenheit temperature in 1999/00. That their Centigrade-Fahrenheit transformation can lead to counter-intuitive results is explored in Section 4.

Enfes30 are “correct” or reliable, unlike the data on food expenditures, and therefore unlike the data on total expenditures. Hence, a transformed estimate of the variable in question (mean, poverty, or inequality e.g. log variance) can yield *comparable* estimates for other years.

*Estimating Bias 1: the **mean** of (log) expenditures in 1999/00*

One form of the relationship between real total expenditures (MPCE5y or MPCE5m) and real Enfes30 expenditure is as follows (the prefix k indicates real) :

(6) $\text{Log}\ (\text{kMPCE5m})_t = á + â\ ^*(\text{log})\text{kEnfes30}_t + u_t$

This equation can be estimated for 1993/94, a base year, and one with ostensibly no “data contamination”. Actually, for the method it does not matter if the non Enfes30 data are contaminated or not – the adjusted data (on mean incomes, variance, and poverty decline) would correspond to whatever definition was employed for the dependent variable in the base year.

The estimates of á and â can then be used, along with the *uncontaminated* Enfes30 data for 1999/00, to obtain the mean kMPCE5y estimate for 1999/00[19] i.e.

(7) $\text{Log}\ (\text{kMPCE5m})_t = á_{t-1} + â_{t-1}{}^*(\text{log})\text{kEnfes30}_t$

If equation (6) is estimated separately for urban and rural areas (in real terms, using either of the two price deflators, PC or DT), one obtains estimates of mean per capita expenditure via equation (7) for 1999/00 and the growth in such expenditures can be derived. This is estimated to be (for kMPCE5y, PC deflator) 8.7 percent in rural areas, and 14.7 percent for urban areas. This compares with the observed “contaminated” growth rates for kMPCE5y of 5.8 percent and 9.4 percent for the two areas, respectively.

There are four different predictions of real growth between 1993/94 and 1999/00: two each for each consumption measure, MPCE5m and MPCE5y, and two each for each

[19] Since, data on the pattern of consumer expenditure in the 1999/00 E&U survey are not available (but Sundaram-Tendulkar had access to it for their computations), this exercise cannot be conducted using the Enfes30 data in the E&U 1999/00 survey.

deflator (PC and DT). Table 4 has the details. This table also contains the predicted or adjusted estimates reported by Deaton-Dreze. Their DT deflator based estimates are very close to ours e.g. for rural areas, we predict a cumulative six-year growth of 9.8 percent vs. their estimate of 8.7 percent; for urban India, the growth estimates are 15.8 and 16.6 percent, respectively[20]. The average bias (this bias exercise can only be done using the MPCE5y definition since data conforming to the MPCE5m definition were not collected in 1999/00) is either 3.5 percent (PC deflator) or 2.6 percent (DT deflator). Thus, the average bias is about **minus 3 percent** *i.e. the 1999/00 survey had a net downward bias in total expenditures.*

Table 4: The bias in real expenditure growth, 1993-94 to 1999-00

	MPCE5y		MPCE5m		
	PC deflator	**DT deflator**	**PC deflator**	**DT deflator**	**Deaton-Dreze DT deflator**
Rural					
Actual	5.8	7.06			
Predicted	8.7	9.2	9.2	9.8	8.7
Urban					
Actual	9.4	11.7			
Predicted	14.71	15.4	15.1	15.8	16.6
All India					
Actual	6.7	8.2			
Predicted	10.2	10.8	10.7	11.3	10.9

Source: NSS Consumer Expenditure Survey for years 1983, 1987-88, 1993-94 & 1999-00
Note:
1. MPCe5m is based on a 30 day recall period for selected non-food items, while MPCE5y is based on a 365 days recall period for these items. See Table 2 and text for exact definitions.
2. Predicted growth is obtained by estimating a standard log-log regression relating non-food expenditures (Enfes30) to total expenditures; see text for details.
3. Deaton-Dreze do not specify the exact method via which they estimate the predicted expenditures for 1999-00. numbers in the table taken form Angus and Dreze (2002), *Poverty and Inequality in India – A Re-examination*, Economic and Political Weekly, Sept 7, pp 3737.

[20] This figure is reported on page 3739, Deaton-Dreze(2002).

The belief to date has been that the 1999/00 CE survey was overstating the "truth" i.e. that the true unmeasured but comparable to 1993/94 expenditure mean was **lower** than the measured mean. Our above result may appear to be counter-intuitive if the assumption is made that the presence of 7 day recall for expenditures on food pushed up the estimate of 30 day food consumption in 1999-00. However, as pointed out earlier, operating in the opposite direction is the negative bias caused by the fact that the 365 day goods questions did not have the benefit of the questions on 30 day goods. The net bias turns out to be negative i.e. an understatement of expenditures in 1999/00 survey. And the close proximity of our mean expenditure growth results to those of Deaton-Dreze (who, in their discussions, only talk about the upward overstatement bias caused by 7 day food questions) also indicates that the latter also obtain a negative value of the net expenditure bias in 1999/00, contrary to the discussion contained in their paper.

The apparent contradiction in Deaton(2003a) and Deaton-Dreze is picked up by Sen-Himanshu who state:

" [The] Deaton's adjustment, which among other things aimed to correct the likely upward bias in 55th round food consumption estimates due to its 7-day questions, had in fact increased these further…..Deaton has also communicated that his method does unexpectedly involve an implicit upward revision of 55th round food expenditures. It is therefore now agreed that they did not gauge 55th round noncomparability fully. (p. 4248).

Because of this apparent contradiction, Sen-Himanshu also conclude that:

"Deaton's claim that poverty declined by 7 percentage points between rounds 50 and 55 stands falsified. His method fails the test above in urban areas of all 15 major states and in rural areas of all but four".

The reason Sen-Himanshu conclude that the Deaton method is wrong is because they have assumed, like others and perhaps even including Deaton, that the *only* bias affecting the estimate of mean expenditures was the upward bias caused by the 7 day recall questions on food; *given* this assumption, they are right, the Deaton method stands falsified and/or the Deaton method yields an unexpectedly high estimate for food expenditures. However, as shown below, the absence of 30 day questions for 365 day purchases has a larger downward bias; the *net* effect, empirically, is negative. Thus, the

Deaton method does not yield unexpected results and/or is “falsified”; it is just that Deaton, and Sen-Himanshu have not incorporated into their calculations all the biases present in the 1999/00 CE survey.

Additional confirmation of the presence and importance of the 30/365 day bias is provided by the Sundaram-Tendulkar result that there was likely a zero aggregate 7 day bias in food expenditures. If the food bias is zero, and there is a net understatement bias, then this means that the gross 30/365 day bias is the net bias in total expenditures. Thus, the most likely result is that the aggregate expenditure reported for 1999/00 was biased downward, perhaps by as much as 2 to 3 percentage points i.e. that *mean* growth between 1993/94 and 1999/00 was not close to 6.7 percent as revealed by official data on MPCE5y, but rather close to 10 percent; and for MPCE5m, somewhat closer to 11 percent.

Section 3d: Food expenditures in CE 1999/00 survey – biased downwards?

This sub-section contains two independent tests of whether food expenditures *per se* were biased upwards in 1999/00. As mentioned earlier, and it bears reiteration, this bias is likely to be zero if it is assumed that the field investigators heeded the advice of their bosses and asked the 30 day questions *first* . Nevertheless, it is important to investigate what other evidence suggests about the magnitude of this bias.

The first test of a bias in food expenditures in 1999/00 is to compare such expenditures with the 2000/01 survey, a year in which there was no food bias. If the 1999/00 food expenditures were indeed biased upwards then we should expect a spike down in 2000/01. Real food expenditures are reported in Table 5, along with the industrial workers food price index. Real food expenditures in 1999/00 (allegedly biased upwards) are the same as in the following “unbiased” year 2000/01; they are also the same as in six years earlier in 1993-94, and the same as in twelve years earlier in 1987-88! If per capita food expenditures have stayed roughly constant, and if the 1999/00 observed food expenditures are the same as in 1987/88 and 2000/01, there is little reason to believe that the 1999/00 expenditures were biased upwards in other than a most marginal manner. Thus, this simple exercise supports the conclusion that there was no *aggregate* bias in food expenditures in 1999/00[21].

The second test is that conducted by Sundaram-Tendulkar. They compare food and other expenditures in the CE survey with analogous expenditures in the E&U survey. For non-food items they find the under-reporting in the E&U survey (the one not subject to 7 day bias) to be of the same order of magnitude – about 10 percent – as the under-reporting of food items. From this they (reasonably) conclude that it was unlikely that there was any aggregate food bias in the CE survey.

Table 5: Real food consumption, 1983-2002

Year	NSS Food Expenditure	Food Price Index	NSS Real Food Expenditure
1983	78.8	117	300.4
1987	109.5	152	321.3
1993	195.2	272	320.1
1999-00	319	446	319.0
2000-01	324	453	319.0
2001-02	321	466	307.2

Source: RBI Handbook of Statistics, Unit-level NSS data for the stated years
Note: Food price index is the food price component of the Industrial workers; 1982 = 100

[21] As the next sub-section documents, there is reason to believe that the *distribution* of observed food expenditures in 1999/00 was biased towards greater inequality.

Section 3e: The collapse in the elasticity of nonfood365 goods, 1993-1999/00

This section documents the basis for the result that Enonfood365 expenditures reported for 1999/00 are likely to be lower than the actual, but unobserved, expenditures for these items. These goods consist of the following five categories: clothing, footwear, consumer durables, education and non-institutional medical expenditures. The expenditure on these items (prior to and including 1993/94) were reported on 30 day basis, while the 365 day questions were also asked in the same schedule. Hence, we can observe the pattern expenditure elasticity for these items for the eighties. Table 6 estimate the expenditure elasticity of these various goods (with respect to Enfes30 goods) for the 1980s and 1990s. Only for consumer durables (items which account for less than 20 percent of Enonfood) does the elasticity increase for the period 1990s, and that also only marginally (1.13 to 1.26). For all other items, there is a steep decline. For example, for clothing (which accounted for 17 percent of Enonfood365 in 1993/94), the elasticity declines from a positive 0.95 to a *negative* 0.18. In other words, real per capita clothing expenditures declined over the period 1993/94 to 1999/00. For footwear, the elasticity declines from 3.4 to only 0.74. For education expenditures, the decline is from an elasticity of 1.62 to an elasticity of only 1.43. (The 30 day expenditure elasticity in the 1980s for education was a much higher 2.92).

These non-food expenditure elasticities should not be declining; a neutral assumption is that such elasticities should be remaining constant. A simple test of our maintained hypothesis of bias in expenditure growth of Enonfood365 items is as follows. Assume the 1983-93 or 1987-93 elasticity for each individual item, and then *predict* expenditures on such items for the 1993/94-1999/00 period. These calculations (see Table 6b) indicate that on an aggregate such expenditures are predicted to be about 16 percent higher than reported (i.e. estimated expenditure in 1999/00 is Rs. 55.3 vs. actual expenditure of Rs. 47.3; total measured expenditure in 1999/00 was Rs. 578).
Given the weight in total consumption of such items of about 10 percent, this implies that per capita expenditure (MPCE5y) was under-estimated by 1.5 percent in the 1999/00 CE survey i.e. almost an identical result as one obtained earlier by a very different method.

Explaining education expenditure differences in the 1999/00 CE and E&U surveys.
In their detailed comparison of the two consumption estimates available for 1999/00 (CE and E&U surveys) Sundaram-Tendulkar note an interesting anomaly: for practically all

items, the CE estimate is higher, on average by about 10 percent. This, they convincingly argue, is due to truncation bias, since the E&U survey had a substantially

Table 6a: Elasticity of 365 day goods consumption with respect to consumption of monthly goods (*Enonfood365 on Enfes30*)

All India	1987-88 to 1993-94	1993-94 to 1999-00
Planning Commission price deflator		
Clothing	0.95	-0.18
Footwear	3.40	0.74
Durables	1.13	1.26
Education*	1.62	1.43
Medical (Institutional)		3.93
Enfes30 (Monthly goods)	1.00	1.00
Deaton - Tarozzi price deflator		
Clothing	0.92	-0.04
Footwear	2.50	0.73
Durables	1.12	1.23
Education	1.56	1.33
Medical (Institutional)		3.53
Enfes30 (Monthly goods)	1.00	1.00

Source: NSS Consumer Expenditure Survey for years 1983, 1987-88, 1993-94 & 1999-00.
Note: For definition of goods Enfes30 and Enonfood365, see Table 2, and text.

Note:

1. Elasticities are on the basis of *365 day recall*; Since *365 day recall* expenditure on education is not available in 1987/88, 1983 estimates elasticities are taken.
2. *Elasticity of education on the basis of *30 day recall* with respect to Enfes30 is *2.92* between 1987/88 to 1993/94.

Elasticity for *Durable* goods with Planning Commission deflator are 1.46, 1.13 & -0.67 for period 1983-87, 1983-93 and 1987-93 respectively. Same for Deaton-Tarozzi are 1.47, 1.12 & -0.17 for period 1983-87, 1983-93 and 1987-93 respectively.

reduced list of questions (about 33 compared with more than 330 questions in the CE survey).

One item, however, shows a more than 30 percent *higher* consumption in the truncated E&U survey – expenditures on education. In a footnote they explain why this anomaly might occur:

"Unlike in the CES, two components, namely, tuition fees and, newspapers, magazines, etc., have a 30-day reference period in the EUS, whereas they – along with school books and other educational articles – are all canvassed with a 365-day reference period in the CES. This could be a factor in explaining why the EUS estimates exceed the CES estimates". (Sundaram Tendulkar, p. 337, fn. 5)

The education anomaly can be explained by noting that tuition expenditures etc. are more in the nature of monthly expenditures. If the elasticity of 2.92 for education, observed for 1987/88 to 1993/94 for 30 day education expenditures is imposed on the 365 day education expenditure data observed for 1993/94 (Rs. 7.8) then the projected value for 30 day education expenditures in 1999/00 is Rs. 26.9 per capita per month. However, the CE reports a much lower (365 day) education expenditure level of Rs. 16.3. In contrast, per capita 30 day education expenditures according to the *1999/00 E&U survey*, are Rs. 13.91 and Rs. 55.83[22], rural and urban areas, or an average of Rs. 24.4, nearly **match** the predicted expenditures obtained from the elasticity model (indeed, the E&U expenditures are less by about 10 percent, the average truncation bias!). This evidence on education expenditure confirms, in a rather striking manner, our maintained hypothesis that Enfood365 estimates were under-reported in 1999/00.

Table: Bias due to absence of 30 days recall for durable goods

	Elasticity	**Reported value in 1993**	**Reported value in 1999**	**Predicted value in1999**
Clothing	0.95	22.2	21.5	26.9
Footwear	3.40	3.1	3.6	6.1
Durables	1.13	7.2	9.3	9.0
Education	1.62	6.3	8.4	8.8
Medical (Institution)		2.1	4.5	4.5
Total		**40.9**	**47.3**	**55.3**

Bias: 1.4 percent of total expenditure in 1999

Source: NSSO Consumer Expenditure Surveys for years 1983, 1987-88, 1993-94 and 1999-00.

[22] From p.330-331, Tables 3R and 3U, Sundaram-Tendulkar(2003).

Section 4a: What happened to inequality in the 1990s: NSSO CE surveys.

The previous section explored the nature of the biases in the estimate for **mean** expenditures in the 1999/00 survey. It was observed that, most likely, the mean estimate of per capita expenditures was downwardly biased i.e. the "true" figure was likely to be 2 to 3 percent higher. Regardless of the direction of bias in the estimate for the *mean*, the bias in inequality estimates could be very different. The conventional, almost universal, wisdom, we are told is that inequality worsened *significantly* in the nineties. This section reviews the various attempts at estimating inequality change in India in the 1990s, interprets the internal consistency of the different methods, and concludes about can be reasonably interpreted about "what happened to inequality in the 1990s".

Inequality trends according to official data, 1983-1999

Before examining the various proposed adjustments, it is of some interest to examine what the official data actually reveals about inequality in the different years. Prior to and including the NSSO CE 1993/94 survey, inequality in India had been "officially" reported to be based on MPCE5m i.e. all the three major expenditures (Efood30 and Enfes30 and Enonfood30_{365}) reported as 30 day expenditures. It was noted in Section 3 that the definitions of per capita expenditure changed, and that for comparing inequality (MPCE5y') in 1999/00, the closest comparable figure for inequality was MPCE5y in 1993/94. And for comparing inequality prior to 1993/94, data for another definition of expenditure, MPCE5m, were available.

Regarding trends in inequality, it is more appropriate to consider changes in real inequality (i.e. per capita expenditures deflated by an appropriate price index) rather than changes in nominal inequality (expenditures not deflated by any price deflator). The latter has been used to estimate inequality change by several authors. Such a calculation has yielded misleading estimates for inequality change for years when there was no problem in definitions e.g. 1983 to 1993/94, when nominal inequality is assumed to be constant but there is a decline in real inequality. Our estimates of adjusted inequality for 1999/00 (including reproduction of the Deaton-Dreze method) suggest that the change in estimated real inequality during the 1990s is a function of the price deflator used e.g. for the Planning Commission deflator, there is less than a 5 percent increase in the variance of logs, for the nominal expenditure series, there is a 10 percent increase in inequality, as estimated by Deaton-Dreze. Table 7 presents estimates of inequality for real inequality (using the official and commonly used PC poverty deflator) and for different definitions of per capita expenditure. In addition to the “5” and complete category of expenditures (MPCE5y and MPCE5m), inequality trends for MPCE3y and MPCE4y are also reported. These definitions either exclude 365 day medical institutional expenditures (MPCE4y) or exclude 365 day education expenditures as well (MPCE3y). These two ”low frequency” items accounted for less than 2.5 percent of total expenditures in 1993/94. This is the disadvantage of using definitions like MPCE3y i.e. that it does not include 100 percent of expenditures. This is counter-balanced by the fact that it is the only definition for which data are available for *all* the large sample survey years, in the eighties and nineties. Hence, a time trend of MPCE3y inequality could be illustrative of the overall trend.[23]

The trend for real MPCE3y inequality is as follows: an 8.7 (log) percent decline between 1983 and 1993/94, and a 2.9 percent increase between 1993/94 and 1999/00. The latter is for the Gini; for variance of logs, the 1990s increase in inequality is even less: 2.1 percent. Yet another comparison (strictly non-comparable but both Sundaram-Tendulkar and Sen-Himanshu employ it) is that between the change in inequality according to MPCE5m (pre-reform decade), and the change in inequality as indicated by MPCE5y in the 1990s. This indicates a 6 percent improvement in the Gini followed by a 4 percent deterioration.

This pattern is repeated (magnitudes are marginally different from above) for nominal and real per capita consumption according to the DT price deflator. Thus, most definitions of consumer expenditure indicate that inequality in India traversed a *very mild* V shaped pattern, with the left leg higher in magnitude than the right leg. The worst case scenario, according to definitions considered comparable, is that the worsening of inequality from 1993/94-1999/00 was a very mild affair; a change of 2-4 percent (not percentage points) or so in the Gini is mild, and as emphasized earlier, is larger than the variation in inequality observed due to different definitions in any given year.

[23] This is subject to the caveat that in 1999/00 mean growth in three expenditures – clothing, footwear and durables - is likely to be underestimated; statements about inequality change, therefore, remain ambiguous.

Table 7: Real Inequality in India, 1983-99; Planning Commission poverty line price deflator

	1983	1993-94	1999-00
Rural			
Gini			
MPCE3y	.2839	.2537	.2531
MPCE4y	.2849	.2558	.2563
MPCE5y		25.8	.2601
MPCE5m	.3117	.2856	
Variance of logs			
MPCE3y	0.2493	0.1928	0.1906
MPCE4y	0.2510	0.1958	0.1951
MPCE5y		0.1979	0.2003
MPCE5m	0.2900	0.2339	
Urban			
Gini			
MPCE3y	.3166	.3102	.3339
MPCE4y	.3191	.3175	.3407
MPCE5y		.3190	.3426
MPCE5m	.3400	.3413	
Variance of logs			
MPCE3y	0.3033	0.2904	0.3104
MPCE4y	0.3089	0.3043	0.3256
MPCE5y		0.3064	0.3292
MPCE5m	0.3423	0.3424	
All India			
Gini			
MPCE3y	.2926	.2706	.2790
MPCE4y	.2942	.2750	.2845
MPCE5y		.2766	.2876
MPCE5m	.3191	.3030	
Variance of logs			
MPCE3y	0.2623	0.2180	0.2232
MPCE4y	0.2653	0.2244	0.2314
MPCE5y		0.2266	0.2361
MPCE5m	0.3026	0.2626	

Source: NSSO Consumer Expenditure Survey for years 1983, 1993-94 & 1999-00.

Notes: MPCE3y is estimated as the sum of food expenditures, Enfes30 expenditures and 365 day based expenditures on three items: clothing, footwear and durables. MPCE4y includes education. MPCe5m is based on a 30 day recall period for selected non-food items, while MPCE5y is based on a 365 days recall period for these items. See Table 2 and text for exact definitions.

