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PRICE LEVELS AND ECONOMIC GROWTH: MAKING SENSE OF REVISIONS TO DATA ON REAL INCOMES

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The price surveys from the 2005 International Comparison Program (ICP) imply substantially lower levels of GDP at purchasing power parity (PPP) for many developing countries than prior estimates. While some observers have questioned the data, this paper argues that the pattern of changes in PPPs between ICP rounds makes economic sense. Consistently with the original Balassa–Samuelson model, more rapidly growing economies experienced steeper increases in their PPPs relative to official exchange rates. This effect was even stronger for poor countries. Taking account of this effect would reduce the need for such large data revisions when new ICP data become available.

JEL Codes: E31, O47

Keywords: China, economic growth, purchasing power parity, real exchange rate

1. Introduction

Purchasing Power Parity exchange rates ("PPPs" for short) have been mainly derived from the International Comparison Program (ICP), which collects data on prices across countries. The 2005 ICP survey round—thought to be the largest international statistical operation ever—collected prices for a great many goods and services, grouped under 155 "basic headings" (corresponding to the expenditure categories in the national accounts) for each of 146 countries in six regions (Africa, Asia-Pacific, Commonwealth of Independent States, South America, Western Asia, and Eurosat-OECD). Region-specific product lists were developed and the regional PPPs were linked through a common set of global goods. The 2005 ICP's governance structure entailed that each of the six regional ICP offices worked closely with government statistics offices in each country, while the World Bank provided global management and estimated the final PPPs. World Bank (2008a) provides estimates of the PPP for GDP and its main components for 2005. World Bank (2008b) compares the results to those based on the main prior ICP round, for 1993.

Some dramatic revisions to past PPPs are implied by the results of the 2005 ICP. Figure 1 compares the estimates of real (at PPP) GDP per capita for 2005 from the World Bank's *World Development Indicators* 2007 database—just prior to

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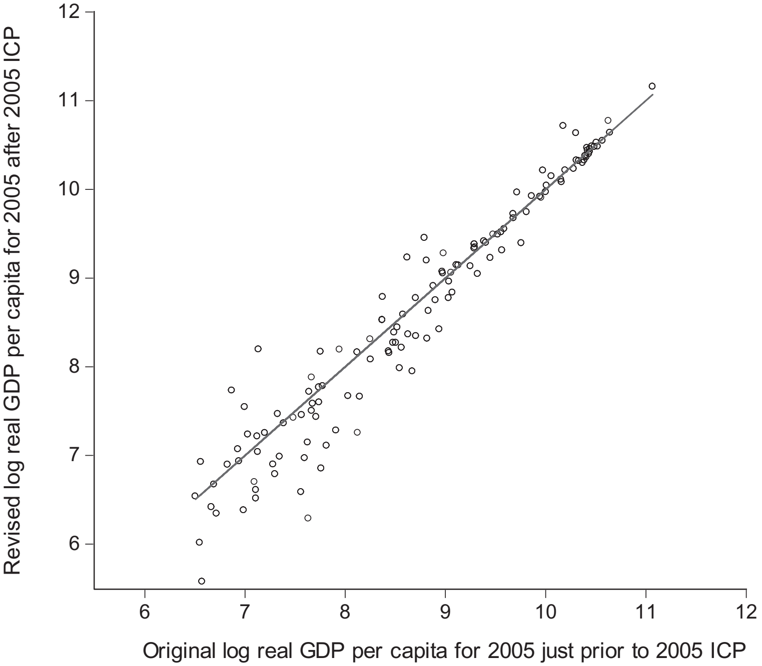

Figure 1. Revisions to GDP Per Capita for 2005 Implied by 2005 ICP

Note: The straight line indicates no revision.
Source: World Bank (2008b).

the release of the 2005 ICP results—with the estimates implied by the 2005 ICP.[1] Due to the changes in PPPs, real GDP was revised downwards for many low- and middle-income countries, and substantially so for some. In the Asia-Pacific region, real GDP for 2005 was revised down by 30 percent (World Bank, 2008b).

