Nutrition, Poverty and Calorie Fundamentalism: Response to Utsa Patnaik

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Utsa Patnaik's critique ("A Critical Look at Some Propositions on Consumption and Poverty", 6 February 2010) of the authors' earlier paper on food and nutrition ("Food and Nutrition in India: Facts and Interpretations", 14 February 2009) does not stand up to scrutiny. She claims that the observed decline in calorie intake at given levels of real per capita expenditure is an illusion due to faulty price indexes, but does not offer any evidence that the consumer price index actually underestimates cost of living increases. Patnaik's "alternative deflator" and "direct poverty lines" are devoid of any convincing rationale. The charge of miscalculations in the original paper is incorrect, and reflects a misunderstanding of what was done.

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In an earlier paper (Deaton and Drèze 2009), we examined recent evidence on food and nutrition in India, and discussed various puzzles arising from it. Among these puzzles is an apparent downward "drift" in the relation between calorie intake and per capita expenditure (in real terms, i e, at constant prices): calorie intake at a *given* level of real per capita expenditure is declining over time. The drift is sufficiently pronounced to drive down average calorie consumption, especially in rural India, in spite of some increase in real per capita expenditures. We discussed a number of possible reasons for this drift, including changes in relative prices, demographic patterns, food habits, and calorie requirements.

We also acknowledged the possibility that the decline in average calorie consumption might actually be driven by rising poverty, hidden in the National Sample Survey (NSS) data by faulty price indexes, but did not pursue this hypothesis because there appeared to be little support for it. In a spirited rejoinder, Utsa Patnaik (2010) revives this hypothesis and argues that faulty price indexes are also responsible for the puzzling drift, which is actually an "illusion". She further argues that we made mistakes in our calculations of the population below given calorie norms; as we shall show, this is wrong, but we start with her refutation of the puzzle itself.

Is There a Puzzle?

Utsa Patnaik's central argument is that the consumer price index underestimates actual increases in the cost of living. As a result, even if the true relation between calorie and real per capita expenditure (PCE) remains unchanged over time, the observed relation would drift, because of growing overestimation of "real" per capita expenditure (PCE).

The last statement is correct as far as it goes, but Utsa Patnaik does not present any evidence that the consumer price index actually underestimates increases in the cost of living (except for an anecdote about the salaries of university professors). Her paper simply *assumes* that this is the case. In the same vein, she *assumes* that the correct price index to use is a so-called "nutrition-invariant deflator", such that calorie intake remains constant at the poverty line if the poverty line is adjusted over time using that deflator. Naturally enough, this deflator spirits away the puzzling drift, but only by construction. Further, as discussed below, these calorie-based poverty line adjustments can be deeply misleading.

Utsa Patnaik castigates us for using "routine procedures alone", and specifically, for using standard deflators such as the Consumer Price Index for Agricultural Labourers (CPIAL) to convert nominal expenditure into real expenditure. We certainly agree that it would be a mistake to accept those numbers uncritically, and perhaps we should have given them more attention in our earlier paper. However, one of us has written and co-authored a whole series of papers (including three published in this journal) recalculating those indexes using different assumptions and different price measures, those directly collected as well as unit values from the NSS surveys. There are certainly problems (for example, some substitution bias, and the use of outdated weights in which the food share is too high) but there is no evidence from these critical examinations that better price indexes would rise fast enough to resolve the puzzle.

So what does Utsa Patnaik herself do, and how does she come up with an "alternative deflator" that does resolve the puzzle? She makes no attempt to construct her deflator from the mass of price data that are available in India. Instead, she *assumes* that real income must have gone down enough to explain the calorie decline, and then *calculates* the increase in the price index that we would need to explain the puzzle, on the assumption that calories are affected by real PCE and real PCE alone.

Patnaik's alternative deflator is an interesting counterfactual, but it is not a price index in any well-defined sense. Unlike the CPIAL, Consumer Price Index for Industrial Workers (CPIIW), or Deaton's reworkings using unit values, it is not an index of actual prices. Indeed, no prices at all are used in its construction. It simply tells us how much nominal PCE should be deflated to "explain" the calorie decline on the assumption that the relation between real PCE and calorie intake is unchanged. That is an interesting calculation, but if it is to be more than a blue-sky thought experiment, there needs to be some evidence that, contrary to all price indexes so far calculated, the Patnaik deflator is correct. We understand that prices and price indexes are not the same thing, but we simply do not see individual prices, of cereals or other items, growing at the rates that would justify the growth in Patnaik's index. Nor does she offer any direct evidence on prices that would justify her index. So we agree that *if prices had risen as claimed*, the puzzle would be resolved. But we do not see any evidence in the actual price data that anything of the kind happened.