The above computations involve use of 1999/00 CE data, and while the definitions are comparable, the 1999/00 data are still subject to the biases in the mean, and in the variance. In particular, as shown below, measured inequality in the 1999/00 CE data is biased *upward*. In addition, the mis-measurement of 30 day / 365 day questions for clothing, footwear and consumer durables may also have led to a positive bias in inequality.

What bias does the 7 and 30 food day recall period do to the estimate of ***inequality****?*
It most likely biases it upwards. Sundaram-Tendulkar compute the percentage excess of food expenditures for each fractile (each 5 percent) of the population. Their results, by fractile levels, are reproduced in Table 8. For rural, urban and (derived) all-India, the percentage excess increases with the expenditure percentile level i.e. the richer the family, the more is the percentage excess of its CE food expenditures. There are two factors causing this “excess”: the abridgement of the questionnaire in the E&U survey, and the presence of the 7 day questions in the CE survey (7 day questions on food were not asked in the E&U survey). Separating out the two effects is not straightforward, though the data shown in the previous section did indicate that *mean* food expenditures in 1999/00 were not overstated.

Sundaram and Tendulkar conclude that “the observed differences between the CES and EUS estimates overwhelmingly reflect the impact of the abridged schedule in the EUS.” There is an implication of their results which they do not state but, given the above quote, is perhaps implicit in their conclusions. This is that the presence of the 7 day questions biased the CE survey towards showing greater inequality. Note the larger difference in the CE/E&U food expenditure ratio for the richer fractiles.

That food expenditures in the 1999/00 CE survey are indeed biased towards greater inequality can also be illustrated as follows. Assume that there are two sources of bias contained in the percentage differences reported in the table: the first is the abridgement bias in the E&U schedule which leads to such expenditures being lower by an average of 10 percent in the E&U survey. The second is the 7 day bias which leads to the variance in food consumption being higher in the CE survey. What Sundaram-Tendulkar conclude is that there is zero *aggregate* 7 day bias in the CE survey, a conclusion also reached by us in the previous section. If the assumption is made that the truncation bias is approximately the same for all households, and that the *aggregate* 7 day bias is close to zero, then the CE survey food expenditures are biased towards greater inequality. Thus, the reported Gini increase in MPCE5y from 30.1 to 32.0 in 1999/00, or from 27.7 to 28.8 (in real terms, PC price deflator) is an over-estimate of the “true” change in inequality. Thus, an increase in inequality in the 1990s, albeit small, should not be construed as a “real” happening, given that the 1999/00 measured inequality is likely to have been biased upward.

Only very strange, and unusual, combinations of the understatement and 7 day bias can lead to the result that the 1999/00 consumption distribution was not biased towards greater inequality. One such assumption is that the poor have lower abridgement bias and higher understatement bias.

One estimate of the magnitude of the inequality bias is through the use of the over-estimation percentages reported in Table 8. When imposed on the data (e.g. each fractile under-estimates food expenditures by the same, aggregate percentage), inequality is estimated to be 2-3 percent lower for the (nominal) Gini, and almost 5-10 percent lower for the (nominal) variance of logs measure of inequality. In other words, adjusting for the inequality bias in 1999/00 indicates virtually no change in inequality in the 1999/00 CE survey relative to the 1993/94 CE survey.

Table 8: Difference in expenditure of Consumer Expenditure & Employment Unemployment Survey

Per capita consumption fractile	% Extra food consumption Rural	% Extra food consumption Urban	% Extra food consumption All India
1	6.2	10.6	6.5
2	6.2	9.5	6.5
3	6.6	9.8	7.4
4	6.8	10.5	6.6
5	6.4	11.7	7.2
6	6.9	13.1	6.9
7	6.4	12.8	7.7
8	7.2	12.0	8.9
9	8.3	11.8	9.2
10	8.8	11.9	9.0
11	8.6	12.0	10.1
12	8.7	13.1	10.4
13	9.4	13.6	10.0
14	9.6	13.9	11.3
15	9.6	14.1	10.7
16	10.9	14.6	11.2
17	10.4	14.6	12.6
18	11.2	14.3	12.6
19	11.7	14.3	14.2
20	14.8	14.3	15.1
Total	**8.7**	**12.6**	**9.7**

Source: Sundaram, K, and Tendulkar, Suresh D. (2003), *Poverty has declined in the 1990s: A Resolution of Comparability Problems in NSS Consumer Expenditure Data*, Economic and Political weekly, Jan, pp-332.
Note: Each column represents the excess (in percent) of the CE expenditure for each fractile over the corresponding fractile expenditure in the NSSO E & U survey.

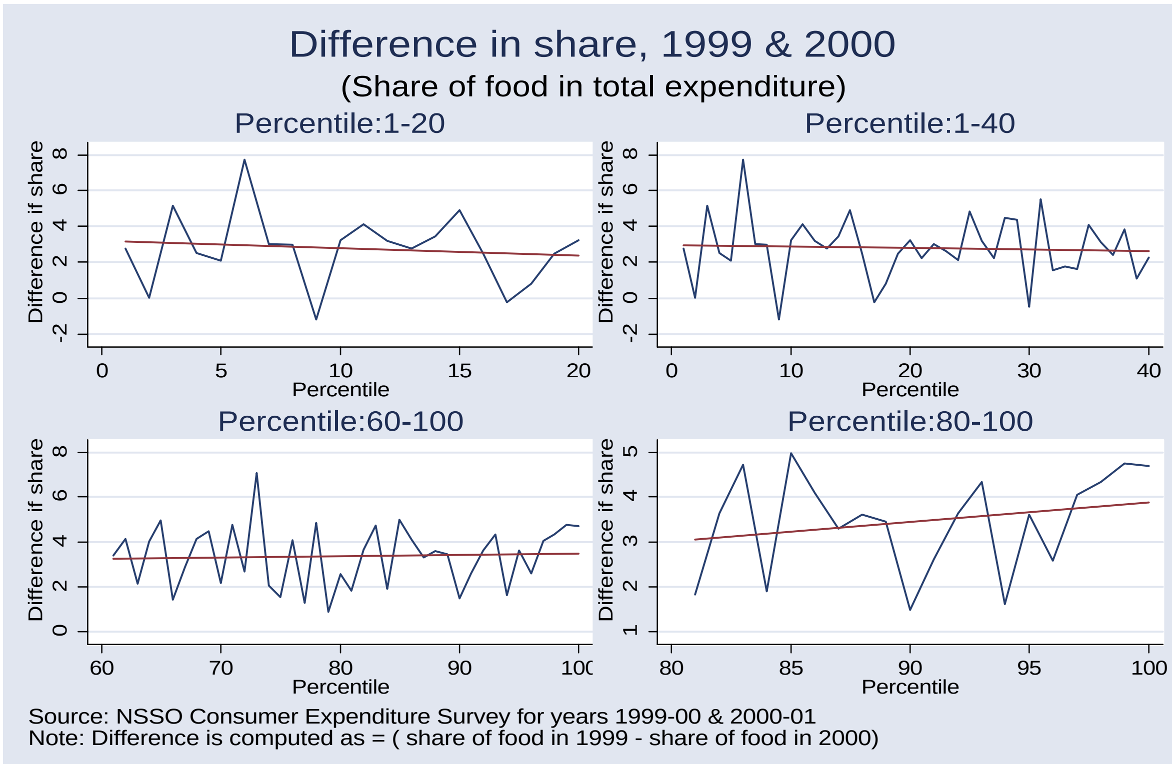
There is one additional estimate of the effect of 7 day questions on measured inequality. This method uses the food consumption pattern from the 2000/01 CE survey to identify the inequality bias in the 1999/00 survey. The former is free from the 7-day recall contamination (such questions were not asked); thus, it's food share can be considered as the "correct" food share. Food shares for quintiles of population are unlikely to change in 1 year. Therefore the *difference* between the 1999/00 observed food share and the 2000/01 food share can yield an estimate of the bias e.g. if the former is higher, that would mean a positive bias.

The graphs (Chart 3) plot the differences in food shares (between 1999/00 and 2000/01) for different quintiles of the population. The poorest quintile shows a downward trend in relative food shares, as does the second quintile. This implies that the "incorrect" food share for 1999/00 is converging towards the "correct" food share of 2000/01. But this difference shows an upward trend for the top 2 quintiles; the trend is even steeper for the top quintile. Thus, it is apparent that in the 1999/00 survey, food shares for the rich were higher than they "should" have been, but not so for the poor. In other words, the 7-day bias had a greater impact on the food consumption estimate for the rich; i.e. measured inequality in 1999/00 was upwardly biased.

What bias does the absence of 30 day questions for 365 day nonfood expenditures do to the estimate of the change in inequality ?

There remains the question of how much bias may have been caused by the fact that some non-food expenditures were asked in 1999/00 on only a 365 day basis, rather than on both a 30 and 365 day basis. The elasticities and observed growth rates for these five items, reported in Table 6, suggest that the direction of the inequality bias is inconclusive. The data on clothing, footwear, and durables would suggest that the 1999/00 CE survey inequality is biased upward; education and institutional medicine expenditure pattern would suggest a bias towards less inequality. We are presently engaged in research to identify the net magnitude and direction of these biases.

Alternative estimate of 7-day food bias on inequality



Section 4b: Additional estimates for inequality in 1999/00 – using the E&U survey

There is yet another measure of consumption inequality available for 1999/00: per capita consumption inequality as revealed by the NSSO E&U survey. Sundaram-Tendulkar who extensively use these data, do not report the inequality estimates (either rural, urban or all India) revealed by the E&U survey for 1999/00. This is unfortunate since these data are *not* contaminated by the presence of the 7 day questions on food. The drawback to the use of these data is that the consumption schedule was an abbreviated one, containing only 33 items of expenditure, rather than more than 330 items of expenditure contained in the CE survey questionnaire.

The E&U survey has a truncation bias, which affects measured inequality, and a lack of a 7 day bias, which also affects measured inequality relative to the 1999/00 survey. The latter has no truncation bias, but a definite 7 day bias. The net resolution of the two biases in the E&U survey can only be identified under certain assumptions. If it is assumed that the truncation bias is neutral, then the E&U Which way do the two "biases" go? If the assumption that the truncation bias is greater for the rich is accepted, then the E&U inequality estimate will be a lower bound to inequality change, and the 1999/00 measured inequality will represent the upper bound. Table 9 reports on two different estimates of inequality (variance of logs and Gini) and three different indicators of consumption (MPCE5y' nominal, and MPCE5y' real according to both the PC and the Deaton-Tarozzi price deflators). The results are also reported for the rural and urban areas.

The results are striking in affirming that *there was virtually no increase in all-India inequality between 1993/94 and 1999/00*. All 1999/00 inequality estimates are within 1 to 2 percent of the 1993/94 levels. Rural inequality is observed to have declined in the 1990s, a result which continues the trend of the 1980s[24]. Urban inequality, however, does show a significant increase, a result also broadly in line with the earlier 1980s trend, and a result consistent with what other scholars have obtained.

These inequality results do not employ any adjustments, nor any extra assumptions. The only assumption made – that the 1999/00 E&U survey is "representative" – is the one

[24] As shown later, the poverty decline observed by Deaton-Dreze for 1993/94 to 1999/00 is indicative of no change in rural inequality.

made by most researchers, especially Sen-Himanshu. The latter make the assumption that the 1999/00 E&US survey is representative for both the mean and (implicitly) the distribution. Sundaram-Tendulkar believe that the CE mean is "uniformly" overstated implying therefore that the E&U mean is "uniformly" understated. So our assumptions are clearly in the spirit of the assumptions made by other researchers involved in searching for the truth on inequality levels, and change, in India.

The next section examines, in some detail, what the different estimates of inequality change in the 1990s have been offered in the literature. Table 9, by indicating the likely upper and lower bounds[25], provides a perspective on the results of the different authors. These bounds indicate that aggregate inequality in India changed by as little as 0 and as much as 2 Gini or variance of logs points. In percentage changes, the *maximum* expected change for the Gini is 6.5 percent, and for the variance of logs, 8 percent. The average expected change is considerably lower, and there are arguments, and data, to suggest that an actual zero inequality change is not unlikely.

[25] The lower bound is of no change, but as shown by Bhalla(2003) and discussed later in this paper, adjusted inequality (if household survey expenditures are matched to national account expenditures) shows a trend decline in inequality continuing from the 1980s.

Table 9: Comparison of Inequality between 1993-94, 1999-00(CE) & 1999-00(EU))

	1993-94	Upper Bound 1999-00 CE Survey	Lower Bound 1999-00 E& U Survey
All India			
(Log) Variance			
Nominal	0.26	0.28	0.27
Real, PC deflator	0.23	0.24	0.23
Real, Deaton-Tarozzi, Deflator	0.24	0.25	0.24
Gini			
Nominal	0.30	0.32	0.31
Real, PC deflator	0.28	0.29	0.28
Real, Deaton-Tarozzi, Deflator	0.28	0.29	0.29
Rural			
(Log) Variance			
Nominal	0.20	0.20	0.19
Real, PC deflator	0.20	0.20	0.19
Real, Deaton-Tarozzi, Deflator	0.19	0.19	0.19
Gini			
Nominal	0.26	0.26	0.25
Real, PC deflator	0.26	0.26	0.25
Real, Deaton-Tarozzi, Deflator	0.25	0.26	0.25
Urban			
(Log) Variance			
Nominal	0.31	0.34	0.32
Real, PC deflator	0.31	0.33	0.32
Real, Deaton-Tarozzi, Deflator	0.30	0.32	0.31
Gini			
Nominal	0.32	0.35	0.33
Real, PC deflator	0.32	0.34	0.33
Real, Deaton-Tarozzi, Deflator	0.32	0.34	0.33

Source: NSS Consumer expenditure and Employment Unemployment Survey for years 1993-94, 1999-00.

Note: Per capita consumption used here is based on a 365 days recall period for Selected items (clothing, footwear, durables etc.).

Section 4c: Exploiting alternative information for trends in inequality

In addition to direct estimates of inequality, derived from unit-level data, inequality trends can also be gleaned from "macro" variable. Two of the most popular are the trend in the ratio of urban/rural expenditures and the presence/absence of "convergence" in state level data. These two are incomplete indicators and often yield only partial, and erroneous trends. A third indicator is the growth in wages of different sectors of the population. This variable is potentially a very reliable indicator of inequality trends; hence, wage trends are examined in detail in Section 5.

Several authors have used the first two indirect indicators as important inequality trend variables in their own words. For example, Deaton-Dreze state:

….we argue that there has been a marked increase in inequality in the nineties, in several forms. First, there has been strong 'divergence' of per capita expenditure across states, with the already better off states (particularly in the southern and western regions) growing more rapidly than the poorer states. Second, rural-urban disparities of per capita expenditure have risen (p. 3729)

And Sen-Himanshu state:

One aspect of this is the urban-rural divide. Chart 5b shows that the growth of nominal urban MPCE outstripped the rural one throughout, and that the differential rose well above the 1980s trend from 1999-2000. A more significant trend break, shown in Chart 5c, occurred in the early 1990s and involved underlying deflators which caused the urban-rural gap to increase even more sharply when calculated using real MPCEs implicit in poverty calculations. (p4365)

While much advocated, and used, the indirect indicators come with a host of problems. Take the increase in the ratio of urban-rural expenditures (URR). When URR increases, an important pro-poor and pro-equality effect also occurs. The incomes of the *poor in urban areas* may or does grow faster than the incomes of the *non-poor or rich in the rural areas*. Or take the much talked about divergence in growth rates of different states (Ahluwalia, Deaton-Dreze, Bhalla(2004a)). The same kind of above reasoning applies. In addition, the rich states can and do change places e.g. Punjab which was a fast grower in the 1980s, and a slow grower in the 1990s. Or take Rajasthan, which was a poor BIMARU state earlier, but now has left that camp. In addition, if consumption is being talked about (the basis for poverty calculations) then labor migration can, and does, mitigate some of the ill-effects of slow growth in poorer states e.g. Bihar, where the consumption profile (because of remittances) may be considerably more pro-poor than an income profile.

Ultimately, it is tricky, and an empirical issue. The real problem is caused by aggregation – what seems plausible at a small micro level may not hold up with aggregation. This is well illustrated by the inequality experience of the formerly Soviet economies. With the fall of the Berlin Wall, Russia became several countries rather than one large country. Inequality increased by very large proportions in a very short period of time. The inequality changes are an illustration of an apparent paradox i.e. overall inequality may not change much, even if inequality within each component (country) worsens substantially.

Table 10 (from Bhalla(2004a, Table 4.6, pg. 67) documents this phenomenon. For most countries, the change in inequality was over 20 percent; for several, inequality worsened by over 30 percent. Yet the overall Gini barely budged – from 41.2 just prior to the break-up to 44.0 just after. In (log) change terms, this represented an increase of only 6.8 percent! Massive upheaval in individual countries, genuine increase in inequality, yet the aggregate increase in inequality is only 6.8 percent. Considerably less than that estimated by Deaton-Dreze and claimed by Sen-Himanshu to have occurred in India.

What this illustrates is that for a large country, overall aggregate inequality can show very little change, despite a considerable amount of churning underneath. Massive structural change can occur, as it did in the formerly Soviet Union countries, yet measured inequality may not change much. So if the finding of little inequality change in India is observed, it should not be inferred that there is little change in the economy, or little change for specific groups of individuals. .

Table 10: Country inequality substantially worsens but group inequality stays relatively constant

	Population	Growth (1980-00)	First Survey Year Gini	Last Survey Year Gini	% Change in Gini
Armenia	3.8	-2.36	39.4	66.0	51.6
Bulgaria	8.3	0.41	25.0	34.1	31.0
Belarus	10.2	0.33	22.8	28.8	23.4
Czech Republic	10.3	0.58	20.7	26.6	25.1
Estonia	1.4	0.47	23.0	37.6	49.2
Hungary	10.1	1.20	21.0	25.3	18.6
Kyrgyz Republic	4.5	-2.28	26.0	53.7	72.5
Lithuania	3.7	-0.80	22.5	35.7	46.2
Latvia	2.4	-0.58	22.5	32.1	35.5
Moldova	4.3	-4.15	24.1	40.6	52.2
Poland	38.7	1.61	24.9	32.7	27.3
Romania	22.7	-0.98	23.4	28.7	20.4
Russia	146.9	-1.34	24.6	37.4	41.9
Slovak Republic	5.3	0.78	23.6	29.2	21.3
Slovenia	2.0	1.15	25.9	28.4	9.2
Turkmenistan	4.2	-2.96	26.4	35.8	30.5
Ukraine	52.2	-3.86	23.3	25.7	9.8
Uzbekistan	21.9	-1.34	25.0	33.3	28.7
Eastern Europe			**41.2**	**44.0**	**6.8**

The urban/rural expenditure ratio

Table 11 reports the time-trend for three indicators of the urban/rural (UR) ratio : nominal (MPCE5m and MPCE5y) and real (PC and DT deflators). If the UR ratio was a good indicator, then it should be correlated with inequality changes. But it is not. For the MPCE5m definition, nominal inequality remained unchanged between 1983 and 1993/94; but the nominal UR increased by about 5 percent. Real inequality (PC deflator) improved by about 6 percent, and the UR ratio increased by 5 percent. Real inequality (DT deflator) improved by 3 percent, and the UR ratio stayed flat.

The same "noise" pattern is revealed by the UR ratio for the MPCE5y definition in the 1990s. For these years, both nominal UR ratios, and the Gini, go in the same direction; hence possibly the wrong inference that the two are expected to move in tandem. For the PC deflator, there is a decline in the UR ratio from 1999 to 2001, yet the Gini shows an increase; ditto the case with the DT deflator. The net conclusion: it is difficult to glean much information about aggregate inequality from noisy UR ratio of expenditures.

Table 11: Time trends in Urban Rural expenditure ratio and Gini

	Nominal		**Real, PC deflator**		**Deaton deflator**	
Year	**Ratio**	**Gini**	**Ratio**	**Gini**	**Ratio**	**Gini**
1983	146.2	.325	110.0	.319	139.0	.325
1987	155.8	.329	107.8	.313	138.4	.321
1993	162.8	.325	116.8	.303	140.9	.310
1993	162.5	.301	116.9	.277	140.9	.284
1999	175.9	.320	124.8	.288	151.0	.294
2000	184.8	.328	123.4	.292	149.8	.295
2001	187.2	.338	119.6	.301	147.5	.300

Source: NSSO Consumer Expenditure Survey for years 1983, 1987-88, 1993-94 & 1999-00.
Notes: 1. The Gini for 1983 to 1993/94 is for the MPCE5m definition; for 1993 through 2002 the MPCE5y definition is being used.

