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**PRELIMINARY AND INCOMPLETE**

# Consumer price indexes, purchasing power parity exchange rates, and updating

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**Abstract**

PPPs are calculated at each round of the ICP, using multilateral, symmetric, and transitive price indexes. From one round to the next, the PPPs change in a way that, at least in the last two rounds, has been consistently different from what would be predicted from the changes in domestic consumer price indexes. Between rounds of the ICP, the World Bank and the Penn World Table use local CPIs to update the PPP and when the 2005 ICP results became available, there was a large change in many PPPs for 2005 compared with earlier published estimates for the same year, typically with the price levels in poor countries revised upwards relative to rich countries. The same happened for the 1993 round of the ICP relative to the 1985 baseline. It is generally understood that changes in PPPs will not be the same as changes in relative CPIs, even in the absence of a multitude of practical issues such as the treatment of the trade balance, the different commodity lists for CPIs and PPPs, differential hedonic quality adjustment in different countries, and changes in procedures from one round to the next. I focus here on the difference between changes in PPPs and changes in CPIs that comes from the difference in their weights; the symmetric indexes that must be used for PPPs, even for two countries, use both countries' weights, while within-country indexes, such as CPIs, use only the weights for one country. I use the data from the 1993 and 2005 rounds to show that the discrepancy is both substantial and systematic, and will generally lead to an inter-round upward revision of the PPPs for poor relative to rich countries. Even so, the effect, which is very small within OECD countries, is not as large as the actual revision in 2005. I make some suggestions for updating, and clarify the relationship with the Balassa-Samuelson effect and the possible use of the Balassa-Samuelson effect for PPP updating between rounds. Much remains to be understood.

**1. CPIs and PPPs in theory**

Imagine an ideal world in which the prices collected for domestic CPIs and the ICP are the same, there are no differences across countries in hedonic quality adjustment, and there are no changes in procedures in the ICP from one round to the next. Even in this ideal case, there will generally inconsistencies between changes in CPIs and changes in PPPs from one round to the next, see for example Locker and Faerberg (1984), Dalgaard and Sorensen (2002), Hill (2004), Rhoades (2004), Biggieri and Laureti (2011), and McCarthy (2011). This paper explores one of the reasons for these inconsistencies.

To fix ideas consider a two country case where preferences are identical and homothetic. Write the common cost (expenditure) function as

$$c(u, p) = ua(p) \tag{1}$$

where $a(p)$ is a linearly homogeneous scalar function of the price vector *p*. If the two countries price vectors are $p_1$ and $p_2$, the rate of change of the domestic CPIs are given by

$$d \ln P_i = d \ln a(p_i) \tag{2}$$

for *i*=1, 2. The log of the PPP for 2 relative to (base country) 1 is

$$\ln PPP_2 = \ln a(p_2) - \ln a(p_1) \tag{3}$$

From (2) and (3), we have consistency between PPP updating and CPI updating, i.e.

$$d \ln PPP_2 = d \ln P_2 - d \ln P_1 \tag{4}$$

which is what everyone would like to happen.

But (4) is very special. In particular, it stops working as soon as we have non-homothetic tastes, even if we maintain identical tastes across countries. In general, we can write, in place of (2),

$$d \ln P_i = \sum_n \frac{\partial \ln c(u, p_i)}{\partial \ln p_{in}} d \ln p_{in} = s_i' d \ln p_i \tag{5}$$

For budget shares $s_{in}$ for good *n* in country *i*. The differential rate of price inflation can then be written

$$d \ln P_2 - d \ln P_1 = s_2' d \ln p_2 - s_1' d \ln p_1 \tag{6}$$

The PPP index, if it is to satisfy symmetry, must involve *both* sets of budget shares. The algebra is simple if I use the Törnqvist index, so that

$$\ln PPP_2 = 0.5(s_2 + s_1)'(\ln p_2 - \ln p_1) \tag{7}$$

Comparing (6) and (7), we have, instead of the simple (4),

$$d \ln PPP_2 = (d \ln P_2 - d \ln P_1) - 0.5(s_2 - s_1)'(d \ln p_2 + d \ln p_1) \tag{8}$$

so that, without homotheticity, the rate of growth of the PPP is not the same thing as the differential rate of change of the two consumer price indexes. If the second term is important, CPI updating of PPPs will fail, not because of errors and differences in procedures, but because the two concepts are distinct. This must be the case if tastes are non-homothetic, and if the PPP uses an index that is symmetric across countries, thus ruling out the use of only one set of weights.