None of this detracts from the possibility that available price indexes do underestimate increases in the cost of living, and that this might explain the puzzle (indeed, we had referred to this possibility in our own paper). Also, contrary to what Utsa Patnaik suggests, we would be quite satisfied if it could be shown that this is indeed the main clue to the puzzle. Beyond that, we certainly recognise that there is a step between a cost of living index, on the one hand, and a price index on the other. But cost of living indexes cannot dispense with actual price data, based on a simplistic assumption that the cost of living is held constant if, and only if, consumption of calories is held constant. How calorie consumption relates to the level of living should be the central object of investigation, and nothing is learnt by simply assuming, without enquiry, that there is a rigid relationship between the two.

Poverty and Calories

Utsa Patnaik's "alternative deflator" is associated with alternative poverty lines that preserve calorie intake (at the poverty line) over time. This approach is superficially appealing, but there are, in fact, very good reasons not to treat calorie intake as an "anchor" for setting or adjusting poverty lines. Indeed, calorie requirements are known to vary between regions, between persons, over time, and so on. This issue was extensively discussed in our paper, and ignored in the rejoinder. What Utsa Patnaik grandly calls "nutrition-invariant poverty lines", or (even more grandly) "direct poverty lines", are just ad hoc poverty lines, devoid of any convincing rationale. This method is like anchoring a ship to an iceberg and hoping that, contrary to all reality, we are safely moored to the land.

The flaws of so-called "nutrition-invariant poverty lines" (calorie-invariant would be a more appropriate term) can also be seen from interstate poverty comparisons based on these poverty lines. As noted in our paper, average calorie intake in India tends to be *higher* in the poorer, less well-nourished areas. As a result, poverty estimates based on calorie-invariant poverty lines lead to very odd regional patterns. For instance, one of the "poorest" states, in this method, would be Kerala, because calorie consumption in Kerala is very low. Yet Kerala has some of the highest PCE levels, lowest poverty rates, and best social indicators (including health and nutrition indicators) among all Indian states. Clearly, the low level of calorie intake in Kerala has little to do with undernutrition, and taking it as a poverty indicator (as happens, de facto, in the "direct" approach) would be highly misleading. Similarly, there are much poorer states, with much higher calorie intake, and much worse nutritional outcomes. Calorie intake and nutrition are simply not the same thing.[1]

In response to this, it may be argued that the calorie "anchor" is applicable over time (to update poverty lines in a *specific* region), but not across regions, because regional variations in calorie requirements and related factors need to be taken into account. But this would, again, be highly ad hoc and unconvincing. If calorie requirements vary across regions, as they do, why would they not vary over time? Is that not a plausible way of explaining the persistent decline in calorie consumption among the *richer* sections of the population, in spite of major increases in standard of living? And if calorie requirements have changed for the rich, why not for the poor? These and related questions, discussed in our paper, would need clear answers for calorie-invariant poverty lines to have any plausibility.

To avoid misunderstanding, we must reiterate that none of this is to dismiss the fundamental problem of massive calorie deficiencies in India, or the need for large sections of the population to have a higher calorie intake. As noted in our paper, had real per capita income increased rapidly enough among the poor, calorie intake would (almost certainly) have risen over time, instead of declining. Low calorie intake is *one aspect* of the undernutrition problem in India. But conflating the two, and assuming that calorie requirements are fixed, does not do justice to the complexity of the problem.

No Mystery

Utsa Patnaik's rejoinder also includes rather unflaterring charges of miscalculations and lack of transparency in the computation of NSS data on calorie intake. However this section of her paper (pp 76-77) is incorrect.