Inter-state disparities in per capita consumption

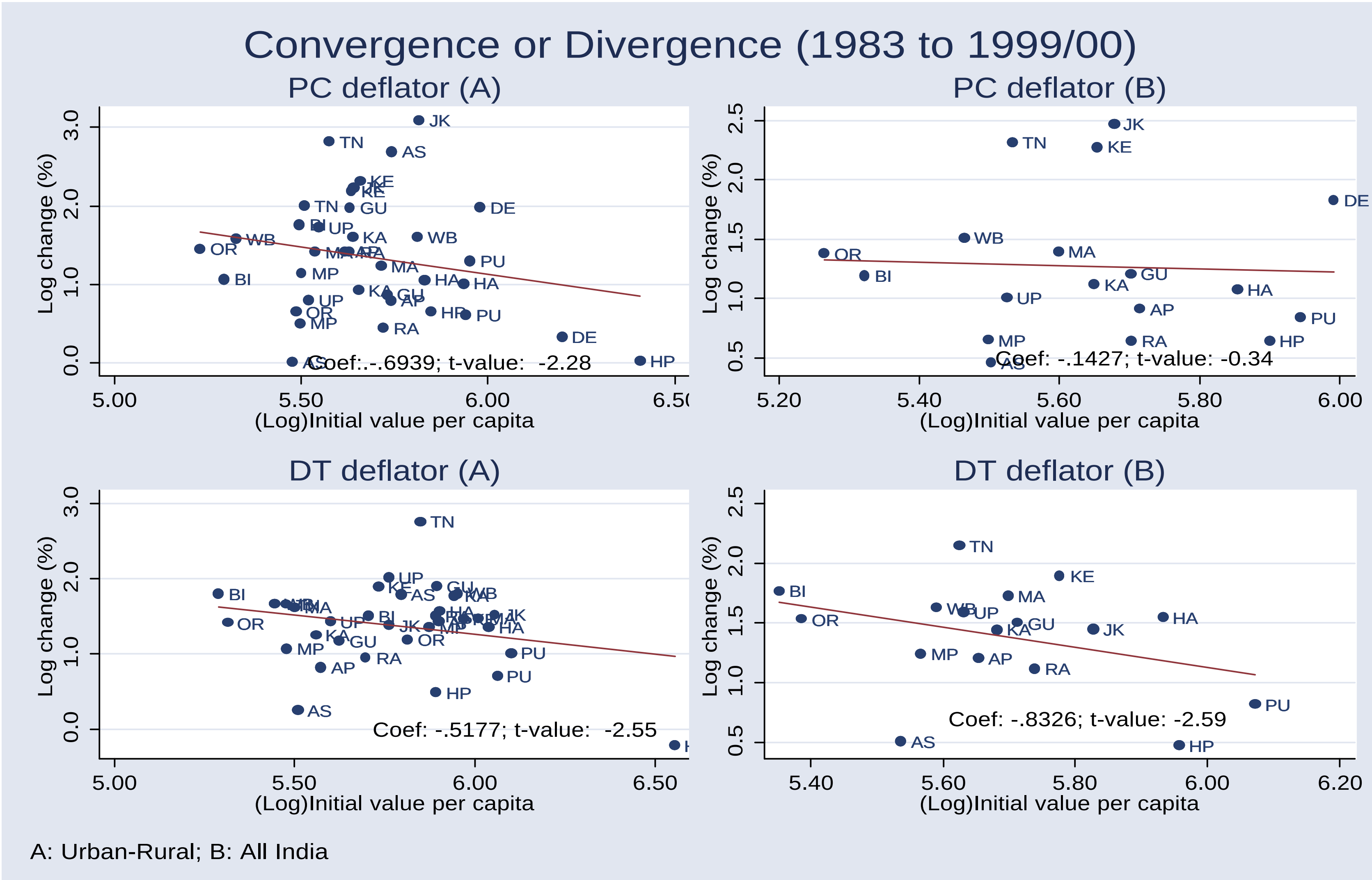
Another indirect indicator of inequality change is given by the convergence/divergence of state level per capita incomes. If national accounts (i.e. state level GDP (or SDP) data are used) then a divergence is observed i.e. richer states are observed to grow faster. (This is reported in Ahluwalia(2000)). It is unclear as to what precise interpretation one should attach to the finding of divergence at the inter-state level. The impact on poverty, and inequality, of inter-state divergence is unclear and ambiguous. If the poor are concentrated in a rich state, and if a rich state grows faster, then inequality and poverty may decline more than the counter-factual of evenly shared growth.

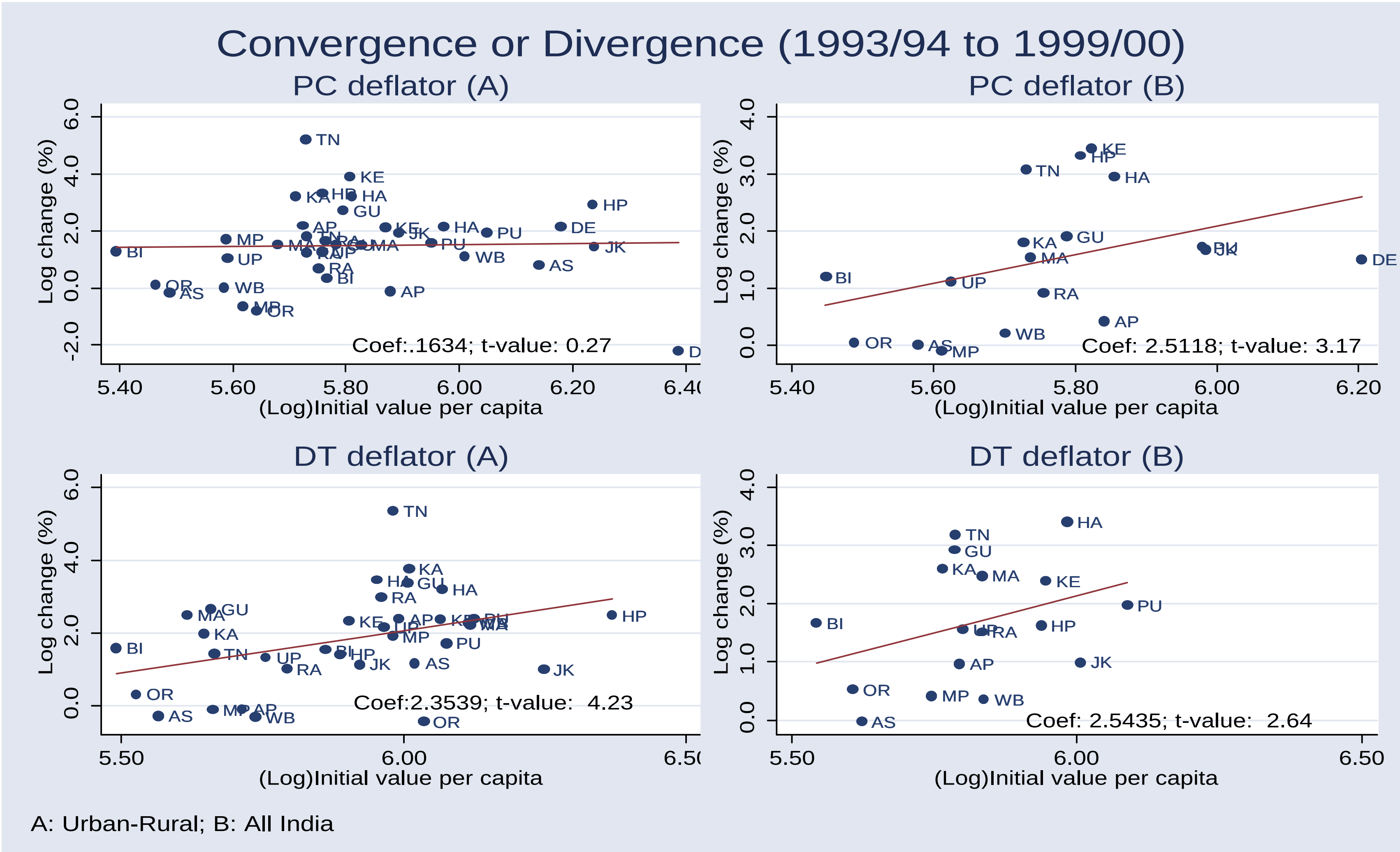
However, the theoretically expected outcome is that of convergence. Deaton-Dreze present a graph of their adjusted 1993/94-1999/00 growth rates with respect to initial (1993/94) geometric mean averages of per capita consumption (MPCE5m definition, NSSO CE data). This graph shows a positive relationship between growth and initial levels. This pattern parallels that found with SDP: “In other words, there has been a growing `divergence’ of per capita expenditure (and also of per capita SDP) across Indian states in the nineties” (p. 3735).

It is worth repeating this exercise for the two periods: the eighties and the nineties. It is also useful to repeat the exercise with the data for MPCE5y, rather than MPCE5m expenditures. We also take a rural or urban area of a state as the unit of observation. The first result of these various convergence tests is that the conclusion from such tests of inter-state disparities is ambiguous and depends largely on the price deflator used[26] and what is the unit of observation (state or urban rural areas of the state). For the 1983 to 1999 period, regardless of test or unit of observation, there is a strong result of convergence. For the 1993/94 to 1999/00 period, the results are ambiguous, but none show convergence. Some of these results are documented in Figure 3a and Figure 3b.

This same error (of interpreting overall inequality from partial inequality) was implicit in the conclusions of many that world inequality had worsened because Chinese (and Indian) inequality had increased. The opposite is the correct result (see *Imagine*). The

[26] This result is also reached by Sen-Himanshu.


Convergence or Divergence (1983 to 1999/00)
PC deflator (A)
PC deflator (B)
DT deflator (A)
DT deflator (B)
Log change (%)
(Log)Initial value per capita
Coef: -.6939; t-value: -2.28
Coef: -.1427; t-value: -0.34
Coef: -.5177; t-value: -2.55
Coef: -.8326; t-value: -2.59
A: Urban-Rural; B: All India



Notes:
The convergence divergence regression reported is a simple regression relating the growth in real survey per capita expenditures and the (log) initial level of expenditure.

result is intuitively so because the poor person's growth (of the poor in China or India) is considerably higher than the rich person's growth (the rich in the West).

In order to place India's inequality change in a comparative perspective, Table 12 reports on the changes in inequality observed in India, China, and developing countries as a whole. The Indian experience is one of very low, and moderate change in inequality. And the comparison with China seems a bit extreme. While Indian inequality has barely budgeted, China's six year inequality change in the 1990s is at least double that of India.

Table 12: Inequality (log) change in the World Inequality, 1980-2000

Lag (Years)	(Log) change in Gini Maximum	Minimum	Average
Consumption surveys			
1	42.2	-10.8	1.1
2	30.9	-44.0	-0.6
3	37.5	-27.4	-0.5
4	22.0	-16.0	0.1
5	32.4	-26.8	3.2
6	20.6	-59.9	-1.2
10	32.4	-11.6	1.8
16	10.1	-10.5	4.1
Income surveys			
1	31.4	-14.8	1.1
2	38.3	-18.3	1.7
3	34.1	-29.8	3.1
4	31.1	-28.9	3.6
5	72.1	-32.4	5.9
6	33.1	-25.1	3.3
10	58.3	-20.7	9.2
16	33.8	-29.4	3.3
India			
Consumption (6 years)	6.0	-2.7	2.0
Income (>=6 years) Gini	7.9	-8.2	0.8
Income (>= 6 years) variance of logs	18.0	-16.6	0.6
China			
Consumption (6 years)	14.4	-2.9	8.6
Income (6 years)	33.1	-2.3	17.5

Source:

Section 4f: Other Results on Trends in Inequality

Inequality change in India has been evaluated by several recent studies. Five of them – in chronological order, Ravallion, Banerjee-Picketty, Dreze- (Amartya) Sen, Deaton-Dreze and (Abhijit) Sen-Himanshu – contend that inequality worsened significantly and pervasively in the 1990s. The first two conclude on the basis of data existing *prior* to the release of the results of the 1999/00 CE & EU surveys. The other three conclude on the basis of the 1999/00 CE survey, and ancillary data, particularly the data on growth in real wages in agriculture. Given the inherent uncertainty surrounding any adjustments to the 1999/00 data, recourse to “supporting evidence” is almost mandatory.

One statistic stands out from these studies – that the rate of growth of wages of agricultural workers declined precipitously in the 1990s. Wage growth is reported by Dreze-Sen and Deaton-Dreze to be a very high, above 5 % p.a., rate in the 1980s; the 1990s rate is estimated to be around 2.5 % p.a.. Given such a large decline in the growth rate of wages of the poor, it does seem plausible that inequality increased markedly.

The wage growth data are explored in detail in Section 5. Part of the reason for this exhaustive study is because we cannot reproduce the Dreze-Sen-Deaton assertions on wage growth in the 1980s and 1990s, and nor can we find a *large* deceleration in wages. Instead, we find a large acceleration in wages for rural residents, and casual workers. For manual workers in agriculture, we find that the rate of growth has accelerated from 2.3 to 3 % p.a.. Thus, our results are the mirror image of the inequality increasing supporting evidence – wage growth actually accelerated, and near doubled in the 1990s with respect to the growth observed in the 1980s. Thus, by the logic used i.e. collapse in agricultural wage growth was inequality increasing, it should also be the case that an acceleration in wages should be inequality decreasing. And this is precisely what happened in the 1990s relative to 1980s.

Sen-Himanshu employ a completely different methodology, obtain very little poverty decline for the 1990s (compared to Deaton-Dreze) and yet obtain very little inequality change – less than 4 percent compared to 10 percent obtained by Deaton-Dreze.

Bhalla(2004a) uses a very different methodology which attempts to correct the NSSO expenditure data for two important biases – the bias caused by the fact that survey mean expenditures have diverged from national accounts, and the bias caused by changing definitions. He finds that adjusted inequality at any point in time is 10 percent *higher* than measured, and that there is *not* an increasing trend in inequality. Indeed, he finds a very minor (3 percent) improvement in inequality in the 1990s.

These conflicting estimates, and trends, should be kept in mind while following the detailed discussion of what the different studies say about inequality. In the examination that follows, it will be established that all but one of the inequality change studies have severe methodological and or data problems. Even if the method is clear (Ravallion) the data are suspect, and easily shown to be wrong. Only one study (Deaton-Dreze) has clearly stated logical set of assumptions.

Ravallion(2002):

The first study to argue the case for an increase in inequality was that of World Bank scholar Ravallion(2000). Using NSSO data till 1997, he constructed national Ginis based on rural and urban distributions for each of the individual years; these Ginis were estimated according to " parameterized Lorenz curves based on general elliptical or incomplete Beta functions, whichever fitted best". (p.3247). He concludes thus:

"Is inequality rising nationally? …With a correction for the serial dependence in the residuals the trends coefficient rises to 0.29, and becomes significant at the 1 per cent level. …..Of particular note is that the critical segment of the distribution from the point of view of the poverty rate have been seeing a statistically significant trend decline in its share of total spending, and markedly so in the 1990s." (p3249-50)

There are several problems with the Ravallion study, and conclusion. Most importantly, and crucially, Ravallion's *estimated* Ginis, on which the entire inequality worsening result is based, are ***not*** accurate for two of the end years of the 1990s (1994 and 1997), and, somewhat inexplicably, reasonably accurate for all the other years. For the two error years, the misfits are large: Ginis of 36.32 is estimated for 1994/95 when the actual "official" unit-level based Gini for that year was 34.88. In 1997 (the last observation used) the Ravallion estimate is 37.83 when the actual official estimate was a considerably lower 34.05[27]. Without these errors, Ravallion's trend increase in inequality becomes woefully insignificant.

Banerjee-Picketty(2002)

The second study to reach the inequality worsening conclusion was Banerjee-Picketty(2002).

"The average income growth among the top percentile of the tax units was 71% in real terms between 1987-8 and 1999-0, which is substantially more than average growth according to the national accounts. Moreover, the higher one goes within the top percentile, the higher the growth (up to +285% for the top 0.01% income fractile)". (p. 10)

The authors use income-tax records to derive conclusions about trends in inequality. Their study has only *limited* use for information about inequality because even today, only a fifth of India's workforce is eligible to file tax returns, and out of this fifth only a third actually file tax returns. In other words, less than 10 percent of the 400 million workforce files tax returns, the basis for the Banerjee-Picketty interpretations of inequality trends. Within this top decile, Banerjee-Picketty conclude that the top .1 percent of Indians had faster growth ("the rich got richer") than the top 1 percent! Even this limited conclusion is questionable, because an important assumption for the authors is that compliance levels have stayed the same. However, Bhalla(2002b, 2004) and Kelkar et. al. (2002), provide strong evidence which suggests that there is a "missing middle" in India's tax returns; both the very rich and the very poor of India's 20 percent tax eligible population have much higher compliance ratios (in the thirties) than the single digit compliance ratios of the middle (those earning between Rs. 2 lacs - Rs. 4 lacs per annum). The rejection of their critical assumption makes the Banerjee-Picketty inequality worsening results very difficult to accept. In addition, even if Banerjee-Picketty are completely correct, no inequality interpretation is possible since it merely maybe the case that the super-rich are taking from the very rich, leaving aggregate inequality unchanged.

[27] Note that the mismatch between actual and estimated Gini in one year alone is more than the estimated inequality increase in India during the nineties, and the cause of much (heated) debate,

But there are deeper problems with the Banerjee-Picketty data, and consequent estimates of income distribution trends over time. The quote above shows that the authors are talking about the change from 1987/88 to 1998/99. Since most data that we have analyzed in this paper is for the two periods, 1983 to 1993/94 and 1993/94 to 1999/00, it would be useful to note the Banerjee-Picketty model estimates for just the 1990s. According to Table A2 in their paper, the average income of the 99th percentile (P99) was Rs. 90,345 in 1993/94 (in 1999/00 Rs.). In 1999/00, the real income level of the richest percentile is reported by them to have been *5 percent less*. There are two possibilities with regard to this unexpected result – either their data (simulated through a Pareto distribution) are incorrect, or that all the wage earners showed a decline in real income. If their data are "correct", then one conclusion from the Banerjee-Picketty paper is that income distribution *improved* enormously in India, since all surveys show that there was an increase in mean incomes (the NSSO E&U survey shows that average wages increased at a 4.6 percent annual rate in the 1990s! (Table 2). This result is somewhat contrary to their central thesis of worsening inequality.

Limited as the intent of the Banerjee-Picketty paper was, and despite the fact that the paper reached contradictory conclusions, it (the paper) has received considerable support for its "implications" of worsening aggregate inequality. The authors themselves never reach this conclusion, though their result has been accepted to be "true" and found by Deaton-Dreze to be "consistent" with their own results:

"our findings on rising economic inequality within the urban sector are consistent with recent work by Banerjee and Piketty (2001), who use income tax records to document very large increases in income among the very highest income earners. They show that, in the 1990s, real incomes among the top one per cent of income earners increased by a half in real terms, while those of the top 1 per cent of 1 per cent increased by a factor of three in real terms". (p. 3740)

Sen-Himanshu also endorse the Banerjee-Picketty conclusions and find it supportive of their own conclusions about inequality change:

Banerjee and Piketty (2003) provide independent evidence from income tax data that inequality at the top of the distribution, that is, income shares of the top percentile and above (that had decreased earlier), increased very sharply between 1983 and 1987 and again after 1992. They show that, if these very rich were not captured adequately by NSS, their increased share alone can explain 20 to 40 per cent of enlarging NAS-NSS differences. This is in addition to inequality increases discussed here, i.e., those captured by the NSS itself, and disregards possible NAS overestimation. (p. 4375)

As the above discussion revealed, there is nothing in the Banerjee-Picketty paper to suggest that inequality worsened in India in the nineties; none of the commentators note that precisely at the time the top 0.01 percent was increasing its share in total income (increasing inequality), the top 1 percent was decreasing its share (improving inequality). Since the top 1 percent has 100 times as much 'weight" as the top 0.01 percent, surely the net result is greater equality, *ceteris paribus*. Independent of this observation, assume that the top 1 percent had actually increased its real incomes (contrary to the decline reported by Banerjee-Picketty). Overall inequality would only worsen if this increase was at the expense of say the bottom 80 percent or so, rather than at the expense of the 98th percentile. This computation is not only not shown by the authors, it is not even attempted.

Bhalla (2002a, 2002b, 2004a):

One alternate adjustment to the NSS data is to match the survey means to NA means. This procedure is suggested by Bhalla to account for the fact that the survey capture ratio (survey means as a percentage of corresponding NA means) has shown a sharp decline in India in the 1990s. An adjusted distribution can be derived on the basis of matching each survey commodity mean with the corresponding national accounts mean.

This method suggests that different under-estimation levels for different commodity expenditures can be identified to some degree of accuracy. First, the national accounts expenditures are tabulated for each *individual* item - e.g. cereals, pulses, vegetable, fruits, dry fruits, consumer durables, education etc. A similar exercise is carried out for the NSSO CE survey data. Thus, two means for each item are obtained – a survey mean and a NA mean. If it is now assumed that the NA *means* are correct on an item by item basis, then the degree of under or over estimation for each household can easily be derived. For each individual, an average multiplier can be obtained, which is the ratio of the adjusted sum of individual and item specific expenditures, to the sum of expenditures in the survey data.[28]

For example, if mean household consumption of cereals in a particular household is Rs. V, and if the mean per-capita consumption of cereals in the NSSO survey is Rs. X, per capita per month, and Rs. Y the mean per capita NA expenditures for the same item, then the adjusted consumption of cereals for the household is Rs. V*Y/X. If this procedure is repeated for all items with each item having its own unique ratio Y/X, then one can obtain an "adjusted" survey mean expenditure per household, and therefore a new adjusted distribution.