Equation (8) also says that if we want to update PPPs in order to get as close as possible to the PPP that would be calculated directly, we need to make an attempt to calculate the second term on the right hand side of (8); at the end, I shall discuss how this might be done. Updating using only the CPIs will, in general, not give the correct answer.

Equation (8) also allows us to say something about the discrepancy between the growth of the PPP and of the differential CPIs. The major difference between poor countries and rich countries in budget shares is that poor countries have higher shares on food, and lower shares on other things, including services. Food is a largely tradable good and so is relatively expensive, while many services—

most notably housing, or local labor intensive goods—are largely non-tradable and so are relatively cheap. Over time, as poorer countries grow richer, the parities the cheaper services can be expected to rise relative to the parities of the more expensive foods. This will induce a negative correlation between the differences in budget shares and the rates of change of the parities; the goods that are relatively heavily consumed in poor countries are those whose parities are rising least rapidly. If this is the case, the last term on the right hand side of (8) will be *positive*, so that the PPPs will rise more rapidly than the differential rate of increase of the consumer price index. This is what happened between the 1985 and 1993 rounds of the ICP, and again between the 1993 and 2005 rounds, so that the explanation for the discrepancy is at least in the right direction. Of course, there were many other things going on too, and we so far know nothing about the size of the effect. That is the task of the next section.

Note finally that the changing relative prices of tradables and non-tradables recalls the Balassa-Samuelson theorem, and indeed the stories have similarities. But they are not the same thing. The Balassa-Samuelson theorem explains the divergence between the rate of change of the *market exchange rate* and the differential rate of change of the CPIs whereas here, we are concerned with the divergence between the *purchasing power parity exchange rate* and the differential rate of change of the CPIs. In most accounts of Balassa Samuelson, this last difference is absent by assumption. I shall return to this issue in the final section on updating.

**2. Empirical evidence**

The ICP 2005 provides data on expenditures and parities (prices relative to the US) for basic headings of consumption, investment, government expenditure, and the foreign balance. I use only the consumption data, and aggregate the categories to match the more aggregated data from the 1993–96 round which distinguish only 26 items of consumption. Expenditures (and shares) are aggregated by summation; for parities, I use the budget shares within groups to obtain weighted averages.

In order to abstract from multilateral index issues, as well as the full complexity of calculating the PPPs as done by ICP2005, I look only at bilateral Törnqvist indexes with the US as base. The correlation

between these short-cut consumption PPPs (bilateral, intransitive, and using aggregated basic heads) and the actual consumption PPP from 2005 is a remarkable 0.9992 in logs for the 100 countries represented in both rounds and is still 0.9763 for the consumption prices (consumption PPP divided by the exchange rates). The scatter is shown in Figure 1; the outlier at the top right is Zimbabwe while those at the bottom left are Tajikistan and Kyrgyzstan. While the correlation is not perfect, the bilateral indexes allow me to use the formulas from the previous section without modification.