These large data revisions have been attributed to the changes in data and methods that were introduced in the 2005 ICP round, as described in World Bank (2008a, 2008b). A potentially important difference is that (compared to prior ICP rounds) stricter quality standards were used in the 2005 price surveys, to assure that the ICP was obtaining prices for internationally comparable commodities. This is important given that one expects that lower quality goods are consumed in poorer countries, creating a risk that (without strict standards in defining the products to be priced) one will underestimate the cost of living in poor countries by confusing quality differences with price differences. With better funding of the ICP in 2005, clearer product descriptions were developed.

Methodological changes between ICP rounds in the price data and in how PPPs were calculated cast doubt on the comparability of the resulting PPPs over time. This is one reason why users often avoid mixing PPPs between ICP rounds,

[1]The least squares regression line has an intercept of −0.580 (White SE = 0.236) and slope of 1.057 (SE = 0.025); $R^2 = 0.938$; n = 136. One can reject the null hypothesis that the original estimate is an unbiased predictor of the revised estimate (i.e., intercept = 0, slope = 1); the F-test is 4.491 with p = 0.013.

letting national price data override the ICP data for inter-temporal comparisons. In other words, the PPP conversion is done at the ICP benchmark year, with national price indices used for inter-temporal comparisons. It has been argued that this is the most reasonable practice, given the changes in methodology (Dalgaard and Sørensen, 2002; World Bank, 2008b; Chen and Ravallion, 2010a). Theoretical arguments have been made both for and against this practice. Nuxoll (1994) argues that the real growth rates measured using local deflators better reflect the trade-offs facing decision makers at the country level, and thus have a firmer foundation in the economic theory of index numbers. Of course, this means that the economic aggregates may well lose purchasing power comparability as one goes further back in time from the ICP benchmark year. In the context of studies of economic growth using PWT, Johnson *et al.* (2009) argue instead that comparisons should *only* be made between ICP rounds, since only then can one be sure that the economic aggregates are consistently evaluated at purchasing power parity.

All this begs a neglected question: *Can we make economic sense of the changes in PPPs?* This paper tries to answer that question by comparing the latest (2005) PPPs with those for 1993 and 1985. Unlike most past empirical studies of PPPs, which have focused on the cross-sectional differences, this paper is concerned with explaining the observed changes over time. The paper focuses on the *price level index*, defined as the PPP for GDP divided by the exchange rate (U.S. = 100 percent);[2] the inverse of this index is sometimes also called the *real exchange rate*. For the same reason that one uses PPPs rather than exchange rates for international comparisons, the paper hypothesizes that the PPP will tend to rise relative to the market exchange rate in a growing economy. In the models of Balassa (1964) and Samuelson (1964), this happens if economic growth comes with higher labor productivity in traded goods (relative to non-traded goods). This can be thought of as a *dynamic Penn effect* (DPE), corresponding to the widely-observed static Penn effect in which the price level index tends to be higher in richer countries.[3] While the bulk of the empirical literature has studied the static Penn effect (using cross-country comparisons), there has been some support for the idea of the DPE in time series data.[4] However, whether one would see the DPE in a growing developing country is a moot point in theory. It can be argued that such economies are characterized by factor-market imperfections and surplus labor, dulling the Balassa–Samuelson mechanism. Productivity increases for non-traded goods could also dull the effect. In addition to the possibility of a DPE, one can expect that there will be measurement errors confounding the PPP comparisons.

[2]While the term "price level index" is widely used in the literature, it is potentially confusing, since the index is a relative price—the ratio of two nominal prices.

[3]The term "Penn effect" appears to be due to Samuelson (1994) and stems from the Penn World Tables (PWT) (Summers and Heston, 1991), which provided the data that were used to establish this effect empirically. Evidence of a static Penn effect in data from various rounds of the ICP is reported in Balassa (1964), Rogoff (1996), World Bank (2008a, 2008b), and Deaton and Heston (2010), amongst others. The most common parametric test entails regressing the log of the price level index on the log of GDP per capita in US$ at market exchange rates.

[4]See Lothian (1990) (using long-run time series data for Japan), Bahmani-Oskooee and Rhee (1996) (for Korea), Lothian and Taylor (2008) (for the UK), Canzoneri *et al.* (1999) (using panel data for 13 OECD countries), and Ricci *et al.* (2008) (using a panel dataset for 48 countries).