This method ensures that there would be "equality" between the survey and NA estimate for the various (39) items of expenditure. This adjusted distribution is computed for each of the survey years (for each year, each survey item is "matched' with the corresponding NA mean and a ratio Y/X obtained) , and the average multiplier obtained for each household, and each decile of households.

Before discussing the results of this adjustment, it is important to note three considerations. First, that the NA mean for each item is accurate – that is what is being matched with. Second, this adjustment method preserves the distribution *for each item* i.e. if a household did not "consume" any TV's in the original distribution, it will not do so in the new distribution. Third, a lower than average overall multiplier for poorer households means that the resulting adjusted distribution will be more unequal; a higher than average multiplier will mean that the new distribution is more equal.

As documented in Bhalla(2004a), the under-estimation of food expenditures is not small even though it is lower than non-food expenditures . This has implications for calculations of *adjusted* inequality. Food items have a low income elasticity – and there

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[28] Since the survey does not contain estimates of imputed rent, house rent is ignored. Further, as documented by Sundaram-Tendulkar, there seems to be a genuine problem with the NAS estimate for clothing. In the 1998 NAS estimate for 1993, clothing expenditures are reported to be Rs. 48350 crores. In the 1999 NAS revised estimates, this expenditure is reduced to only Rs. 21403 crores. Which figure is correct? The first estimate of clothing is taken as the "correct" figure and the difference between the two figures – Rs. 48350 and Rs. 21403 crores, or Rs. 26947 crore, is subtracted from the figure of Rs. 143787 crores for miscellaneous goods and services.

is a physical limit to how much extra food the rich can consume. So the “benefits” of under- estimation of food items accrue “disproportionately” to the poor, as do benefits of underestimation of non-food items accrue “disproportionately” to the rich. Further to this point, it is worth noting that a relatively income elastic food category - milk and milk products and meat, etc. plus fruits and vegetables – is estimated by the NSSO surveys to be just more than half the national accounts estimate. But the rich cannot possibly be drinking all this milk and eating all the missing vegetables. Accordingly, it must be that the non-rich have also under-estimated their food consumption by large proportions. Generally, the large error for the food group suggests that whatever adjustments are done to the Indian survey data on the basis of national accounts data, they will yield the result of a reasonably constant multiplier.

Given these item-wise ratios, it is now a simple calculation to adjust each individual’s consumption to national accounts. Note that the resulting multiplier is *different* for each individual and a function of their consumption pattern. Table 13 reports the resulting average multiplier for each decile of households, arranged according to per-capita expenditures. The results are striking - even for the poorest decile, the average under-estimation is of the order of 30 percent. The multiplier rises progressively with the richness of the households, but the variation for the first eight deciles is in the narrow range of 50 to 60 percent for 1993/94. The border decile for the poor is the 40th percentile. The multiplier for this decile, 1.25 in 1983 with the average multiplier being 1.29; for 1993/94 the 4th decile had a multiplier of 1.55 with the national average being 1.62.

But are these multiplier results plausible? Is it reasonable to expect that most of the missing consumption is accounted for by the rich. Indeed it is! The adjustments preserve the original distribution for each *item*. Since the top 20 percent of the population command about 45 percent of expenditures, they can also claim 45 percent of the *missing* expenditures. Further, as shown in Bhalla(2004a), the under-estimation ratios for different categories of expenditure seem intuitive and plausible. The large under-estimation of food items, about 42 percent, is considerably less than the 68 percent under-estimation for non-food items.

Table 13: How much have decile multipliers changed, 1983-2000?

	1983	**1987**	**1993**	**1999**
Decile1	119.6	118.5	150.3	166.4
Decile2	121.6	120.7	153.2	170.2
Decile3	123.4	122.4	155.5	173
Decile4	125	124.6	157.9	175
Above4	122.4	121.6	154.2	171.1
Decile5	126.4	126.4	159.9	177.7
Decile6	128.3	128.3	162.1	179.3
Decile7	130.2	131	164.4	182
Decile8	132.8	133.9	166.6	184.7
Above4	129.5	129.9	163.2	180.9
Decile9	136	120.4	170	188.3
Decile10	145.1	143.9	176.5	193.1
Above2	140.6	132.2	173.3	190.8
Average multiplier	128.8	127	161.6	179
NA/S average	128.8	127	161.6	179
S/NA average	77.62	78.73	61.87	55.88

Sources: NSSO surveys, respective years.
Notes:

1. Columns labeled 1983, 1987, 1993 and 1999 show the *relative* multiplier for each decile for the respective years.
2. Each figure represents the average number (divided by 100) that NSSO household consumption needs to be multiplied by to achieve an economy wide match between NA data and adjusted survey expenditures.

Table 14: Consumption Inequality in India, 1983 - 1999/00

NSSO Consumer Expenditure data (unadjusted and adjusted)

	Gini			
	1983	**1987/88**	**1993/94**	**1999/00**
Nominal Measured				
Rural	.304	.300	.285	.263
Urban	.339	.350	.343	.347
All India	.325	.329	.325	.320
Nominal Adjusted				
Rural	.329	.333	.311	284
Urban	.359	.364	.368	.360
All India	.351	.360	.352	.336

Source: Unit level data, NSS surveys, 1983-1999/2000; CSO data for personal consumption expenditure of individual items, 1993-94 series.
Notes:

1. Survey means of 18 broad food items and 21 broad non food items of expenditure have been matched with CSO data to obtain the adjusted distribution of expenditures.
2. The 1999/00 data for unadjusted Gini is based on MPCE5y; the unadjusted Gini for earlier years are based on MPCE5m.

Inequality levels for the synthetic and actual distributions are documented in Table 14. The data are presented separately for rural and urban areas, for actual and synthetic distributions, and for real levels of expenditure, deflated by both the planning commission and Deaton price deflators. In all there are *six* inequality estimates for each year, and therefore six separate estimates of inequality change. The level of inequality is higher by about 10 percent at any point in time; but there is nevertheless a decline in inequality, both between 1983-1993 and between 1993 and 1999. Given the prevalence of greater understatement by the rich, the result that inequality at any given point is greater (about 10 percent) is to be expected. The second interesting pattern which emerges is that the *adjusted data shows a decline* in inequality for the 1980s and the 1990s. *No* all-India estimate shows that inequality worsened during 1983-1999, or during 1993-99 period.

Dreze-Sen (2002):

This comprehensive study about India's development does not offer an estimate of inequality change, but does offer evidence it considers is conclusive about a sharp increase in inequality. Two of their indicators of inequality change – increase in urban/rural expenditure ratios and increasing inter-state disparities – were examined earlier and found to be highly inconclusive. In addition, the authors had contended that the growth rate of rural wages declined by half in the 1990s; this "evidence" was picked up Deaton-Dreze below, and by several other authors. It is shown in the next section that the wage growth decline evidence is not only weak, but it cannot be reproduced, and indeed, the reality (according to all the wage data available, including the AWI wage data used by the authors) is the opposite – wage growth did not decline in the 1990s, but accelerated, and near doubled according to the NSSO data. The NSSO data are considered to be most reliable by several experts on poverty.

Deaton-Dreze (2002):

The Deaton-Dreze(2002) study is the only study which develops a "model" for estimating an adjusted distribution in 1999/00. As such, it is the most rigorous and deserves critical scrutiny. Perhaps recognizing the uncertain nature of their adjustments, the authors present alternate "estimates" of trends in inequality, trends which they believe clinch the conclusion of a pervasive increase in inequality in the 1990s. They argue the likelihood of this result on the basis of widening disparity in urban/rural incomes (this reasoning is problematic, as shown earlier). Besides these conjectures or "associated" evidence, the authors, unlike others, do concretely present estimates of inequality change for the period 1993/94-1999/00. This evidence is the most compelling of all their arguments (and the arguments of others who conclude that inequality significantly worsened in the nineties). It is worth examining this evidence in some detail.

Unfortunately, the authors do not describe the method which they use to obtain adjusted and comparable inequality (log variance) in 1999/00, and compare it to the actual, observed measure of inequality in 1993/94. Their unspecified method generates a 10 percent change in the log variance for the 1990s – an increase from the measured log-variance in 1993/94 of 0.29 to a forecasted 0.32 in 1999/00

A suggested interpretation of the unspecified Deaton-Dreze method, and a successful replication of their results, is as follows. Deaton-Dreze correctly emphasize the use of data on those goods for which the recall period has stayed constant in all the years (m goods in Deaton, or Enfes30). Intuitively, changes in the variance of expenditures on such goods should correspond to changes in the variance of total expenditures. Equations (7) and (8) illustrated how the expected mean could be forecast from knowledge of Enfes30 in 1999/00; rewriting equation (7),

$$\text{Log } E_{t-1} = á + â \text{ *(log) Enfes30}_{t-1} + u_{t-1} \quad , \tag{7}$$

This equation can also be used for extracting an estimate of log variance in the year when E_t is not observed. From the above equation, it follows that:

$$\text{variance } (\log E_t) = â^{2} * \text{ variance } (\text{Enfes30}_t) + \text{variance } (u_{t-1}) \tag{8}$$

While the above equations can be made complicated (e.g. addition of non-log linear terms on the right hand side, non-parametric estimation etc.), the suggested method is extremely straightforward and involves two stages. First, estimate equation (7) for 1993-94 i.e. for the "benchmark" year. This estimation yields the structural coefficient b as well as an estimate of the unexplained variance, variance(u_t). By assumption (no change in tastes and relative prices), b is expected to remain the same in 1999-00 as in 1993-94. The second stage imposes these coefficients on the data for Enfes30 for 1999-00 (these data are uncontaminated) and a predicted variance obtained for total expenditures in 1999/00, the variable of interest.

This suggested method involves an *estimate* of the error variance from the previous year, an *estimate* of the elasticity (which has its own standard errors) and an *estimate* of the variance of Enfes30 in the "present" year. In other words, a highly non-linear process.

With this method, one can obtain the predicted variance for any year using any other year as the base e.g. one can project the variance of MPCE5y for 1983, a year in which MPCE5y was not measured (but MPCE4y was). Thus, variance can be estimated in a "chain" fashion. Each year provides the information for an estimate of inequality in the next year, provided the assumption is made that the structure of the relationship between Enfes30 goods and total expenditure stays the same. Note that what is carried forward from one year to the next is not just the coefficient â but also the error variance whose magnitude ranges between a quarter and a third of total variance.

The hugely non-linear process, and the fact that adjusted inequality can be obtained for each survey year (in addition to measured inequality) raises an interesting methodological question. In order to assess the change in inequality, does one compare the adjusted estimates with measured inequality, or adjusted inequality in one year with adjusted inequality in another year (even if for that year the estimate of measured inequality is "correct")? Our conclusion is that given the extremely non-linear nature of the above method, and the fact that the error variances are not small, it is prudent to compare the adjusted inequality series over time, rather than to compare observed inequality in 1993-94 with the adjusted inequality in 1999-00.[29]

[29] The case for this procedure is vehemently made by Deaton-Dreze(2004). They claim that the above involved second-order transformation is equivalent to a first order transformation involving just the means. The authors claim that their adjustment in 1999/00 is akin to a Fahrenheit to Centigrade conversion. The authors fail to note that what might be appropriate for comparing means may be entirely inappropriate for comparing variances.

There are three ways in which Enfes30 goods can be measured – in nominal terms, or adjusted for inflation by either the Planning Commission (Poverty line price index or the Deaton price index). Total expenditures in 1993-94, however, can be measured in four different ways: as either MPCE5m or MPCE5y, and as real (two different poverty lines). Deaton-Dreze prefer the use nominal MPCE5m expenditures; Table 15 reports their state level and all-India results, and our estimates, according to the method outlined above; following them, this table contains adjusted log variance for nominal (MPCE5m) expenditures..

Deaton-Dreze do not outline their procedure, so it is impossible to attempt an exact reproduction. But it appears that the simple method outlined above matches the Deaton-Dreze results exceedingly well.[30] There are 38 "matches" that can be made between our results and those of Deaton-Dreze. For eight cases, our inequality estimate is exactly the same as Deaton-Dreze. If exactness is defined as being within .01 (Deaton-Dreze report rounded up figures to 2 decimal places, and we have done the same) of the "correct" Deaton-Dreze result, then our method is exact for 28 out of 38 matches, including the important matches for rural India, urban India, and all-India. For the other 10, the matches are within a reasonable tolerance level. It is quite clear, therefore, that the two methods (our simple method outlined above and the undefined Deaton-Dreze method) yield virtually identical results.

The "test" of the method for 1999-00 nominal expenditures being so successful, we extend the test for different years, and for two dependent variables, MPCE5m and MPCE5y. The results are reported in Table 16. The result that the log variance increased from 0.29 to 0.32 is reported in row 3 – this is the Deaton-Dreze result. If instead we use real expenditures (a more accurate description of inequality change), then the observed inequality change is a third lower – from 0.27 to 0.29. If instead of MPCE5m, an estimate is made of log variance in MPCE5y in 1999/00, and the PC deflator, then the inequality change is only from 0.23 to 0.24 i.e. only about a 4 percent change or almost no increase.

[30] Deaton-Dreze(2004) mistakenly think that this method yields the "wrong" results. "We would have been more impressed by Bhalla's compliment if he had understood the corrections we made, but unfortunately he has badly misread what we did. In consequence, his own calculations, which he claims follow our method, do nothing of the sort, so that it is not surprising that he gets the wrong answer."

Table 15

Log Variances : Replication of the Deaton-Dreze method of estimating inequality in 1999-00

(Matches are reported in bold)

	Actual				Predicted			
					Equation 2			DD
States	**1983**	**1987**	**1993**	**1999**	**1987**	**1993**	**1999**	**1999**
Rural								
Andhra Pradesh	0.22	0.22	0.19	0.17	0.29	0.25	**0.23**	**0.22**
Assam	0.11	0.14	0.10	0.13	0.14	0.11	**0.12**	**0.11**
Bihar	0.18	0.16	0.14	0.13	0.21	0.17	**0.16**	**0.16**
Gujarat	0.18	0.19	0.16	0.18	0.20	0.17	**0.18**	**0.18**
Haryana	0.21	0.22	0.22	0.19	0.23	0.25	0.28	0.31
Himachal Pradesh	0.19	0.17	0.19	0.17	0.20	0.22	**0.23**	**0.24**
Jammu & Kashmir	0.13	0.17	0.13	0.12	0.15	0.20	**0.14**	**0.14**
Karnataka	0.24	0.25	0.18	0.18	0.28	0.23	**0.23**	**0.22**
Kerala	0.27	0.27	0.22	0.24	0.30	0.26	**0.26**	**0.27**
Madhya Pradesh	0.21	0.20	0.18	0.18	0.27	0.23	**0.22**	**0.22**
Maharashtra	0.20	0.20	0.22	0.21	0.25	0.24	**0.29**	**0.28**
Orissa	0.22	0.18	0.15	0.18	0.27	0.19	**0.22**	**0.21**
Punjab	0.21	0.19	0.17	0.19	0.25	0.23	0.22	0.24
Rajasthan	0.28	0.24	0.16	0.14	0.36	0.25	**0.18**	**0.18**
Tamil Nadu	0.31	0.27	0.24	0.23	0.36	0.30	**0.24**	**0.24**
Uttar Pradesh	0.21	0.19	0.19	0.18	0.25	0.23	**0.22**	**0.21**
West Bengal	0.26	0.17	0.15	0.15	0.30	0.17	0.17	0.15
All India, Rural	0.24	0.22	0.20	0.20	0.28	0.24	**0.24**	**0.24**
Urban								
Andhra Pradesh	0.27	0.29	0.27	0.29	0.33	0.35	**0.34**	**0.33**
Assam	0.19	0.21	0.23	0.30	0.21	0.21	0.33	0.27
Bihar	0.23	0.21	0.26	0.30	0.30	0.28	**0.29**	**0.30**
Gujarat	0.26	0.20	0.22	0.25	0.23	0.24	**0.25**	**0.26**
Haryana	0.30	0.21	0.22	0.27	0.33	0.26	0.25	0.28
Himachal Pradesh	0.37	0.20	0.28	0.29	0.39	0.28	0.38	0.40
Jammu & Kashmir	0.17	0.18	0.20	0.16	0.17	0.24	**0.20**	**0.21**
Karnataka	0.33	0.28	0.29	0.32	0.39	0.34	0.36	0.34
Kerala	0.37	0.43	0.28	0.32	0.40	0.35	0.35	0.37
Madhya Pradesh	0.23	0.26	0.25	0.29	0.29	0.30	**0.33**	**0.33**
Maharashtra	0.33	0.32	0.35	0.36	0.37	0.40	0.42	0.40
Orissa	0.23	0.24	0.27	0.26	0.27	0.28	**0.30**	**0.29**
Punjab	0.34	0.21	0.21	0.25	0.31	0.24	**0.24**	**0.25**
Rajasthan	0.32	0.27	0.22	0.23	0.31	0.29	**0.25**	**0.26**
Tamil Nadu	0.32	0.34	0.33	0.34	0.38	0.36	0.41	0.35
Uttar Pradesh	0.26	0.27	0.27	0.31	0.29	0.31	**0.35**	**0.34**
West Bengal	0.30	0.30	0.32	0.31	0.37	0.33	**0.36**	**0.35**
Delhi	0.34	0.35	0.38	0.39	0.35	0.43	**0.45**	**0.46**
All India, Urban	0.31	0.30	0.31	0.34	0.34	0.34	**0.38**	**0.37**
All India	0.28	0.26	0.26	0.28	0.33	0.29	**0.32**	**0.32**

Source for DD: Deaton Angus, and Jean Dreze, "Poverty and Inequality in India: A Re-examination," *Economic and Political Weekly*, September 7, 2002, pp.3729-3748;

Method:

Notes:

Thus, the most extreme estimate of inequality change – 10 percent – appears to be that reported by Deaton-Dreze and reproduced successfully by our “approximate” method. Just using the poverty line deflator reduces this inequality change estimate to only 4 percent. No evidence of a “pervasive” increase in inequality as stated by Deaton-Dreze.

Other results from our “simulation” exercises. The method also works rather well in backcasting 1987/88 from the base year of 1993/94, but seems to fail for the backcasting for 1983. For 1983, all the methods project an inequality considerably lower in that year than that which was actually observed. For example, for MPCE5m, the measured log-variance is 0.31; the projected variance with 1987/88 as base is lower at 0.27, and with 1993/94 as base, it is also lower at 0.27.

Collecting all the results, the conclusion remains the same as what was observed with comparable “official” data i.e. some decline in inequality between 1983 and 1993/94, some worsening thereafter, but essentially no change between 1987/88 and 1999/00, the end years used by Deaton-Dreze. For all estimates of *measured* inequality, 1983 was a peak year; for all estimates of the Deaton-Dreze method of *adjusted* inequality, 1983 was a trough year of inequality. No adjoining large sample survey years show much variation, and even the movement from peak to trough is not more than 10 percent (in the worst estimate scenario). Which means that somewhat remarkably so, inequality in India, at least as estimated by the Deaton-Dreze model, witnessed very little inequality change between the 1980s and 1990s.

Sen-Himanshu

The authors do not present an explicit estimate of inequality change. They offer the trend in the urban/rural ratio as indicative of significantly worsening inequality. This was shown to be questionable. There is an inequality estimate implicitly contained in the author’s paper. They estimate that real expenditures of the rural poor (this time based on the MPCE5y definition) increased by only one-fifth of the national increase in per capita expenditures, and that of the urban poor increased at one-half the national rate. These statistics are enough for us to derive the following cumulative six-year growth rates consistent with Sen-Himanshu : Rural poor, 1.52 percent; rural non-poor 7.96 percent;

Table 16: Actual and Deaton-Dreze adjusted inequality, 1983 – 1999/00

	1983	1987	1993	1999	2000	2001
Variance of logs						
MPCE5m						
Nominal						
Measured	0.31	0.30	0.30			
Adjusted	0.27	0.33	0.29	0.32	0.32	0.34
PC deflator						
Measured	0.30	0.28	0.26			
Adjusted	0.25	0.27		0.28	0.28	0.29
DT deflator						
Measured	0.31	0.29	0.27			
Adjusted	0.26	0.29	0.27	0.29	0.28	0.29
MPCE5y						
Nominal						
Measured	0.27	0.26	0.26	0.28	0.30	0.33
Adjusted				0.28	0.28	0.31
Adjusted (1993 base)	0.23	0.27				
PC deflator						
Measured	0.26	0.24	0.23	0.24	0.25	0.27
Adjusted				0.24	0.24	0.25
Adjusted (1993 base)	0.22	0.24				
DT deflator						
Measured	0.27	0.25	0.23	0.24	0.25	0.26
Adjusted				0.25	0.25	0.25
Adjusted (1993 base)	0.22	0.25				
Gini						
Bhalla (2003)						
Measured	0.33	0.33	0.33	0.32*		
Adjusted	0.35	0.36	0.35	0.34		
Sen-Himanshu (rural)						
Measured			.286			
Adjusted				.314**		

Source: NSS Consumer Expenditure Survey for years 1983, 1987-88, 1993-94 & 1999-00

Notes:

1. Estimates for MPCE5y for years 1983 and 1987 are MPCE4y and MPCE3y respectively.
2. * Gini estimated from MPCE5y definition; others are estimated from MPCE5m definition.
3. ** Stands for the adjusted estimates comparable to 1993 URP MPCE5m estimates.
4. Bhalla (2004a) estimates are obtained by matching NSS to National Accounts estimates. See text for details.

rural India: 6.7 percent; urban poor: 3.8 percent; urban non-poor, 11.1 percent; urban India, 9.92 percent. If these growth rates are imposed on the 1993-94 distribution, a Gini of .3127 is obtained, which is less than 4 percent more unequal than the 1993/94 distribution (Gini of 0.3011).