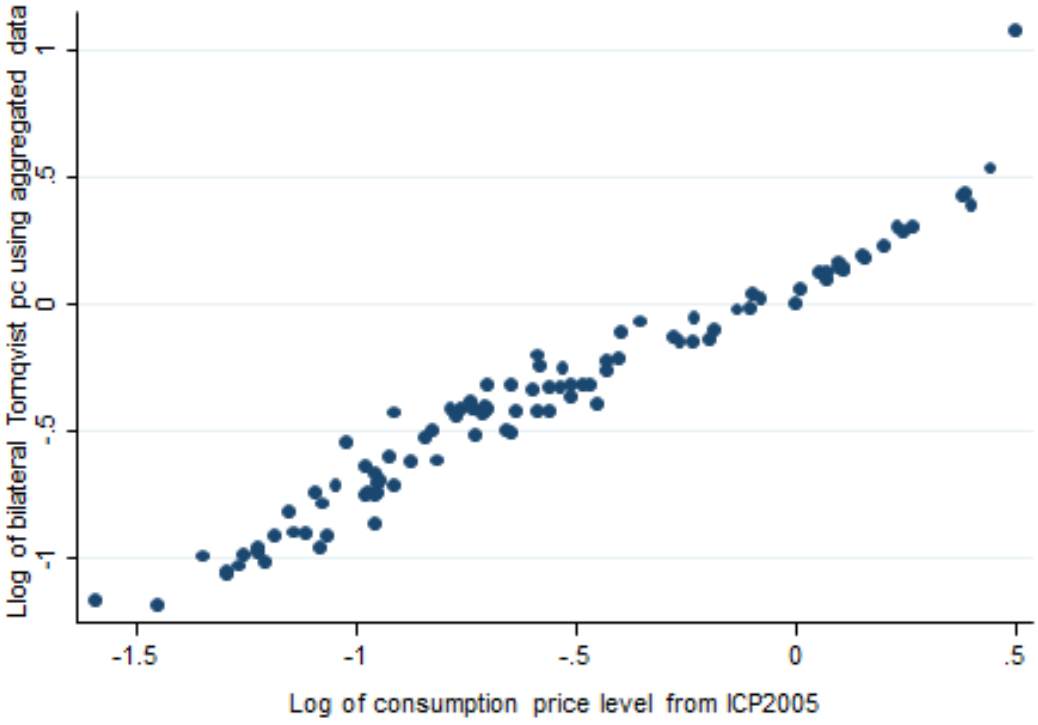


**Figure 1: Consumption prices in 2005: actual and bilateral approximation**

I calculate the second term on the right-hand side of (8)—the excess of the change in the log PPP over the change in the log ratio of the two CPIs—taking the 1993 shares as the baseline shares, and the parities from 1993 and 2005 for the prices. Note that I do not have price levels for either round, only parities relative to the US, but (8) can be written in this form. Updating the notation, I calculate

$$\Delta_i = -0.5(s_i^{93} - s_{USA}^{93})'(\ln \pi_i^{05} - \ln \pi_i^{93}) \tag{9}$$

where $\Delta_i$ is the calculated discrepancy for country *i*, the superscripts 93 and 05 index the two ICP rounds, and $\pi$ is vector of parities.


**Figure 2: Contribution to PPP growth of weighting difference versus lnGDP**

Figure 2 shows the plot of (9) against the logarithm of GDP per capita in 2005 international dollars. As anticipated, the calculated correction is larger the poorer the country; the correlation is –0.39. If the correction is ignored, we will be progressively understating international consumption inequality, only to see a jump at the time a new ICP round becomes available. The discrepancy is essentially zero for the rich countries while for poor countries, e.g. those with log per capita GDP less than 8, the average discrepancy is about 9 percent. For those countries, we could expect the PPP to rise by 9 percent even if their CPI rose at exactly the same rate as the CPI of the US. As we shall see, this is not large relative to

the revisions that actually took place, but it is a correction that is surely worth having if it can be calculated in advance.