Patnaik questions the figures presented in our paper (Table 5) for the proportion of the rural population living in households with per capita calorie consumption below 2,400 – specifically, the figure of 79.8% for 2004-05. She claims that the figure is wrong, and that the method used to calculate it is unstated as well as non-transparent. Following on that, she launches "detailed statistical detective work" (sic) to uncover the procedure used and the mistakes involved. All this is highly misleading, and reflects Patnaik's own misunderstanding of our very straightforward calculations.

There is no mystery about what we did, which is explained rather precisely in the paper, and there is no need for elaborate detective work to understand it, though it does require analysis of the unit record data. The NSS calculates, for each household in the sample, the number of calories consumed over the last 30 days. We divided the NSS figures by 30 to put them on a daily basis, and then divided by the number of people in the household to obtain daily per capita calorie consumption. We then counted the number of people who lived in households

with daily per capita calorie consumption below the 2,400 norm, and expressed it as a fraction of the sample (or, since we used weights, as an estimated fraction of the population). These are the numbers in our table. They are simple headcounts based on NSS calorie figures, and we believe them to be correct.

Note that this calculation is straightforward. In particular, it makes no use of a poverty line expressed in PCE, nor is it affected by the distinction between Uniform Reference Period (URP) and Mixed Reference Period (MRP).[2] The issue does not arise because there is no reference to PCE at all. Nor do we (or for that matter the NSS) assume, along with Charlie Chaplin, that we can make soup out of shoes, or obtain calories from non-food items in any other way. We simply count the number of people whose calorie consumption is below the cutoff. We would have been happy to explain this in more detail if asked, and to save a lot of unnecessary detective work, but we were not asked, and the procedure is so simple that we thought it needed no more explanation. Indeed, it takes some ingenuity to make it complicated, and to turn it into a mystery story.

What does Patnaik calculate, and why is it different? She starts by calculating the level of PCE at which, on average, household per capita calorie consumption is equal to the 2,400 norm. She then asks what fraction of the population has a PCE below that cutoff. The answer to this question, as Patnaik notes, depends on whether PCE is defined by MRP or URP, something that is not true for our simple and direct calculation. But the fraction of the population whose PCE is below Patnaik's PCE cutoff is not the same as the fraction of the population whose calorie consumption is below the 2,400 calorie norm. This is because there are some people below Patnaik's PCE cutoff who consume more calories than the norm, and some above the PCE cutoff who consume less than the norm. The former are excluded from our calculation, but not from Patnaik's calculation, while the latter are included by us but excluded from Patnaik's calculation. In general, the two sets of differences do not cancel out. So it is entirely possible for both calculations to be correct with Patnaik's calculation giving a larger fraction than ours. However, Patnaik's calculation will not correctly estimate the fraction of the population whose calorie consumption is below the norm. In our calculation, we did exactly what Patnaik recommended, "go always to the basic NSS data source and take the trouble to understand the data" (p 80). Had she done the same, and used the unit record data, she could readily have replicated our results. Instead, she makes a futile attempt to replicate them from summary tables (published in the NSS reports) that do not carry the required information – hence the confusion.

Case Dismissed

In short, none of the charges made in Utsa Patnaik's rejoinder stand up to scrutiny, and her own "calorie fundamentalist" approach is quite misleading. Yet we entirely agree with her concluding words to research scholars, advising them to question received wisdom, avoid shortcuts, scrutinise the data, and "think out of the box". This advice would have carried more weight had she headed in a more productive direction as she rushed out of the box.

NOTES

1 The anomalies of "direct poverty lines" and "nutrition-invariant deflators" are also evident in Utsa Patnaik's own data. For instance, according to Table 2 of her paper, real per capita expenditure has sharply *declined* for all sections of the *urban* population (except the top and bottom 5%) between 1993-94 and 2004-05, with an average decline as large as 42%, and even 70% or so among some of the better-off classes. This is hard to square with independent data, or for that matter with common sense.

2 There is, thus, no basis for Utsa Patnaik's repeated charges of obfuscation on this count, such as: "They have not mentioned the poverty lines at which they obtain these percentages, and neither the data for the ogives they use, nor for the g relation are given" (p 76).

REFERENCES

Deaton, A and J Drèze (2009): "Food and Nutrition in India: Facts and Interpretations", *Economic & Political Weekly*, 14 February.

Patnaik, Utsa (2010): "A Critical Look at Some Propositions on Consumption and Poverty", *Economic & Political Weekly*, 6 February.