Interpreting the method behind Sen-Himanshu estimate of rural inequality increase from 28.6 in 1993/94 to 31.4 in 1999/2000

All estimates of *rural* inequality (including Deaton-Dreze) indicate a worst case scenario of little change for the nineties. Sen-Himanshu estimate the increase in *rural inequality* to be 3 Gini points (or 10 percent) for the MPCE5m definition of expenditures. This Gini increase is consistent with a much higher than 10 percent inequality increase according to the more "volatile" variance of logs estimated by Deaton-Dreze. With a 10 percent change in the Gini, the authors have an estimate of change which is "radical". Hence, it is important to understand as to how this estimate was derived; the method is not explained by the authors, but we attempt a "reproduction".

The authors provide only the following explanation of the "method":

"Thus, in the 50th round, which had both the 30 and 365-day questions, rural Ginis were 28.6 and 25.8 by these two recalls; and these averaged 29.7 and 24.8 in rounds 51 to 53 where the two recalls were put to separate samples. Adjusted for the fact that the 55th round used only the 365-day recall, the correct change in rural inequality between the 50th and 55th rounds is an almost 3-Gini point increase rather than a 2-Gini point decline. (pp 4367, EPW 2004)

Given this quote, we attempt to reconstruct the Sen-Himanshu method. Table 17 presents the data for rural inequality according to the different surveys undertaken in the nineties. It appears that the Sen-Himanshu "model" is to use the MPCE5m estimate for the benchmark year, 1993/94. For years after 1993/94 and prior to 1999/00 the NSSO experimented with experiments and had one set of households answering only 30 day questions for all goods; and another set of households having the following mixture: 7 days for food, 30 days for Enfes30, and 365 days for the 30/365 group of goods.

These data are summarized in Table 17. In 1993/94 we have MPCE5m estimate of inequality; in 1999/00 we have both a 7 day and 365 day estimates. Assume that the 7

Table 17: Inequality Estimates Rural, Urban (1993/94-1999/00)

NSSO Survey Year	1993/94	1994/5	1995	1997/98	1998	1999/00
Rural						
MPCE5m (Official)	28.6					?
MPCE5y/5y[1]	25.9					26.3
MPCE7						26.5
MRP_a		30.2	27.3	30.4		
MRP_b		25.0	27.2	26.0		
MRP_a*		29.7	29.7	29.7	29.7	
MRP_b*		24.8	24.8	24.8	24.8	
Urban						
MPCE5m (Official)	34.4					?
MPCE5y/5y[1]	32.0					34.7
MPCE7						34.7
MRP_a		37.5	34.0	36.3		
MRP_b		31.9	34.9	31.8		

Source:
Notes: MRP_a* and MRP_b* are Sen-Himanshu estimates of Mixed Recall Periods (see Table 1).

day questions on food is all important and that overall inequality is driven by this estimate (MPCE7). The underlying assumption is that the presence of longer recall period does not affect the reporting of expenditures for a shorter recall period.
During the years bracketed by the large sample years, there is a presence of two sets of households – those, which reported expenditures on all goods on a 30-day basis, and those that reported expenditures on food on a 7-day basis and on non-food on a 365-day basis. The difference in the Gini between these two estimates is assumed, by Sen-Himanshu, to be equal to the 7 day food and 30 day food expenditure inequality difference in 1999/00.

Consequently, it appears therefore that Sen-Himanshu (2004) calculate the average Gini for the intermediate years (51^{st} to 54^{th} round) for both set of households. Average Gini estimates are 29.7 and 24.8 for MRP_a* and MRP_b* respectively. Therefore they observe a difference of **5** percentage points between two estimates. Since MRP_b*in the intermediate years are comparable to MPCE7 in 1999, they inflate the 1999 estimate (MPCE7) by 5-percentage point (26.5 + 4.9=31.4) to obtain a comparable Gini to the uniform recall period estimate (URP) in 1993. Based on this method, they show a 3-percentage point *increase* in Gini from 28.6 in 1993 to 31.4 in 1999/00 according to the MPCE5m definition.

Section 5a: Wage growth in the 1980s and 1990s

The difficulties of making the NSS CE surveys inter-temporally consistent has enhanced the interest of researchers, and policymakers, in the trends revealed by data on alternative indicators of monetary welfare. A natural alternative, and correlate, of trends in consumption are the trends in wages and income. The NCAER does collect information on the income distribution and consumption (particularly durables) of households in India. Such data have been collected on a continuous basis for the last ten years, and soon such data are likely to be made publicly available.

In addition to the NCAER, NSSO itself has collected data on incomes. Its employment and unemployment (E&U) surveys conducted in 1983, 1987-88, 1993-1994 and 1999-00 contain a large amount of information on wages, employment, and incomes. Not only did these surveys have the same sampling pattern as the CE surveys conducted during these years, but also that for each of the years prior to 1999-00, the data were collected from the *same* households as the CE survey[31]. Thus, prior to 1999-00, the expenditure data (MPCE5m, or MPCE5y) and wages and wage income data are for the same household.[32]

The NSSO E&U surveys contain a wealth of information about trends in employment and unemployment. Indeed, these surveys form the "official" source of information for the government on this important issue e.g. see three government of India reports on unemployment which have been reproduced in one volume (see Kapila and Kapila (ed)., 2003). These surveys have not been much used (to date) for trends on household *incomes and welfare* for three important reasons. First, because a parallel consumer expenditure survey was always available to derive information on poverty, and this parallel (CE) survey was also the official, hence definitive, source for trends on poverty. Second, it is generally believed that household income is something considerably more difficult to achieve accuracy with than household expenditure. Third, the NSSO household income data were incomplete in that they only had information on labor income.

[31] This was not a panel survey i.e. the households change from one year to the next, but for any given year prior to 1999-00, the households canvassed by the two surveys (CES and E&US) were the same.

[32] Thus, one can obtain information on the savings in each household for the survey years, 1983, 1987/88 and 1993/94. However, these are incomplete savings since income from non-wage sources are not available in the NSSO surveys.

But the NSSO expenditure data are not without their share of problems. The urgency for an alternative source of information on poverty therefore arose sometime *before* the publication of the doomed-to-be-controversial NSSO 1999/2000 CE survey. A major reason was the recognition that the NSSO CE surveys had been capturing less and less of the consumption as revealed by the national accounts. Some have argued that this is to be expected since the consumption of the rich is difficult to capture in household surveys. But this stretches the imagination because half of national *consumption* (the S/NA ratio fell below 50 percent in 2001/02) cannot be attributed to a handful of percentiles. (In contrast, income or wealth can (theoretically) be attributed to a few.)

The NSSO E&U survey:

In the NSSO E&U survey, if an individual had any labor income, whether in cash or kind, such activity, occupation, industry and wages was recorded for the week in question i.e. the wage data are based on information about the previous week. For salaried or "regular" workers, this meant that a person worked the entire 7 days of the week. For non-salaried workers, the number of days worked, and the wages received per day, are recorded separately. Thus, the average per day wage rate, as per the preceding week, can be estimated for all workers.

In rural areas, and especially for small cultivators, incomes received from wages is an incomplete indicator of family income because it excludes the contribution of family labor, and excludes the income from any form of self-employment, including cultivation. But there is no problem in interpreting trends in per day wages as indicators of what is happening to the growth in incomes of those whose major asset is labor, especially the poor.[33]

Thus, these wage, and earnings, data can be used to shed light on the various controversies generated by the CE surveys. For the absolute poor, income growth should be equal to wage growth; for the non-poor, increases in income can go towards

[33] As Sen-Himanshu point out, there is a "problem" with the earlier wage surveys (prior to 1999/00) in that such surveys did not include data on overtime payments. It is unclear, however, as to what was the strict definition of overtime payments that were excluded before 1999/00; the impact on wage growth of the poor due to this earlier omission is likely to be minimal, if not inconsequential.

savings. Given these simple assumptions, an alternative estimate can be obtained for the decline in poverty. This estimate is reported in Section 7.

A simple calculation of what the NSSO data states on the trend in wages can help to resolve some of the various debates on what happened to poverty and inequality in India in the 1980s and 1990s. There are no definitional changes involved in the NSSO wage data so there should be little controversy over what it says. Neither has any author questioned the accuracy, or the importance, of the NSS data on growth in real wages. The derived growth in real wages for the 1980s and 1990s can help shed light on the respective pace of poverty decline in the two periods – a higher rate of wage growth in the 1980s, for example, would be indicative of the growth process being more pro-poor in the pre-reform period. A faster growth rate for urban wages may be indicative of increasing inequality.

In two articles, Sundaram(2001a,2001b) documented the pattern of wage growth as revealed by the NSSO data. Yet the controversy continues about what the NSSO wage data says. This is documented below.

Section 5b: Discrepancy in interpretation of trends in real wages

The importance of data on wages, and wage growth, for interpretations of trends in poverty, has been well recognized e.g. Deaton-Dreze:

Agricultural wages provide an important source of further information on poverty. There are, in fact, two ways of thinking about the relevance of this information. First, real agricultural wages are highly correlated with standard poverty indexes such as headcount ratios: where poverty is higher, wages tend to be lower, and vice versa. Based on this statistical association, real wages can be used to provide some information about other poverty indexes. Second, it is also possible to think about the real wage as a rough poverty indicator in its own right. (p 3737).

Deaton-Dreze then go on to document some of their findings on wage growth, along much the same lines as Dreze-Sen had done in their book. The major emphasis, by the authors, is on what they believe is contained in the AWI (Agricultural Wages in India) data on the trend in agricultural wages. These AWI data are possibly the longest running set of data on agricultural wages in India, but in the recent past, the use of these data had been shunned by most observers on grounds that such data revealed volatile year to year trends. These data were also published in the annual Government of India, document, *Economic Survey*.[34] However, they still contain useful information about what is happening to agricultural wages i.e. useful information about the poorest of workers. These data were also published in the annual Government of India, document, *Economic Survey*.[35]

The advantage of the AWI data are that they provide annual state level data on agricultural wages (generally for a ploughman). The disadvantage is that these data are not as reliable as the more comprehensive and reliable NSSO data; but the NSSO data are only available on a five-year basis. An alternative agricultural wage series can be derived from the annual Cost of Cultivation surveys, surveys which document the cost of labor, quantity of labor, cost of other inputs etc. for all the different crops cultivated. These data seem considerably more "reliable" than the AWI data, but the data have not been disseminated as widely.

Given the data availability of the AWI, what seems a prudent research strategy is to estimate the trend according to the AWI data and see if this trend broadly conforms to the pattern yielded by the intermittent large sample NSSO data. This seems to have been the strategy adopted by Dreze-Sen, Deaton-Dreze and Sen-Himanshu.

Dreze-Sen (p. 328)

Similarly, the growth rate of real agricultural wages fell from over 5 per cent per year in the 1980s to 2.5 per cent or so in the 1990s.* Given the close (inverse) association between real agricultural wages and rural poverty, this pattern is consistent with the belief that poverty has continued to decline in the nineties, but perhaps at a slower rate than in the eighties…..* Sundaram (2001a) presents independent estimates of the growth rates of real wages and earnings, based on comparable NSS data for 1993-4 and 1999-2000. His work suggests that the yearly 'wage earnings' of casual agricultural labourers have grown at 2.5 per cent per year in real terms over the period. *This conclusion is highly consistent with our own estimates*…" (emphasis added).

[34] Since the 2001/02 Economic Survey, the practice of publishing the AWI annual state level wage variation data has been discontinued.
[35] Since the 2001/02 Economic Survey, the practice of publishing the AWI annual state level wage variation data has been discontinued.

Deaton-Dreze (p. 3737, 3738):

According to recent estimates based on AWI data, real agricultural wages were growing at about 5 per cent per year in the eighties and 2.5 per cent per year in the nineties. Thus, real agricultural wages were growing considerably faster in the eighties than in the nineties….

Sundaram estimates that the real earnings of agricultural labourers have grown at about 2.5 per cent per year between 1993-94 and 1999-2000. These are tentative estimates, based as they are on data for two years only. Yet *it is reassuring to find that they are consistent with the AWI estimates* (emphasis added).

Sen-Himanshu (p.4368):

The different time-series available agree that, *although less than during the 1980s*, 1990s growth of real agricultural wage rates averaged 2-3 per cent per annum at the national level…… The consequent decline, through unemployment and lower work participation, of days of employment per member of agricultural labour households makes clear why *although NSS reports real wage rate growth of 2.8 per cent per annum* during 1993-2000, much higher than rural MPCE, it reports per capita consumption of these households increasing less than the rural average… (*emphasis added*)

The above quotes make clear that the authors believe that the NSSO data "verification" is needed to support the AWI wage growth estimate for the 1990s. Once this "consistency" is found, the authors feel they can accept the AWI trend of a large decline in wage growth rates in the 1990s relative to the 1980s.

There are no problems with the above research reasoning; however, there are three associated problems. These problems are large enough to invalidate most conclusions based on AWI data. First, the AWI data only yields an estimate of *wage growth*, and the NSSO data does not indicate the *wage growth* that the authors claim it does. Second, the NSSO data can also yield an estimate of the wage growth in the 1980s, something none of the above authors even mention in their 1980s and 1990s wage growth comparison. Third, the time-trend of wage growth yielded by the NSSO data shows a marked *acceleration* in rural wages, and not the *deceleration* "found" by the AWI data.

Was there a major decline in agricultural wages, 1980s and 1990s?

The origins of the debate on a wage slowdown are not in Dreze-Sen, but in a government of India Planning Commission document, Mid-Term Appraisal of the Ninth Five-Year Plan. It stated, on the basis of AWI data (preliminary after 1996/97) that

“Change in real wages in pre-reforms (1981-91) period was 4.7 percent and in the post reform period (1991-99) 2 percent” (Planning Commission, 2000, p. 468).

The NSSO data were (in 2000) also under the jurisdiction of the Planning Commission, but it made no mention of the NSSO trend in wages. Sundaram(2001a,2001b) was the first to document the wage growth revealed by the NSSO data. He discusses the origins of the debate on growth in real wages:

“In respect of agricultural wages and earnings, one of the issues highlighted in some recent studies, including the Mid-Term Appraisal of the Ninth Five-Year Plan, is the alleged slow down in the growth of real wages in rural India during the 1990s relative to the growth during the 1980s”.

In these two papers Sundaram presents a detailed analysis of NSSO wage growth data for two time-periods, 1983 to 1993/94, and 1993/94 to 1999/00. His Table 9 from the (2001b) paper is reproduced in full as Table 18, along with a table from the Ahluwalia(2001) report on employment and wages (as Table 19). The Ahluwalia report presents an estimate of wage growth for the 1987/88 to 1993/94, and 1993/94 to 1999/00 time-periods. Both authors use tabulated NSSO data, and an all-India inflation estimate (derived from the CPI for agricultural workers). Access to unit-level data, and use of state and urban/rural specific poverty lines allows us to more precisely estimate wage growth in the 1980s and 1990s (see Table 20). This more precise inflation adjustment actually does matter – the rate of growth of wages for “manual work in agriculture” is not constant (2.8 % p.a., Table 18, row 3), but actually is somewhat lower for the 1980s (2.3 rather than 2.8) and higher for the 1990s (3.0 rather than 2.8). More on this below.

For the moment, we want to emphasize that the Sundaram and Ahluwalia NSSO wage estimates, which were in the public domain, emphasized that there had been wage acceleration in the 1990s. For casual labor in all activities, (row 9) Sundaram

Table 18: Average Daily Wage-Earnings Received by Adults (15-59) Casual Wage Labourers in Rural India by Gender and Activity at Constant 1993-1994 Prices: All India, (1983, 1993-94 and 1999-2000)

Activity (Operation Codes)	Rate of Growth of Real Wages (Per Cent Per Annum)					
	Rural Males		Rural Females		All India	
	1993-94/ 1983	1999-2000/ 93-94	1993-94/ 1983	1999-2000/ 93-94	1993-94/ 1983	1999-2000/ 93-94
Manual work in cultivation (1-6)	2.62	2.93	2.92	2.98	2.72	2.95
Manual work in other agricultural work (7-11)	2.61	2.98	4	3.1	3.07	3.02
Manual work in agriculture (1-11)	2.74	2.77	3.09	2.93	2.86	2.82
Non-manual work in agriculture (13-14)	**4.51**	**3.47**	**2.69**	**3.18**	**3.91**	**3.37**
Casual labour in agriculture (1-11, 13-14)	2.75	2.79	3.09	2.94	2.86	2.84
Manual work in non-agriculture (12)	NA	NA	NA	NA	NA	NA
Non-manual work in non-agriculture (14)	2.82	3.07	4.12	4.57	3.25	3.57
Casual labour in non-agriculture (12-14)	2.39	3.7	4.08	4.07	2.95	3.82
Casual labour in all activities	**2.51**	**3.59**	**3.15**	**3.19**	**2.72**	**3.46**
Public works	2.29	3.83	4.1	5.04	2.89	4.23

Source: Reproduced exactly as in Sundaram (2001b, Table 9); All India figures are *not* in the original table, but are calculated by using the observed worker 'weights' - 0.67 for males and 0.33 for females, respectively.

Notes: Adjustment for inflation has been made by reference to Consumer Price Index for Agricultural Labourers.

CPIAL: 1960-61=100 1983:511; 1993-94:1147

CPIAL: 1986-87=100 1993-94:194.74; 1999-2000: 309

Table 19: Growth of Average Daily Wage Earnings in 1993-1994, Prices in Rural India

	Rural Males		Rural Females	
	1987-88 to 1993-94	**1993-94 to 1999-00**	**1987-88 to 1993-94**	**1993-94 to 1999-00**
Public Works	1.55	3.83	1.9	5.04
Casual Labour in Agriculture	1.36	2.8	2.34	2.94
Casual Labour in Non-Agriculture	1.33	3.7	1.32	5.07
Casual Labour in all Activities	0.77	3.59	1.95	3.19

Source: NSSO Surveys for 1987-88, 1993-94, and 1999-2000

Notes: Reproduced from the Ahluwalia report: "The Employment Unemployment Situation: An Assessment" *pp.38-67, Planning Commission Reports on Labour and Employment with an introduction by Raj Kapila and Uma Kapila* (2002), Academic Foundation, New Delhi documented a 1 percent *acceleration* for both male and female workers in the 1990s. The Ahluwalia report (reproduced as Table 19) documented that there was only a 0.8 percent growth in real casual worker wages for males, 1987/88 to 1993/94, and an acceleration to 3.59 percent in the 1990s. A robust conclusion emerging from these two studies is that there was a marked acceleration of agricultural wage growth in the 1990s, something that Sundaram emphasized.

"For adult rural male casual wage labourers, with the exception of 'non-manual work in agriculture', in all the other nine-categories distinguished, the rate of growth of wages between 1994 and 2000 has been about the same (in fact marginally higher) or significantly higher than that realized between 1983 and 1994…Thus, except for rural females engaged in manual work in other agricultural work and for manual work in agriculture as a whole, the *evidence from the NSS Employment-Unemployment Surveys offers no support at all for the hypothesis of a slow-down in the rate of growth of average daily wage earnings of adult casual labourers during the 1990s relative to that between 1983 and 1994*." (emphasis added, p. ??)

It appears therefore that the pattern of wage acceleration revealed by the NSSO data in no manner supports the major trend decline in wages that is reported for the AWI data. This is in a complete contradiction of the AWI data contained in the Planning Commission study and in Dreze-Sen-Deaton-(Abhijit) Sen-Himanshu documents; and repeated by several other studies (e.g. Mahendra Dev(2004)). It is imperative, therefore, that one assess which source is more accurate, and therefore, which pattern, acceleration or deceleration, is closer to the "truth". Some (the authors quoted above) have concluded that both the AWI and the NSSO data are correct for the 1990s, and therefore inferred that the AWI wage growth figure for the 1980s is also correct. How this conclusion can be reached is attempted through the reconstruction outlined below.