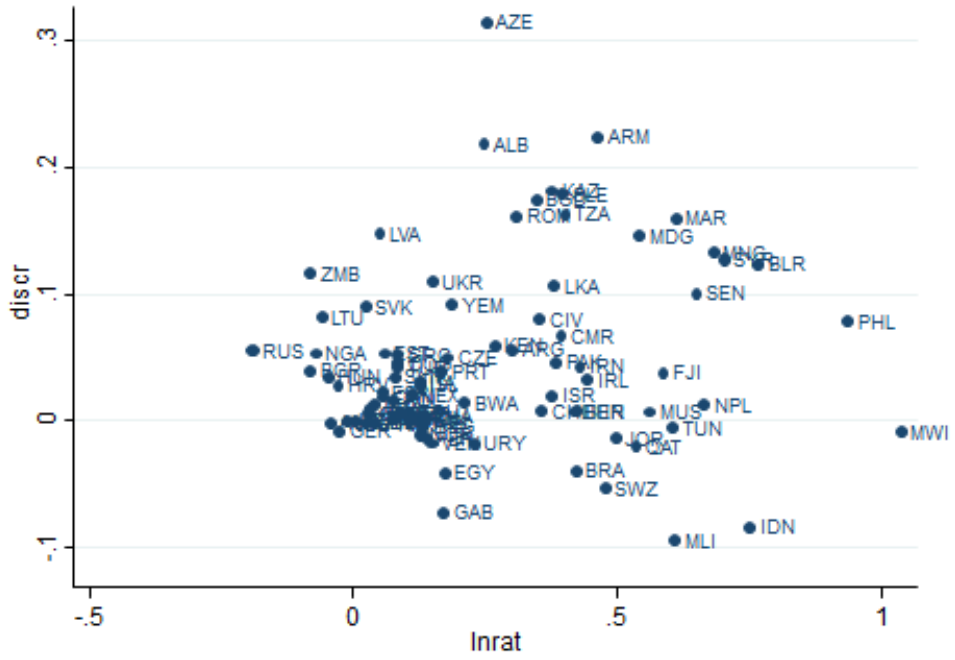


**Figure 3: Actual revisions in log PPP (horizontal axis) versus predictions from weight differences**

Figure 3 shows a plot of the actual revisions—the log of the ratio of the 2005 PPP for consumption after to before the ICP 2005 results—against the discrepancies calculated here and shown in Figure 2. Although the correlation is positive, it is not significantly different from zero, and more importantly, the *size* of the actual discrepancy dwarfs that of the predicted discrepancy. Both are small for the rich countries—recall that the OECD results are updated in between rounds—but if we again look at countries whose log GDP in 2005 is less than 8, the actual average adjustment was 45 percent upwards, more than nine times the predicted upward adjustment of 9 percent.

Why do the formulas here predict so little of the adjustment? There are reasons that are internal to the current analysis as well as reasons that are external, of which the latter seem likely to be more important. Of the internal reasons, I have approximated the continuous derivatives of equation (8) by the

discrete difference in (9). The discrete difference is taken over more than ten years, and the approximation might well be poor. I have also had to aggregate the basic heads for 2005 to match 1993, and I am using only bilateral Törnqvist indexes, not the full multilateral indexes in the ICPs own calculations. Perhaps more importantly, I am not allowing for any difference between the domestic weights that go into the CPIs and the domestic weights that are used in the ICP. The external reasons cover all of the other reasons why the consumption PPP for 2005 differed from the consumption PPP for 1993, all of the changes in procedure, the hedonic quality corrections to some CPIs not others, the differences in the definitions of commodities from one round to the next, and general measurement errors, particularly in the very weak 1993 round. (Note that the trade balance—a major source of adjustment in the PPPs for GDP—plays no role in the consumption PPPs.) That the net effect of all of these adjustments should be large is not a surprise. What remains unexplained is why the direction should be so strongly positive for poor countries.

Greater hedonic adjustment in the US goes in the wrong direction, because it would lead to an overstatement in poor countries CPI relative to the US CPI, so that the poor country price levels would tend to fall when, at each ICP round, only strictly comparable goods are compared. The much more precise definition of commodities in 2005 over 1993 probably increased the price levels in poor countries relative to rich countries, but does not explain why there was a similar effect between the 1985 and 1993 rounds. Nor do the results in Deaton (2010) support the contention that the collection of data on rare and expensive Western goods played much of a role in explaining the high price levels in poor countries. The correction discussed here is in the right direction, but is small relative to the actual discrepancy in 2005.

### 3. How to update between rounds and the Balassa-Samuelson theorem

The correction developed here, equation (8), seems to be worth using in updating PPPs for the period between each round and the advent of the next. Current practice in the Bank is to update the PPPs from the previous round using the differential rate of change of the country's CPI and that of the US, which amounts to using the first term on the right hand side of (8) but ignoring the second. The second term is perhaps more difficult to evaluate than the first, but it still seems practicable. The weights are available

from the ICP itself, and the prices for updating are required only at the basic heading level. They could come either from the implicit price deflators of consumption in the national accounts, or from some disaggregation of CPI data. If, as I suspect, the main effect is coming from food versus other items, it would be possible to get a useful figure with only the very rough disaggregation that is likely to be routinely available.

Note that my claim here is very limited, only that the correction seems worth doing on its own account, and not that it will solve the mass upward revision of poor country price levels that has occurred in the last two rounds of the ICP.

Ravallion (2010) has suggested that updating be done using what he refers to as a dynamic version of the Penn effect, and provides empirical results that support this method over updating using the differential CPIs. I discuss this briefly, and elucidate its links to the updating procedure suggested here.