What these quotes reveal is that the maintained conclusion in a large part of the literature is that the Sundaram NSSO based wage growth evidence supports the conclusion that wage growth collapsed in the 1990s to less than half the 1980s level. There are several problems with the above evidence/deduction. Most importantly, there is no match at all between the AWI and NSSO wage growth rate for the nineties, a "consistency" that Deaton-Dreze found so reassuring. Further, regardless of what has seemingly been erroneously interpreted, the NSSO pattern of wage growth remains radically different than the AWI pattern: to reiterate, the former shows an acceleration, the latter a deceleration. In order to resolve the apparent discrepancy between what Sundaram obviously did not state, and what he has been interpreted as saying, the data contained in the above quotes is summarized in Table 20.

Row 1 is the wage growth reported by Dreze-Deaton-Amartya Sen on the basis of AWI data – a high growth in the 1980s (above 5 % p.a. in real terms, and one estimate as high as 5.5 percent) and a halving of the growth to 2.5 percent in the 1990s. Row 2 is the Planning Commission Mid-Term Appraisal estimate of a collapse in wage growth in the 1990s to almost a third of the 1980s level – a decline from 4.7 to 2.0 percent. Rows 3 through 5 are estimates contained in Sundaram(2001a, 2001b). The last row is a Sundaram estimate of average earnings per capita (not per worker); this estimate is an involved computation of the hours worked per week, the unemployment rate, the number of workers in a family, the "imputed" wages for self-employed workers in agriculture, etc. It is likely an accurate computation and it reveals that *per capita* wage earnings increased at a rate of 2.5 % p.a. in the 1990s. *This* is the NSSO number picked up by Dreze et. al and claimed as corresponding to the *per worker* wage growth estimate of the AWI. The two obviously are not comparable; what is comparable is either the 2.8 percent estimate of the 1990s or the 3.5 percent estimate. While the former is close to 2.5, it does not show a deceleration from the 1980s.

What Table 20 reveals, therefore, is the basis for the "2.5 percent NSSO" wage growth estimate for the 1990s; only it is not a wage growth estimate. Table 20 also emphatically demonstrates that there is no basis for an NSSO based conclusion, or support for a conclusion, that agricultural, or casual worker, or rural wages collapsed in the 1990s. Independent of these facts, it is of some interest to determine whether the AWI wage growth estimate reported by Dreze-Deaton-Sen is observed in the AWI data itself.

Table 20 : Wage growth, different estimates

	1980s	1990s
Wages per worker, AWI data		
Dreze-Sen; Deaton-Dreze	5 - 5.5	2.5
Planning Commission	4.7	2.0
NSSO (Sundaram)		
Wages per worker (casual worker in all activities)	2.72	3.46
Wages per worker in agricultural activities	2.86	2.84
Per capita earnings (Unemployed and other household members included)		2.54

The (non-existent) correspondence of the AWI with the NSSO wage data for the 1990s cannot be taken as an affirmation of the accuracy of the AWI series. A judgment about the discrepancy between NSSO and AWI data is critical to evaluate what happened to poverty decline in the pre and post reform periods. If Deaton and Dreze and Himanshu and Abhijit Sen and Amartya Sen are right then a decelerated wage growth in the 1990s is important evidence confirming, and perhaps even extending, the conclusion that inequality worsening was pervasive in the nineties, conceivably even by a significant margin. If they are incorrect, and instead the NSSO data actually state what Sundaram and Ahluwalia (and our tabulations, see Table 20) show i.e. that rural and agricultural wage growth accelerated in the 1990s, and did so in a significant manner, then a large chunk of the inequality worsening story may be untrue (though it cannot be proved that inequality either improved or remained the same). But a firmer conclusion of poverty decline can be inferred – it would be greater in the period with higher wage growth, especially if robust wage growth occurred among the poorest. For the absolute poor, almost by definition, wage growth should translate one for one into consumption growth and therefore poverty decline.

Section 5c: AWI and Cost of Cultivation survey data on agricultural wages

This sub-section assesses the veracity of the claim that non-NSSO wage data (regardless of any mis-match with NSSO data) indicates a large decline in the rate of growth of wage rates. There are two non-NSSO sources of data on agricultural wages: wages as provided in the Agricultural Wages in India, a publication of the Directorate of Economics and Statistics, Ministry of Agriculture and wages derived from various issues of Cost of Cultivation of Principal Crops in India, Directorate of Economics & Statistics, Department of Agriculture and Cooperation, Ministry of Agriculture, Government of India[36].

In a report for the Planning Commission, Bhalla(2004a) used the AWI data for the three years 1981-82, 1991-92 and 1996-97. These data are reproduced in Table 21 along with recently available data for 1997-98. These data show that the real wage for unskilled agricultural labour (males, field labour and ploughman) increased at an annualized rate of 3.1 percent in the eighties and 3.0 percent for the period 1991/2 to 1996/97. The

[36] I am thankful to Mr. Abusalleh Sharif for use of his tables on AWI data, and Mr. Pradiman Kumar for kindly providing the data on cost of cultivation data for the different states in India.

growth for the 1990s is 4 percent with the inclusion of one extra year of data (for 1997/98).[37]

Table 21: Average Daily Wages (in Rs.) of Field Labour and Ploughman

	At 1993-94 Prices				Growth Rates		
	81-82	**91-92**	**96-97**	**97-98**	**81-91**	**91-96**	**91-97**
Andhra Pradesh	19	25.1	28.4		2.81	2.44	3.1
Assam	23.5	32.4	30.6	30.2	3.23	-1.17	0.0
Bihar	20.1	26.5	25.3	32.4	2.74	-0.92	0.9
Gujarat	22.1	26.9	31.6	27.9	1.99	3.19	5.4
Haryana	40.5	49.8	58.3	37.3	2.07	3.16	3.8
Himachal Pradesh	23.7	40.5	55.5	62.5	5.35	6.3	7.9
Karnataka	18.4	20	22.8	65.1	0.83	2.58	4.9
Kerala	36	47.2	79.6	26.8	2.7	10.44	11.2
Madhya Pradesh	15.1	24	28	92.7	4.63	3.12	3.0
Maharashtra	16.2	27.3	31.4	28.6	5.22	2.78	3.8
Orissa	14.8	20.7	21.5	34.3	3.37	0.75	1.0
Punjab	34.4	51.5	52.6	22.0	4.05	0.42	0.8
Rajasthan	28.8	37.1	44.2	54.1	2.53	3.53	3.7
Tamilnadu	18.4	21	30.8	46.3	1.29	7.69	8.2
Uttar Pradesh	22.6	30	32.1	34.3	2.83	1.35	3.5
West Bengal	20.7	33.6	38.8	37.1	4.86	2.89	3.2
Average					**3.2**	**3.0**	**4.0**

Source: Agricultural Wages in India, Department of Agriculture & Co-operation, Ministry of Agriculture, Government of India, Different Years.

Note: Growth Rates are annualized and the average growth rates are un-weighted simple averages

[37] This is indicative of the volatility in the AWI series alluded to earlier.

Table 22 and Chart 4, compares and contrasts the two wage growth series issued by the Department of Agriculture: AWI and wages derived from the Cost of Cultivation survey. Data for AWI for the 1980s are taken from Dreze-Sen; for the 1990s, data from the Economic Survey are used. The AWI based 5 percent plus wage growth figure is not obtained by us for any computation for the 1980s; the highest estimate we obtain is a semi-log regression for the period 1980-1990, 4.6 % p.a.. For the same period, the end-point annualized growth estimate is 4.1 % p.a.

The 1990s AWI growth rates (both end-points and semi-log) for various periods is also reported. The largest deceleration observed for the AWI data is when the recession year 1991/92 is included in the 1990s; with this inclusion, the deceleration is from 4.6 percent to 2.7 percent (semi-log) and from 4.0 to 1.9 percent (end-point). If the estimate is taken from 1991/92 (and therefore staring at a lower base – the 1991/92 AWI wage growth estimate is –6.4 percent), then the deceleration decreases to only 1 percentage point.

Also reported is the wage growth estimated from the more reliable cost of cultivation data. It shows an annual growth of about 2.7 percent in the 1980s, and for the 1990s, the lowest figure obtained is a growth rate of 3.3 % p.a. (for 1991 to 1999). This series is not that affected by inclusion/exclusion of data for 1991/92.

The appropriate conclusion, given the diversity of data, is that agricultural wages at least maintained the eighties pace in the nineties. The "low" estimate of wage growth in the nineties was observed to be entirely contingent upon the inclusion of one year, 1991/92. And for this year, very divergent estimate of wage growth is indicated by the more representative cost of cultivation data: ((0.2 percent vs. –6.1 percent). As documented below, the conservative conclusion of no agricultural wage acceleration in the nineties becomes a conservative conclusion of significant wage acceleration in the nineties, once the broadly accepted NSSO wage series. In turn, the NSSO result is supported by cost of cultivation data, and by national accounts data..

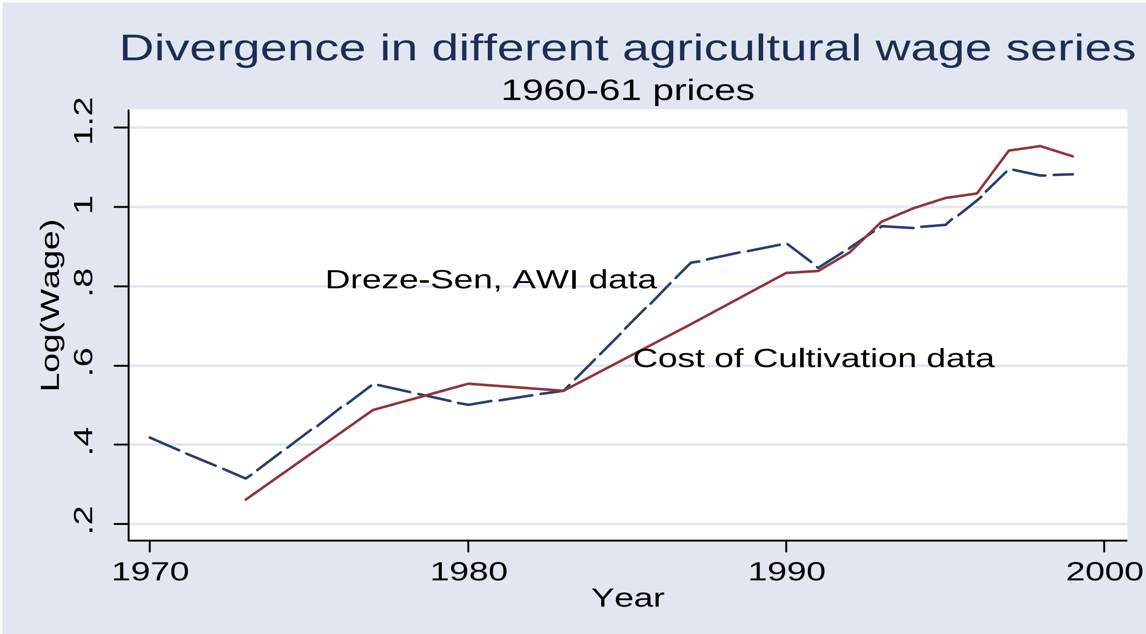

Chart 4

Table 22: Wage Growth according to AWI and Cost of Cultivation Surveys

	AWI data (annual)		Cost Of Cultivation	
	Semi log	**End point**	**Semi log**	**End point**
1980s				
1980-90	5 - 5.5*			
1980-90	4.6	4.07	3.1	2.8
1980-91	4.0	3.1	3.0	2.4
1980-92	3.7	3.3	2.9	2.4
1980-93	3.6	3.5	3.1	3.3
1990s				
1990-99	2.7	1.90	4.2	3.3
1991-98	3.4	3.32	5.3	5.1
1991-99	3.1	2.95	4.4	3.7
1991-00			4.6	5.1
1990-00				4.4

Source: Jean Dreze and Amartya Sen, *India: Development and Participation*, pp-328 & Table A6, Oxford University Press, 2002,; Cost of Cultivation of Principal Crops in India, Directorate of Economics & Statistics, Department of Agriculture and Cooperation, Ministry of Agriculture, Government of India; supplied by Mr. Praduman Kumar.

Notes:

1. Figures are growth of real wages (1960-61 prices). End point are processed data, see above graph and text; Semi log estimates are computed by a semi log regression based on annual data. Cost of Cultivation data for 2000 is incomplete.
2. * stands for Dreze-Sen & Deaton-Dreze estimate
3. The cost of cultivation data for 2000 has fewer data (very crops) compared with earlier years.

Section 5d: **Details on NSSO survey results – Growth in wages**

Table 23 reports per day wage growth data for two categories of workers – full-time ("regular" or "salaried workers") according to NSSO, and all workers (full-time and part time). Within each category, five categories are specified: three for rural areas (agricultural workers[38], non-agricultural workers and all rural workers), one for urban areas, and one for all-India. The data are presented for three NSSO survey years, 1983, 1993/94 and 1999/00.[39] Thus, wage growth (annualized) data are available for two broad periods: the pre-reform eighties (1983 – 1993/94) and the post-reform nineties (1993/94 to 1999/00).

Table 23: NSSO based estimates of wage growth (PC deflator)

	1983 to 1993/94	1993-94 to 1999/00	1983 to 1999/00
All workers			
Agricultural workers	2.2	3.1	2.5
Non -agricultural workers	2.7	4.8	3.5
All rural workers	2.4	4.3	3.1
All urban workers	2.3	4.9	3.2
All workers	2.4	4.5	3.2
Full time workers			
Agricultural workers	2.5	3.2	2.8
Non -agricultural workers	3.0	5.2	3.8
All rural workers	2.9	5.6	3.9
All urban workers	2.3	5.2	3.4
All workers	2.7	5.7	3.8

Source: NSSO Employment & Unemployment Surveys, 1983, 1993/94 and 1999/00

[38] The survey contains 14 operation codes for rural workers. Agricultural work is all of these codes except code 12 "manual work in non-agricultural activities" and code 14 "activities other than cultivation".

[39] Data for 1987/88 are not analyzed because of unresolved problems in the data.

High average wage growth – biased by growth in wage growth of the rich? No.

Four basic results emerge from the analysis of the NSSO wage and employment data. First, that use of unit-level data along with state and urban/rural price indices (the PC deflator) does affect the results; compared with the "national" one figure inflation yielded by the agricultural labour price indices (as used by Sundaram and Ahluwalia) the more refined real wage growth estimates are lower for the 1980s (agricultural wages are seen to rise by 2.2 percent rather than 2.8 percent) and higher for the 1990s (agricultural wages are seen to rise by 3.1 percent rather than 2.8 percent). Second, that the growth in wages parallels to a surprisingly large extent the pattern reported by the national accounts data on personal incomes, a result in sharp contrast to the mis-match between the NSSO expenditure growth and private final expenditure growth in the national accounts. Third, that the growth in wages shows a sharp acceleration in the 1990s over the growth recorded in the 1980s. Fourth, that unemployment rates, regardless of definition (usual status, weekly status or daily status) does not show any significant increase in the 1990s; indeed, for most definitions, there is a perceptible decline in the unemployment rate. Fifth, given the data on wages and unemployment (shown below), neither the growth in wages per worker nor the growth in wage incomes per capita could have logically declined in the 1990s (a conclusion reached by many experts e.g. Dreze-Sen, Deaton-Dreze, Sen-Himanshu).

The results on wage growth reveal a somewhat different pattern of inequality growth, and especially poverty decline. In sharp contrast to the average growth in mean per capita consumption of 1 % p.a. in the 1980s (in years when CE surveys ostensibly had no problems), average wages rose at a robust 2.4 % p.a. – a gap of 1.4 percent per year. But the NSSO gap (between E&U and CE surveys) widens in a fashion identical to the widening observed with CE surveys and national accounts data. For the 1990s, the gap almost triples to 3.2 % p.a. i.e. survey measured wage growth is expanding at a 4.5 percent pace in the 1990s, compared to survey measured consumption growth of only 1.3 % p.a.. The average wage growth revealed is also consistent with that obtained from national accounts data.[40]

[40] The corresponding wage growth data in the national accounts is the growth in the ratio of private income (national accounts) and the number of workers (weekly status, NSSO employment and unemployment survey). This growth is reported in Table 1. While not popular in some government documents, the weekly status is the generally accepted international basis for employment and unemployment, and is also the definition used by Subramanian?? in his definitive work on factor incomes.

It could be argued that the above figures reflect the possibility that the high wage growth reflects the growth in incomes of the rich. But this is unlikely since the average wage growth is close to the growth in per worker incomes, NA data (Table 2). Further, for the 1980s, the wage growth of the rural poor is higher than the average. Also, the growth in wages of agricultural workers increased from 2.2 % p.a. in the 1980s to 3.1 % p.a. in the 1990s. A marked acceleration, and one consistent with a tightening labor market, or at least a labor market characterized by a decline in labor surplus. Non-agricultural workers did much better in the 1990s – the growth accelerated from 2.7 percent to 4.8 % p.a..

The NSSO wage data are consistent with all the other information available on trends in non-farm income, and the increasing importance of such income for rural workers. Rural workers per se also witnessed a near doubling of their wage growth rate – from 2.4 percent to 4.5 % p.a.. Thus, an inescapable conclusion from the NSSO survey data is that the wages have accelerated, income growth has accelerated and if the CE survey is not showing a marked acceleration (it is not) then the CE survey has most likely under-estimated the growth in consumption for all sectors in the economy – and done so continuously since 1983, and in an accelerated manner (of underestimation) since 1993/94.

The observed wage growth rate is large and alien to the expectations of most observers[41]. Further, the *acceleration* in such growth, is also contrary to the expectations, and writings, of many scholars on Indian poverty. The growth data are not a mis-print and have been verified by the data published in the NSSO documents published for 1993/94 and 1999/00. These data also match the NSSO wage growth estimates first published by Sundaram and reproduced as Table 18. Sundaram results for agricultural workers indicates an annual growth rate of approximately 2.8 % p.a. for both the 1980s and 1990s. Our numbers, comparable to his, are in the column "full-time workers" – we obtain a growth rate of 2.2 percent in the 1980s and 3.1 percent in the 1990s. The reason we obtain different results is due to the choice, and accuracy, of the price deflator. Sundaram uses a national "point" estimate of inflation change in rural India, an inflation rate based on the CPI for agricultural laborers (CPIAL). If the Planning Commission poverty line deflator is used (and one which while based on the CPIAL

[41] When presented with these data, one leading poverty expert sarcastically asked if these growth rates were in nominal terms!

shows different cost-of-living levels and different inflation rates for different areas of the country) the real wage growth, for exactly the same nominal wage growth, is not *constant* in the two decades (as reported by Sundaram) but instead shows the marked acceleration noted above.

In a recent paper, Rosenzweig and Foster have emphasized the development of the non-farm sector in India. Their analysis is based on a panel of households surveyed over two decades (1981 and 1999) by NCAER. Increase in real incomes of rural households revealed by the Rosenzweig-Foster for the seventeen year period 1982-1999 is 3.1 % p.a., a number identical to the 3.1[42] percent growth rate revealed by the NSSO data.

These data (NSSO *and* NCAER) lead to the following safe conclusion: *Rural wages exceeded 4 percent in the nineties, and at a rate double that which was observed in the eighties.* This acceleration is obtained for all workers – agricultural, non-agricultural, rural, urban, male, female, etc.[43] The acceleration was 0.9 % p.a. for agricultural workers, and 1.9 % p.a. for all rural workers[44].

Section 5e: Pattern of unemployment

What about growth in agricultural incomes or earnings, as opposed to growth in wage rates? There have to be extremely peculiar circumstances for wage rates to rise at a fast pace and yet earnings growth to go down. Essentially, unemployment rates have to not only increase but grow vertically to "compensate" for the increase in wage rates and "enable" wage earnings to collapse. But this is theory; it is of some interest to see what the NSSO data itself has to say on trends in unemployment.

Independent of the contradiction inherent in the Sen-Himanshu claim of rising real wages and rising unemployment, NSSO survey data itself does not support their extraordinary claim. Table 24 is a reproduction of a table from the Ahluwalia report on unemployment, and Table 25 averages these data for the two decades. The tables report the unemployment rates according to four different definitions. All but one of these

[42] Obtained as a weighted sum of 2.4 % p.a. (for 10.5 years) and 4.3 % p.a. for six years.
[43] See Bhalla-Kaur(2004), "Getting Even: The Road to Gender Equality". This paper examines in considerable detail the trends in female and male wages, the trends in occupational choice, etc.
[44] As the table makes clear, a worker can both be an agricultural and a non-agricultural worker in rural areas. The classification has been made according to the "operation code" supplied for each individual e.g. manual work, ploughing, sowing etc.

measures indicates an unemployment rate below 4 percent in the nineties; only the daily status measure shows an increase in the unemployment rate of 1.5 percent between 1993/94 and 1999/00. Even this measure, however, shows the unemployment rate to be below the level observed for 1983.

There are no data that support the contention that the unemployment rate was higher in the 1990s. If data for 1993/94 are not included in the 1980s (as really they should not) then the fall in the unemployment rate in the 1990s is steep; if the 1993/94 unemployment data are used to average the data for both the 1980s and 1990s, then the fall in unemployment rate is not as steep.

In any case, a simple logical experiment can yield insight into how less important a flattish unemployment rate is in the context of fast rising wages. Assume there are a 95 people working out of every 100 i.e. an unemployment rate of 5 percent. Also assume that the wage rate is unity. Now assume wages rise at 3 % p.a. for six years i.e. an increase of 20 percent. At the same time, unemployment rises to 7 percent. At the end of the six years, average per person income would be (1.20*93/100) or 1.116 rather than the per person average of (1*95/100) or 0.95 in the beginning year. The increase in average per person income: 2.7 % p.a. vs. the observed increase in per person wage of 3.0 percent.

Table 24 : Unemployment rates - Alternative Measures (Percentage of labour force)

	Usual Principal Status (UPS)	Usual Principal and Subsidiary Status (UPSS)	Current Weekly Status (CWS)	Current Daily Status (CDS)
Rural				
1977-78	3.26	1.54	3.74	7.7
1983	1.91	1.13	3.88	7.94
1987-88	3.07	1.98	4.19	5.25
1993-94	1.8	1.2	3	5.63
1999-00	1.96	1.43	3.91	7.21
Urban				
1977-78	8.77	7.01	7.86	10.34
1983	6.04	5.02	6.81	9.52
1987-88	6.56	5.32	7.12	9.36
1993-94	5.21	4.52	5.83	7.43
1999-00	5.23	4.63	5.89	7.65
All India				
1977-78	4.23	2.47	4.48	8.18
1983	2.77	1.9	4.51	8.28
1987-88	3.77	2.62	4.8	6.09
1993-94	2.56	1.89	3.63	6.03
1999-00	2.81	2.2	4.41	7.32

Source: Ahluwalia report on Employment.

Note: These estimates of Employment are base on NSS data combined with census data. The NSS gives Labour Force Participation Rate(LFPR) and Work Force Participation Rate (WFPR) for four categories: rural male(RM), rural female (RF), urban male (UM) and urban female (UF) separately. We obtain estimates of total labour force and total work force in each year by applying LFPR and WFPR to the total population of each category (RM, RF, UM & UF) obtained from the interpolation for inter-census years. However, for the year 2001 the four component (RM, RF, UM & UF) are not available in the key results of census released in April 2001. Hence, estimates of populations for these four categories made by the Technical Group (which was chaired by Registrar General) in 1996 have been used to combine the component-wise LFPR's and WFPR's to the level of Rural, Urban and All India.

Table 25: Unemployment rate by to various definition (Percent of labour force)

Definitions	1983	1987	1993	1999	1980s	1980s	1990s
Current Daily Status							
Rural	7.9	5.3	5.6	7.2	***6.6***	***6.3***	***6.4***
Urban	9.5	9.4	7.4	7.7	***9.4***	***8.8***	***7.5***
All India	8.3	6.1	6.0	7.3	***7.2***	***6.8***	***6.7***
Current Weekly Status							
Rural	3.9	4.2	3.0	3.9	***4.0***	***3.7***	***3.5***
Urban	6.8	7.1	5.8	5.9	***7.0***	***6.6***	***5.9***
All India	4.5	4.8	3.6	4.4	***4.7***	***4.3***	***4.0***
Usual Principal Status							
Rural	1.9	3.1	1.8	2.0	***2.5***	***2.3***	***1.9***
Urban	6.0	6.6	5.2	5.2	***6.3***	***5.9***	***5.2***
All India	2.8	3.8	2.6	2.8	***3.3***	***3.0***	***2.7***
Usual Principal & Subsidiary Status							
Rural	1.1	2.0	1.2	1.4	***1.6***	***1.4***	***1.3***
Urban	5.0	5.3	4.5	4.6	***5.2***	***5.0***	***4.6***
All India	1.9	2.6	1.9	2.2	***2.3***	***2.1***	***2.0***

Source: *Report of the Task Force On Employment Opportunities,* Ed Raj Kapila & Uma Kapila; Planning Commission Reports on Labour and Employment, Academic Foundation, 2004

Note:
These estimates of Employment are base on NSS data combined with census data. The NSS gives Labour Force Participation Rate(LFPR) and Work Force Participation Rate (WFPR) for four categories: rural male(RM), rural female (RF), urban male (UM) and urban female (UF) separately. We obtain estimates of total labour force and total work force in each year by applying LFPR and WFPR to the total population of each category (RM, RF, UM & UF) obtained from the interpolation for inter-census years. However, for the year 2001 the four component (RM, RF, UM & UF) are not available in the key results of census released in April 2001. Hence, estimates of populations for these four categories made by the Technical Group (which was chaired by Registrar General) in 1996 have been used to combine the component-wise LFPR's and WFPR's to the level of Rural, Urban and All India.

Section 6a: What happened to poverty in India, 1983-1999/00?

One objective of this paper was to evaluate the magnitude, and trend, of poverty decline in India during 1983-1999; in particular, the pattern of the poverty decline during two time-periods, 1983 to 1993/94 and 1993/94 and 1999/00. The end-years chosen are those conforming to the years in which the large or "thick" sample NSSO expenditure surveys were undertaken. As everyone knows, in the early nineties (1991 to 1993 to be precise) major economic reforms were undertaken in India. These reforms, while not radical from an international perspective, were certainly radical from an Indian vantage point. These reforms have also been politically controversial. To this day, politicians argue about the efficacy of these reforms, particularly in terms of removing poverty.

The researchers have followed the political debate, and termed the pre 1993/94 experience as pre-reforms, and the post 1993/94 experience as post-reforms. It is also granted that growth was perhaps higher after the reforms. But was such an acceleration in growth in the interests of the bottom half of the population, the absolute poor?[45]

Like most debates, there are three sides to the issue – two extremes and a middle. On the extreme left, is the recent paper by Sen-Himanshu. It somewhat boldly contends that the 1990s were a lost decade in terms of poverty reduction, and that, at most, there was only a 3 percentage point decline in poverty reduction in the 1990s. And second, that this decline was considerably less than that which occurred in the 1980s.

In the middle, are experts like Deaton-Dreze, and Datt-Ravallion who contend that poverty decline proceeded at more or less the same pace in the nineties as in the eighties. At the extreme right, are Sundaram-Tendulkar and Bhalla, who have both contended (through very different methods and data) that the poverty decline in the nineties proceeded at a faster pace than that witnessed in the earlier decade.

[45] Forgotten in this debate is the observation that for the first thirty-six years of Indian independence (1947-1983), the absolute poverty ratio was stagnant at around 50 percent of the population. Indeed, the poverty ratio observed in 1983, 45 percent, is near identical to that observed in the early 1950s, early 1960s and early 1970s. The debate in India has shifted from concern about constant poverty levels, to about the acceleration in this decline. The economic reforms of the nineties may have in part been responsible for this change of vision i.e. growth makes it possible to talk about a faster rate of poverty decline.

Since all the authors use NSSO data to buttress their claims, it is somewhat difficult to ascertain the truth. Part of the problem is the large numbers of numbers provided by each set of authors. Each study presents information on what happened in each of the large states in India, there are numbers for just these big states, as well as rural and urban India, and there are numbers from the different surveys, and numbers from different methods (each authors own plus what other people are contending and why those numbers are incorrect etc.). As Section 3 documented, the NSSO experiments with surveys have added to the debate, and the confusion. The reader is referred to the various papers to study whether rural poverty in Rajasthan proceeded at a faster pace than urban poverty in Gujarat, and whether a partially 365 day based consumption measure (MPCE5y) is a better indicator of welfare than a partially 30 day based measure (MPCE5m).

In the first (and admittedly) large part of this paper we have tried to assess what all the available data available states about the two determinants of poverty decline – growth in expenditure and change in inequality. Besides the important “statistical” influence of initial conditions, in the form of the congestion of the poor close to the poverty line, growth and inequality change do fully describe the process by which monetary poverty has, or will, decline. Below we evaluate each of the positions (especially the extreme left and the middle) for the consistency of their *own* evidence i.e. are the assessments of poverty decline in each of the papers/positions, consistent with the evidence of growth in consumption and the change in inequality.

The Sen-Himanshu position is examined first, if only because it is the most provocative, and most extreme. The authors believe that there was growth in the nineties, but that this growth was accompanied by a steep increase in inequality. Indeed, that such an inequality increase was comparable to that which occurred in China.

“However, it is also clear that the source of 1990s within-rural inequality increase was not factorial. With urbanization slowing down during the 1990s despite faster growth in urban incomes and with farm incomes at best stagnant in most parts of the country after 1996-97, this was largely an outcome of demographic pressure and of very uneven rural linkages with the highly unequal urban growth. *The big picture*, even ignoring other dimensions of true inequality, for example, the effect of relative price shifts on the poor and inability of NSS to capture consumption of the rich adequately, *is that measured 1990s rural poor to rise by at most a fifth, and of the urban poor only half, of the average national per capita real growth measured by the NSS.*

Nonetheless, although these results from adjusted NSS data may disturb some, it may be noted that of the distributional data presented so far, the 1990s urban Gini increase was similar to that in China, the percentage increase in nominal urban-rural ratio was two-thirds that of the Chinese, and the rural Gini increase in India was less than half. This still leaves space to argue that India's relative failure was in growth and not distribution, provided of course that the unequal growth observed in both countries is considered inevitable and put beyond policy discourse". (p.1436, emphasis added).

The national per capita growth that Sen-Himanshu believe in is that revealed by the growth in consumption of MPCE5y (adjusted *downward* to account for the 7 day bias in affecting food expenditures). The measured growth between 1993/94 and 1999/00 (on the basis of MPCE5y) is 8 percent. In the above quote, we obtain the relative growth rates believed by Sen-Himanshu but not the "national per capita growth". It is believed by the authors to be below 8 percent, but how much below is unclear. A large part of their paper is devoted to establishing that this growth is an *over-estimate* of actual growth, the over-estimate caused by the 7/30 day food problem. Noticeably, the authors do not account for the possibility that the change in questionnaire for the 30/365 non-food goods could have biased the aggregate growth in the means. A likely possibility, as was documented in Section 2).

Though the authors do not provide an exact estimate of the bias in growth, it can be inferred that it was in the order of at least a few percentage points e.g. cumulative 3 to 5 percent rather than a cumulative 8 percent. Indeed, the authors assert that the abridged schedule for the parallel E&U survey, the one which showed the level of per capita consumption to be about 10 percent below the CE survey, was broadly accurate. This survey yields a cumulative absolute decline of per capita mean consumption in 1999/00, relative to 1993/94, of minus 2 percent! In other words, it may indeed have been the case (according to Sen-Himanshu) that there was zero per capita consumption growth in the six-year reform period 1993-1999! Even if poverty decline between 1993-1999 was not as large as 3 percentage points, even a 1 percentage point decline in the poverty rate (with near zero growth in per capita consumption) would make the 1990s a miracle period of poverty reduction.

While not providing an explicit average for the national growth rate, Sen-Himanshu provide explicit estimate the growth in expenditures of the poor (the target for policy) as being only one-fifth of the national average. That of the urban poor – one half the average.

When these growth estimates are imposed on the data, two results obtain. First, a poverty decline of *only 1.6 percentage points* is obtained – from a base case of 30.7 percent in 1993/94 (MPCE5y definition) to a 29.1 percent adjusted level for 1999/00. This is about half the maximum deemed possible by the authors; the no inequality change estimate is about 6.5 percentage point decline in poverty. Second, the inequality change is, surprisingly, very small: from a Gini level of .3011 to a Gini level of .3127, a 4 percent deterioration. The variance of logs change is 9.2 percent. In neither case is it even close to the worsening of inequality in China.

Given their assumptions, the inequality result seems out of sync with the other results – the worsening of inequality, as expected by the authors, should have been substantially higher. One interpretation of the Sen-Himanshu assumptions and results is to conclude that their inequality change is inconsistent with their calculations of poverty decline, with their conclusions of per capita consumption growth, and their conclusions of the 7 day bias on total expenditures.

The NSSO wage data also sheds some light on the Sen-Himanshu growth rates. Wages for poor agricultural workers (adjusted by the poverty line deflator, as are consumption data above) grew at about a 3.0 percent pace in the nineties; for all rural workers, wage (and earnings) growth was upwards of 4 % p.a.. In other words, the maximum cumulative *six year* or seventy two months Sen-Himanshu rural consumption growth is achieved by NSSO wage growth of the poorest of the poor in *six months*! For all rural workers, the Sen-Himanshu growth is achieved in approximately 4 months. One of these numbers (Sen-Himanshu or wage data from all sources) is radically out of sync with the "truth". Their poverty numbers fail the simple 'smell" or "duck" tests.

Dreze-Sen & Deaton-Dreze:

These two reports contain two somewhat different conclusions: either the pace of poverty decline was the same in both the pre and post-reform periods, or that it was worse in the post-reform period. The authors also conclude that the wage growth of agricultural workers decelerated to half the pace of the eighties. The authors also conclude that there was a pervasive increase in inequality. This would imply that the weight of the evidence, and reasoning, is towards the Sen-Himanshu conclusion: poverty decline significantly slowed down in the nineties.

However, the Deaton-Dreze estimates of poverty decline fail to support this conclusion. For rural areas, the authors find that the decline was the *same as that which would have occurred with no inequality change*, and for urban India the decline was slightly worse than the no-inequality change estimate. These conclusions are under the assumption that the growth rate of urban and rural areas is as estimated by the authors, with the rural growth being significantly lower than urban. A counter-factual, estimated by the authors, is the estimation of poverty decline between 1993/94 and 1999/00 if all had shared equally in average growth. In this scenario, they find that the poverty level in 1999/2000 would have been 21.4 percent compared with the 22.7 percent that is estimated by them as having occurred. Thus, their estimate of the "cost" of the inequality increase is a 1.3 percentage point less poverty decline than that which could have occurred. With no change in inequality, and according to their own DT price deflator, the base 1993/94 level of poverty was 29.1 percent. So the percentage loss in poverty decline, due to the increase in inequality, is (1.3/7.7) or only 16 percent. Not small, but not large either[46], and not indicative of the effects of inequality increase witnessed a la China.

A failed consistency check of the Deaton-Dreze conclusions is on the rate of growth of wages of agricultural workers. Far from halving, such wage growth increased by about a third (i.e. acceleration of wages from 2.3 to 3.0 % p.a.). No evidence was found (by us) to support the Deaton-Dreze claim of a halving of growth of agricultural wages, including

[46] This is also the conclusion reached by Bhalla(2004c) who shows that for developing countries, inequality change accounts for less than 10 percent of poverty decline. Bourguignon(2003) concludes differently: his estimate of the contribution of change in inequality to poverty decline is around 50 percent. In a recent paper, Ravallion(2004) estimates that on average, the elasticity of inequality change with respect to income growth was zero in the eighties and 0.12 in the nineties. This is a very low number; it indicates that with 20 percent cumulative per capita growth, one can expect an inequality increase of only 2.4 percent!

the "original" source cited by the authors, the Agricultural Wages in India, series published by the Directorate of Economics and Statistics[47].

Sundaram-Tendulkar results on poverty decline

In their analysis, the authors do not incorporate any adjustments to the 1999/00 CE survey. They are well aware of the problems caused by the change in the questionnaire in the 1999/00 survey (though they fail to incorporate the possibility that the reported growth in nonfood365 is *downwardly* affected by the *absence* of 30 day questions on the same goods). The authors first prove, by employing use of unit level data on consumption for different goods in the 1999/00 E&U survey[48], that the bias caused by the presence of 7 day food questions did not affect/bias the responses to the questions on consumption of 30 day food. Once this is established, the authors proceed to estimate the growth, and poverty decline, between 1993/94 and 1999/00 on the MPCE5y definition. For the 1983 to 1993/94 period, the authors use the MPCE5m definition of consumption. This comparison yields the result that the pace of poverty decline was significantly higher in the nineties than in the eighties, a result in contrast to that obtained by either Sen-Himanshu or Deaton-Dreze.

But this is inconsistent with the results on inequality change according to the MPCE5y definition, something the authors believe is comparable for both 1993/94 and 1999/00. According to the MPCE5y definition, inequality worsened by 6 percent (MPCE5y Gini in 1993/94 was .3011; that in 1999/00 was .3197). The reason Sundaram-Tendulkar obtain a "faster" rate of decline in the 1990s, and therefore an estimate suggestive of "better" performance, is because of what was outlined in Section 2(a) i.e. the elasticity of poverty change with respect to expenditure growth is itself a function of the head-count ratio in the initial time-period. This functional dependence is inverse i.e. lower the base period head count ratio, the higher the elasticity, *ceteris paribus*.

The authors do not explicitly calculate changes in inequality, nor do they note that the inequality as revealed by the 1999/00 E&U survey is identical to that reported in the 1993/94 CE survey i.e. no change in inequality.

[47] Note that we have not commented upon the relative accuracy of the AWI series; there are reasons to believe that it is not very accurate, and certainly nowhere as close to being accurate as the NSSO wage series.

[48] These unit level data are not available on the CD Roms available to the public.

Section 6b: **Estimating Poverty in 1999-00 from Wage Growth during 1983-1999**

Given the interest in the subject of the efficacy of reforms and poverty reduction in the post-reform period, and at least the theoretical problems caused by the 7/30 day problem, there is merit in finding out what NSSO non-consumer expenditure data suggests about the magnitude of poverty decline. The NSS survey data on wages of casual workers in rural areas (the lowest paid workers in the country) can be used to estimate a *lower bound* to average consumption, and therefore an *upper bound* to poverty, in 1999-00. Since the absolute poor can be assumed to have zero savings, changes in wage income are expected to translate into changes in mean consumption. These data suggest a very different story of India's development than that indicated by the NSS expenditure surveys.

What level of poverty in 1999/00 is suggested by growth in rural wages, 1983-1999? The survey data for 1999 cannot be used for the forecast because the survey in 1999/00, apart from facing methodological contamination in the form of 7 versus 30 day recall for food consumption, also faces contamination in the form of the survey capturing about 17 percent less of national accounts than the 65 percent captured by the 1983 survey[49]. If the distribution of consumption did not change between 1983-1999 (and all indications are that it did not) then the 1983 distribution, and if the growth in the wages of the poorest, agricultural workers, are allocated to all (at 2.5 % p.a. for 16.5 years) an adjusted poverty level for 1999/00 can be estimated. For example, if real wages grew at 2.5 % p.a. real for 16.5 years, the average real consumption in 1999/00 would be 51 percent higher. The poverty ratio according to the Indian poverty line for this higher level of consumption and the 1983 distribution: 13.3 percent, or half the official level of poverty in 1999/00.

[49] This implies, of course, that the incomes of the poor would have to rise by 17 percent in real terms, between 1983 and 1999 before any decline in the poverty ratio is observed.

Section 7: **Conclusions and a final assessment of what happened**

This paper has attempted to evaluate, using all the evidence available, the performance of the Indian economy on three dimensions: growth, inequality, and poverty. Regarding growth, the overwhelming conclusion is that per capita expenditure growth, as estimated by the NSSO consumer expenditure surveys, is an outlier in reporting only a 1.1 % p.a. average growth for the period 1983-2000. All other survey estimates (ranging from NSSO's own employment and unemployment survey to NCAER to Agricultural Wages in India to crop cultivation surveys) indicate that wages of the poorest, agricultural workers, grew at 3 percent plus in the 1990s, and around 2 to 2.5 percent in the 1980s. For rural workers, the average growth rate for the two decades is estimated via the NSSO surveys to be 3 % p.a., with the 1990s growth rate of high 4.3 percent per annum.

The NSSO CE and E&U surveys were both examined to assess the underlying trend in inequality. While estimates vary by method and definition of consumption and type of price deflator, the overwhelming conclusion is of little change in inequality over the two decades, with a best case estimate of some improvement between the end points of 1983 and 1999/00.

Given the above trend in expenditure/income growth, and the static nature of inequality, a firm result is that poverty declined at a rapid pace, and, given a higher growth rate, at a higher pace in the so-called reform period of the 1990s. If a 'correct" survey based minimum estimate of growth is applied to the NSSO consumption distribution data for 1983, then poverty in India in 1999/00 was not 26 percent as officially stated by the government of India, not higher as claimed by several analysts, but considerably lower at 13 percent of the population.

There was one undeniable, and on the face of it, an unfortunate aspect of growth in the 1990s. Job growth was very slow – about 1.4 % p.a. compared to 3 percent plus growth in the 1980s. How does the Indian "miracle economy" explain the shortfall in employment?