A classic treatment of the Balassa-Samuelson (BS) effect can be found in Chapter 4 of the text by Obsfeldt and Rogoff (1996). BS is about the discrepancy between the rate of change of the exchange rate, on the one hand, and the differential rates of change of the CPIs on the other. The basic idea is that, apart from high frequency fluctuations, and in the absence of tariffs and transport costs, the exchange rate should equalize the prices of *traded* goods. If the CPI consisted entirely of traded goods, then the ratios of CPIs should move with the exchange rate. However, the CPI also contains non-traded goods, whose parities are unconstrained by trade. In consequence the ratio of CPIs for two countries can move independently of the exchange rate. If the relative prices of non-traded to traded goods are lower in poor countries, and if this is to do with poor countries being poor, then economic growth in poor countries that is higher than in rich countries should work to eliminate the BS effect, so that ratio of a poor country's CPI to a rich country's CPI should increase faster than their exchange rate increases, so that the poor countries should experience real exchange rate appreciation.

If the market exchange rate is denoted by $X$, then the rate of change of the BS effect can be written as

$$\Delta_{BS} = (d \ln P_2 - d \ln P_1) - d \ln X \tag{10}$$

Suppose that country 2 is the relatively poor country—think China—and 1 is the relatively wealthy baseline country—USA. In PPP parlance, (10) is the rate of growth of the Chinese price level, the PPP divided by the exchange rate; in international macro parlance, this is real exchange rate appreciation. If productivity growth in China is relatively rapidly, the relative price of its non-tradable goods will be rising, and its CPI will rise relative to the exchange rate, and $\Delta_{BS}$ will be positive. The international macro literature is rarely concerned with the *level* of the price level—the ratio of the PPP to the exchange rate—but instead focuses on its *rate of change*, which is the right hand side of (10). (This is also why the PPP and macro literatures rarely make contact. The former—the ICP—focuses mostly on the construction and measurement of the levels of PPPs at a moment in time. In the macro literature, with no need to measure levels, the rate of change of the PPP is simply *assumed* to be the relative rate of change of the two CPIs. In terms of (8), the left hand side is equal to the first term on the right hand side, and the last term does not exist. No consideration to the fact that the levels of actual PPPs, as measured by the ICP, use different weights from the domestic CPIs.

What is called the Penn effect is the empirical finding, in accord with BS, that price levels are systematically lower in poorer countries, and what Ravallion calls the dynamic Penn effect is the link between the rate of growth of price levels and the rate of growth of per capita GDP. Write this

$$\Delta_{BS} = \theta_0 + \theta_1 d \ln Y \tag{11}$$

where $Y$ is per capita GDP. If (11) is substituted into (10), we have

$$d \ln PPP = (d \ln P_2 - d \ln P_1) = d \ln X - \theta_0 - \theta_1 d \ln Y \tag{11}$$

This equation is the basis for the alternative updating scheme that Ravallion proposes. The parameters $\theta$ can be estimated from the levels version of (10) using the current round of the ICP, and the change in the PPPs going forward calculated according to the last term in (11), ignoring the first term—the relative rate

of growth of the CPIs—which is the basis for current practice. Ravallion provides empirical evidence that the second term does better—gets closer to the 2005 PPPs from the 1993 PPPs—than does the middle term, and recommends replacing the adjustment procedure.

I can see both advantages and disadvantages to Ravallion's proposal. On the plus side it is superior empirical performance, at least for 2005 ICP from 1993. On the negative side, as (11) makes clear, and as Ravallion also notes, updating with the CPI—the middle term of (11)—should give the same answer. That it does not means that there is something unexplained, and unless we know why the CPIs and the PPPs have diverged in the past, it is hard to be sure that they will continue to do so in the future, or if so, by how much. This would be less of an issue if we could be sure that the Balassa-Samuelson regularity is indeed a regularity, meaning that we can be confident that the parameters of equation (10) will remain stable over time, and that their values estimated in the cross-section will be valid for updating over time. The Balassa-Samuelson effect is driven by differential rates of productivity growth. The validity of (10) requires that there is a stable relationship between per capita GDP and productivity growth, for which there seems no solid foundation. The Penn effect regressions in 1993 and 2005 have slopes that are significantly different from one another, though the differences are not sufficiently large to invalidate updating using the 1993 parameters. It would be good if these issues were better understood than is currently the case.

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