Table 2(a): Economic Growth in India: 1950/51 - 2003/04

Decades	Share of Agri.	GDP	Agri.	Non-Agri	Pop.	NSSO Pop. (15-59)	NSSO Workers (15-59)	NSSO Wages (15-59)
		Growth In						
1950s	57.5	3.8	3.1	4.9				
1960s	49.5	3.6	2.3	5.0	2.3			
1970s	44.5	3.0	1.6	4.2	2.3			
1980s	38.5	5.5	3.7	6.6	2.1			
1983-93	**36.4**	**5.0**	**3.0**	**6.2**	**2.1**	**2.7**	**3.1**	**2.4**
1990s	31.2	5.5	2.6	6.7	1.8			
1993-99	**30.4**	**6.4**	**3.0**	**7.9**	**1.9**	**1.9**	**1.4**	**4.5**
2000s*	25.3	5.8	3.4	6.6	1.6			

Source: Economic Survey, Govt. of India (2003/04)
Notes:

1. Growth figures for 1950s represent annualized growth in respective variables from the 1st year of the decade (1950-51) to the first year of the next decade (1960s), so on and so forth.
2. *Growth figures for 2000s are till the year 2003/04 for which advanced estimates reported in the Economic Survey are used.
3. Decadal share of agriculture is estimated by taking simple mean over the respective decade(s).
4. Wages are real, for all workers, with poverty line as a deflator, and NSSO data for 1983, 1993-94, and 1999-00.
5. Rural wages, non-NSSO data, are from Dreze-Sen and Deaton-Dreze.

The other *side of the story*

It is with great trepidation, therefore, that we offer an alternate view of what happened in the 1980s and the 1990s. This alternate view requires very few assumptions, only two, in fact. First, that the Census figures on population are broadly accurate i.e. population in India in 1983/84 was 719 million, that in 1987/88 it was 799 million, in 1993/94 it was 896 million, and that in 1999/00 it was 1005 million. Second, that the GDP growth figures cited above i.e. the national accounts based figures, are broadly accurate. While there has been considerable debate over the figures for poverty in India, and growth in China, no one to our knowledge has even non-seriously questioned the population or GDP growth figures for India.

The emerging big picture is communicated in the simple statistics just cited. Population growth has been declining – from an average 2.1 percent growth rate in the 1980s to 1.9

% p.a. in the 1990s. Perhaps, an imperceptible decline, but a very important one, as we shall soon follow. At the end of the decade i.e. around 1999/00, the population growth rate had declined to only 1.6 % p.a. (See Table 2a reproduced above).

For purposes of jobs, workforce, wage incomes etc., the age group of concern is the 15 to 59 year olds. The growth rate of this *potential* workforce grew by only 1.9 % p.a. in the 1990s (again, 1993/94 to 1999/00), a steep decline from the 2.7 percent growth rate of the 1980s (again, 1983 to 1993-94). What this simple statistic implies is that the much hyped jobless growth was much more about the *supply of labor* bottlenecks in the 1990s rather than about jobless growth.

This is the case because both under assumption and reality, total output growth (GDP growth) stayed the same or was higher in the 1990s. With a declining rate of growth of the labor force, labor tightening is to be expected, even with the same rate of growth, let alone with sharply accelerating growth. This simple logical exercise negates one of the major results of the "conventional view" i.e. the 1990s growth process was "jobless" because of the pattern of growth; it may have been "jobless", but this had more (relatively) to do with the fact that there weren't enough workers available (unless they were to be imported).

It could still be the case that the tightness in the labor market was only in select sectors of the economy, in particular, the poor rural areas of India. Hence, inequality worsened, rate of poverty decline slowed, etc. There are at least three answers to this extension of the conventional view. First, unemployment data per se does not corroborate this view – i.e. unemployment rates were the same, or declined marginally, over the rates prevailing in the eighties. This is according to all definitions of unemployment – usual status, weekly status, or daily status. Second, the average education level of the unemployed is very high relative to the average (4.0 years of education for the average Indian, compared to 7.6 years for the unemployed Indian in 1993/94; for 1999/00, the figures are 4.6 and 7.2 years, respectively). This just proves the adage that the poor are too poor to be unemployed, a reality forgotten by some poverty experts.

If labor market was tightening, it should show up in numbers on growth in real wages. If tightening proceeds from slackness, then the market could still be tightening without an increase in real wages. The NSSO data on real wages, analyzed earlier, very convincingly shows that the poor, and others, have benefited significantly in the 1990s. Rural wages grew at 2.3 percent in the 1980s, somewhat ahead of urban wages which increased at 1.4 % p.a.. In the nineties, there was a significant across the board acceleration, a doubling in average growth rates. As a result, wage growth in rural areas jumped to a rate of 4.3 % p.a., and those in the urban areas to 4.8 percent.

These results are completely opposite to the conventional wisdom, expounded by several international and domestic experts - that wage growth declined in the 1990s to half the rate of the eighties. It was shown earlier that there is no known source (including that cited by the experts themselves) corroborating this statistic. According to the above figures NSSO data indicates that wages grew faster, at near equal levels in urban and rural areas, and poverty is likely to have declined at a considerably faster rate, and inequality is likely to have remained the same. *And these trends have to have been observed, because of a tightening labor market (or a less slack labor market).*

References

Ahluwalia, Montek S. (2000): Economic performance of states in the post-reform period", *Economic and Political Weekly*, May 6.

Anderson, W.H. Locke. 1964. "Trickling down: The relationship between economic growth and the extent of poverty among American families", Page 511-524, Vol. LXXVIII, No. 4, *The Quarterly Journal of Economics*, November.

Banerjee, A and Piketty, T (2001): Are the Rich growing richer: Evidence from Indian tax data", mimeo, Cambridge.

Bhalla, Surjit S. 2000. Growth and Poverty in India-–Myth and Reality. In Govinda Rao ed. *Development, Poverty and Fiscal Policy: Decentralization of Institutions,* Oxford: Oxford University Press, Oct. 2002.

Bhalla, Surjit S. 2002. *Imagine there's no country: Poverty, Inequality and Growth in the Era of Globalization*, Institute of International Economics, Sept 2002; first draft, 2001.

Bhalla, Surjit S, 2003a, "Recounting the Poor: 1983-99", *Economic and Political Weekly*, Page 338-349, Vol. XXXVIII, No. 4, January 25-31

Bhalla, Surjit S., 2003b. "Crying Wolf on Poverty: Or How the Millennium Development Goals of poverty reduction have already bean reached", *Economic & Political Weekly*, vol 38, # 27, July 5, p. 2843-2856.

Bhalla, Surjit S. 2004a. *Not as Poor, nor as Unequal, as you think: India, 1950-2000.* Report on research project entitled *The Myth and Reality of Poverty in India*, Planning Commission, Government of India, May.

Bhalla, Surjit S.(2004b), "Poor Results and Poorer Policy: A Comparative Analysis of Estimates of Global Inequality and Poverty", CESifo Economic Studies, vol. 50, 1/2004, p.85-132.

Bhalla, Surjit S, (2004c) "Is inequality *irrelevant* for growth and poverty reduction?", mimeo, April.

Bhalla, Surjit S. and Associates, 2003. *End of Asian Poverty?* Report prepared for research project RETA-5917. Asian Development Bank, Manila, May.

Bourguignon, François. 2004. "The Poverty-Growth-Inequality Triangle", Paper prepared for a lecture delivered at the "Indian Council for Research on International Economics Relations", Delhi, February 4.

Datt, Gaurav and Martin Ravallion, 2002, "Is India's Economic Growth Leaving the Poor Behind?" Volume 16, Number 3, Summer 2002, Pages 89-108, *Journal of Economic Perspectives*

Deaton, Angus, (2003a), "Adjusted Indian Poverty Estimates for 1999-2000", *Economic and Political Weekly*, January 25-31.

Deaton, Angus, (2003b), "Prices and Poverty in India, 1987-2000", *Economic and Political Weekly*, January 25-31.

Deaton, Angus, and Dreze, Jean. 2002. "Poverty and Inequality in India: A Re-examination", *Economic & Political Weekly*, vol XXXVII, # 36, September 7, p. 3729-3748.

Deaton, Angus, and A. Tarozzi. 1999. "*Prices and Poverty in India" Princeton University,* Princeton, NJ. Photocopy (13 December).

Dev, Mahendra , "Post-Reform Regional Variation

Dreze, Jean and Amartya Sen, 2002: *India: Development and Participation*, Oxford University Press, 2002

Kakwani, Nanak, 1993, "Poverty and Economics Growth with Application to Côte D' Ivoire", *Review of Income and Wealth* 39: 121-139.

Kapila, Raj and Kapila, Uma, (2002): *Planning Commission Reports on Labour and Employment*, Academic Foundation, New Delhi.

Kundu, Amitabh, (2003), "Urbanisation and Urban Governance Search for a Perspective beyond Neo-Liberalism", *Economic and Political Weekly*, p. 3079-3087, July 19.

Mohan, Rakesh and S. Dasgupta: "Urban Development in India in the 21st century: Policies for Accelerating Urban Growth", mimeo, June.

Planning Commission, (2000), "Mid Term Appraisal of the Ninth Five Year Plan (1997-2002)", Chapter 21, Planning Commission, Government of India.

Planning Commission, (2002), "Report of the Task Force on Employment Opportunities", Chairman: Montek S. Ahluwalia, Planning Commission reports on Labour and Employment.

Planning Commission, (2002), "Report of the Steering Committee on Labour & Employment for the 10th Five Year Plan (2002-2007)", Chairman: S.P. Gupta, Planning Commission reports on Labour and Employment.

Planning Commission, (2002), "Report of the Special Group on targeting Ten Million Employment Opportunities per Year over the 10th Plan Period", Chairman: S.P. Gupta, Planning Commission reports on Labour and Employment.

Ravaillon, Martin 2000, "Should poverty measures be anchored to the national accounts", Economic and Political Weekly, pp.3245-3252, Aug. 26-Sept. 2.

Sen, Abhijit, and, Himanshu, (2004a), "Poverty and Inequality in India – I", *Economic and Political Weekly* pp 4247-4263, Vol. XXXIX No. 38, September 18, 2004.

Sen, Abhijit, and, Himanshu, (2004b), "Poverty and Inequality in India – II: Widening Disparities during the 1990s", *Economic and Political Weekly* pp 4361-4375, Vol. XXXIX No. 39, September 25, 2004.

Singh, Nirvikar, Laveesh Bhandari, Aoyu Chen, and Aarti Khare (2003), "Regional Inequality in India: A Fresh Look", *Economic and Political Weekly*, March 15.

Sundaram, K, (2001), "Employment-Unemployment Situation in the Nineties Some Results from NSS 55th Round Survey", *Economic and Political Weekly,* p. 931-940, March 17.

Sundaram, K, (2001), "Employment and Poverty in the 1990s: Further Results from NSS 55th Round Employment-Unemployment Survey 1990-2000", *Economic and Political Weekly,* p.3039-3049, August 11-17.

Sundaram, K, and, Suresh, D, Tendulkar, (2003a), "Poverty Has Declined in the 1990s: A Resolution of Comparability Problems in NSS Consumer Expenditure Data", *Economic and Political Weekly,* January 25-31.

Sundaram, K, and, Suresh, D, Tendulkar, (2003b), "Poverty in India in the 1990s: An Analysis of Changes in 15 Major States", *Economic and Political Weekly,* April 5-11.

Sundaram, K, and, Suresh, D, Tendulkar, (2003c), "Poverty in India in the 1900s: Revised Results for All-India and 15 major States for 1993-94", *Economic and Political Weekly,* January 15-22.

Tarrozzi, Alessandro (2002), "Estimating Comparable Poverty Counts from Incomparable Surveys: Measuring Poverty in India" Research Programme in Development Studies, Princeton University. Available at http://www.wws.princeton.edu/~rpds.

World Bank, 1991, *Malaysia: Growth, Poverty Alleviation and Improved Income Distribution in Malaysia: Changing Focus of Government Policy Intervention*, No. 8667-MA, June.

Zettelmeyer, Jeromin. 2003. "Bhalla Versus the World Bank: An Outsider's Perspective", *Finance & Development*, June.

Appendix I

An integrated framework for examining poverty, inequality and growth

A consistent framework requires that the definitions of the three important variables be explicit, and clear. There are several definitions of *Poverty*, but the one used here is the head-count ratio i.e. the fraction of the population whose per-capita expenditures (or income) are less than, or equal to, a pre-defined level of expenditures given by a "poverty line". *Inequality* can be measured by several indices (share of expenditures of the bottom 20 percent, the ratio of mean expenditures of the bottom 20 relative to the top 20 percent, the Gini index, the Theil index etc). All these are aggregate indices – the point of departure of our analysis is that what is relevant for poverty reduction is not the decline in *aggregate* inequality but rather the change in inequality *at (or close to) the poverty line*. *Growth* is the least problematic, definitionally, but even with this variable there are problems, and controversy. In particular, as argued in Bhalla(1997), *Imagine* etc. there is a need to be consistent about deriving *growth estimates from the same source as the poverty estimates*. For example, given an estimate of the distribution of expenditures, poverty calculations using *growth* rates as revealed by national accounts will be different than the poverty estimates which use household survey growth rates. There is no comment here on which is the correct procedure – only that when exploring the growth-poverty relationship, one has be sure that both are obtained from the same source.[50]

If growth and poverty are consistently defined, then a straightforward identity relationship exists between the two i.e.

(1) $$H_t \qquad F(Y_t)$$

Where H is the head-count ratio at time t and $F(Y_t)$ the cumulative distribution of per-capita expenditures. To every level of cumulative expenditure Y, there corresponds a unique level of the head-count ratio. Thus, H goes from 1 to 0 as Y goes from 0 to . (For a given value of Y, say the poverty line P, the head-count ratio is read as F(P)).

[50] This has been a common error. Among others, both the World Bank's poverty expert, Martin Ravallion, and Nobel laureate, Mr. Joe Stiglitz, have been guilty. In *Imagine*, this confusion was termed the Peter-Paul error i.e. linking growth rate from national accounts, to decline in survey data poverty was akin to attributing Peter's income to Paul's poverty.

This identity makes clear that *monetary* poverty (i.e. poverty in money incomes and not poverty due to lack of education etc.) is explained by knowledge of the level, and distribution of expenditures. Note that there is no simultaneous relationship between the level of income and poverty, nor is poverty a function of either time or other exogenous variables, after controlling for mean incomes and the distribution of such incomes.[51]

Differentiating equation (1) and noting that the derivative of the cumulative distribution F(Y) is the density f(Y), one obtains[52]

$$(2) \qquad dH = f(Y)dY$$

Multiplying both the numerator and the denominator on the right hand side of the equation by Y (*one* value of which is the poverty line, and another value is the mean), one obtains

$$(2') \qquad dH = Y^*f(Y)dY/Y = Y^*f(Y)^*Y'$$

where prime indicates growth. If this equation is evaluated at a value of Y equal to the poverty line P, then equation (2') can be written as

$$(3) \qquad dH = P^*f(P)^*P'$$

where P' indicates the income growth at the poverty line, and f(P) the density at the poverty line.

Equation 3 is arguably *the* equation of interest to policy makers i.e. it answers the question of the change in poverty, in percentage points, for a given amount of growth, Y'. The left hand side of equation (3) is not the *percent change* in the head-count ratio, H', but rather dH or change in *percentage points*.

[51] See Srinivasan(2003) who argues that such is the case.
[52] See *Imagine*, section entitled, pro-poor math, pages 151-58, for a discussion of the formulae presented here

It is important to note, and this becomes critical in the empirical application later, that the left hand side of equation (3) has not growth (percentage change) in poverty but rather just the change in percentage points. In contrast, conventional poverty-growth models have percentage change on both sides of the equation (see below).

Note also that the term on the right hand side of the equation is a product of two terms, the poverty line, P, and the density evaluated at the poverty line, f(P). Since this density refers to the condition existing *prior* to economic change, the RHS of the equation should properly be written as

(4) $dH = \{P*[f(P)]_{t-1}\} * Y'$

What this means is that the coefficient of Y' is known *ex-ante*. But 3' has not been the equation estimated by most analysts; (one exception is *Imagine*). The equation estimated is with percent change in the HCR as the dependent variable. Division of both sides of equation 2' by initial head-count ratio (H_{t-1}) or Ho does yield the "standard" growth-poverty elasticity.

(5) $dH/Ho = Y*f(Y)*Y'/Ho$

(6) $ç = [H'/Y'] = Y*f(Y)/Ho$

The elasticity evaluated at the poverty line, P, is therefore given by

(6') $[H'/Y'_P] = P*f(Y_P)/Ho$

where Y_P represents the mean incomes of the individuals near the poverty line.
In turn, the growth in incomes at the poverty line, Y_P', can be written as the sum of the growth in aggregate mean incomes , Y', and the growth in the *share* of incomes of the fraction X close to the poverty line, Y_X' i.e.

(7) $Y_P' = Y'_M + Y_X'$

Substituting for Y_P'

(8) $$H' = \{P^*f(Y_P)/Ho\}^* (Y'_M + Y_X')$$

Dividing both sides of equation (8) by the mean growth in average expenditures, Y', one again obtains the expression for the growth-poverty elasticity,

(9) $$[H'/ Y'_M] = ç = \{P^*f(Y_P)/Ho\}^* (1 + (Y_X'/ Y'_M))$$

Thus the conventional elasticity, *ç ,* is a function of the poverty line, *and* the initial poverty level. With the latter it is unambiguously negatively related, suggesting that it be problematical if inferences about the nature of growth were made from knowledge about the magnitude of this elasticity.

Equation (9) integrates the trinity – poverty decline, inequality change and average income growth. If it is assumed that inequality change Y_X' is zero, then this equation *reduces* to the conventionally measured elasticity as a special case.

(10) $$[H'/ Y'_M] = P^*f(P)/Ho$$

The central role in the above equations is that of the product P*f(P) and its property that for any period of forecast (or change) it is known before-hand. In *Imagine*, the product P*f(P) was termed the ***"shape of the distribution" elasticity*** or SDE – *this quasi-elasticity yields the total change in poverty that can be expected with a unit change in mean expenditures of individuals clustered around the poverty line*.

Rewriting equation(10), and realizing that income distribution is staying constant, the equation can be rewritten as

(10') $$H' = \{SDE/Ho\}^* Y'_M$$

The implication of both equation (3) and (10) is radical. It means that the poverty elasticity for any growth process is known before-hand. Does that mean that different patterns of growth do not affect poverty differentially? No. Because in equation 10' inequality change has been assumed constant – so , by definition, the only change in incomes that occurs is through growth and it has a pre-determined effect given by the ratio (SDE/Ho).

This, in turn, has a strong implication – one cannot, as has been done by several authors (see Datt-Ravallion(2002)) use the estimate of the elasticity as an estimate of the goodness (or pro-poor nature) of growth. The elasticity will vary with the poverty line chosen, the density at the poverty line, and the poverty level in the previous period!

What happens in the more general case when inequality is allowed to change; in this instance, it is equation (9) which is of interest. Assume for a moment that growth was primarily concentrated in the poor regions of the country and only the poor benefited e.g. the growth in incomes of the poor (Y'_P) was say 10 percent and mean growth, Y', was 3 percent. By construction, there has been a positive inequality change in that the share in incomes of the poor, Y_X', has gone up by 7 percent (from equation 7). What is the predicted impact of this income growth on poverty – exactly equal to the ex-ante value of (SDE/Ho)! In other words, the elasticity has remained the same, regardless of whether inequality improved or worsened.

It is easier to see the impact of SDE in the equation for dH: (rewriting equation 4):

$$(4') \qquad dH = P * f(Y_P) * Y'_M \quad \text{or}$$

$$dH = SDE * Y'_M = SDE * Y_P' = SDE * (Y'_M + Y_X')$$

It maybe a difficult matter to estimate Y_P' , but equation (4') is exact *at* the poverty line P, and quite exact for reasonably large changes in consumption Y_P' . For non-small changes in expenditure, equation (4) does not hold exactly, for the simple reason that these changes in expenditures most likely traverse a large portion of the density function and the arc elasticity estimated by SDE is an average of several “arcs”. However, the approximation for even large changes is very close (see *Imagine*).

The above demonstrated importance of SDE for both within and cross-country analysis has been either not appreciated, or misunderstood, by Ravallion and Zettelmeyer in their respective "reviews" of *Imagine*. Both authors believe that adjustment by SDE is either superfluous or irrelevant!

"However, Bhalla has done nothing more than re-scale the regressor, multiplying the growth rate by a number (specific to each data point) that is typically less than unity". (Ravallion 2002, p.]

"The first exercise involves regressing changes in the poverty head-count ratio on income growth multiplied by a factor, which he calls the "shape of the distribution elasticity," that captures the density of the income distribution at the poverty line. Bhalla estimates a regression coefficient of about unity. But this regression is almost meaningless because the true regression coefficient is unity by construction". (Zettelmeyer, 2003, p.)

It is true that the coefficient for Y_P' is unity by construction (in equation 4), and under the assumption that inequality does not change, the coefficient of income growth is also unity. But empirical economics is sometimes facts in search of a theory; other times it is facts as verification of theory. In neither instance can the pursuit of facts be described as "meaningless". Ravallion's criticism is also not understandable – the regressor is being rescaled differently for different circumstances – this is the method for incorporating initial conditions, something that research and policy analysis should control for.