A better understanding of the economics of PPP changes should help in estimating PPPs for the (many) years for which there is no ICP round, with implications for the extent of the data revisions needed at each new ICP. There are two distinct problems of missing data in estimating PPPs; the first is that some countries chose not to participate in the ICP's price surveys, and the second is that there are long gaps between those surveys. Existing data compilations (such as the World Bank's (2011) *World Development Indicators*) use extrapolations based on non-ICP data to fill in these missing PPPs, but very different types of data are used. The extrapolations to deal with the first problem are based on GDP per capita at market exchange rates, exploiting the static Penn effect. By contrast, in addressing the second problem, the PPPs for non-benchmark years are estimated by re-scaling the PPP from the most recent ICP round according to the inflation rate (GDP deflator for the GDP PPP, and Consumer Price Index for the consumption PPP) in the country in question relative to the U.S. inflation rate. The reliability of this method is unclear. While in theory, a suitable inter-temporal price index could deliver reliable extrapolations, it is far from obvious how well the methods used in practice perform, notably in reflecting the changes in the relative price of non-traded goods in growing economies.[5]

However, as this paper shows, there is also evidence of the DPE, and this provides an alternative method for the inter-temporal extrapolations—a method that is more consistent with how the first problem of missing data is dealt with. Despite the methodological differences between ICP rounds, the paper shows that the cross-sectional relationship between price levels and mean income also holds over time. This can be exploited in the dynamic extrapolations between ICP rounds, thus offering scope for reducing the need for the large data revisions often implied by each new round of ICP data.[6] Comparability problems remain, but they are evidently only part of the explanation for the data revisions. A sizeable part also stems from the use of a method for updating PPPs between ICP rounds that does not take account of the DPE. Changing that method will allow better estimates between ICP rounds.

The following section summarizes the arguments as to why one might expect the PPP changes to be predictable. Section 3 describes the empirical models to be estimated while Section 4 presents the results. Section 5 tests for whether the 2005 PPP for China—the country that has clearly received the most attention in the debates surrounding the release of the results of the 2005 ICP—is consistent with the pattern seen across countries. Section 6 looks at the implications for extrapolating PPPs between ICP rounds. Section 7 concludes by summarizing the paper's findings and drawing some implications for future analyses of ICP data.

[5]Neither the underlying prices nor the aggregation methods are typically the same between the national deflators and the PPPs constructed by the ICP.

[6]In the only precedent to this alternative approach I know of, Prados de la Escosura (2000) fills in the missing data on GDP at PPP both cross-sectionally and over time using a single model of the price level index, estimated on pooled data for OECD countries.

2. What Might Account for the Observed Changes in PPPs between ICP Rounds?

Some of the country-specific factors that influence price levels (or, equivalently, real exchange rates) can be treated as time-invariant between ICP rounds.[7] By focusing instead on the changes in PPPs, the following analysis will eliminate the influence of all additively-separable error components stemming from such country-specific factors.

An important clue to why PPPs change over time can be found in the very same reason PPPs were developed. It has long been recognized that international comparisons of GDP at market exchange rates are deceptive about the differences in real income, given that some commodities are not internationally traded, notably most services. Without trade, there is no mechanism for assuring price parity across borders. The most common economic explanation of why the PPP would differ systematically from the nominal exchange rate is the Balassa–Samuelson model (outlined independently by Balassa, 1964, and Samuelson, 1964).[8] This assumes a competitive market economy in which all factors of production are fully employed and are freely mobile between the traded and non-traded-goods sectors. The relative price of traded goods is then given by the labor productivity differential between traded and non-traded goods. To see this more formally, let MP_T denote the marginal (physical) product of labor in the traded goods sector and let MP_N denote the corresponding marginal product in the production of non-traded goods. Also let P_T and P_N denote the prices of traded and non-traded goods while W_T and W_N are the corresponding wage rates. Under standard assumptions (including competitive, profit-maximizing, producers) we have $W_T = P_T MP_T$ and $W_N = P_N MP_N$. With perfect labor mobility, we have $W_T = W_N$ in equilibrium. The key relationship generating the Balassa–Samuelson effect is then immediate, namely that $P_T/P_N = MP_N/MP_T$.

In using the Balassa–Samuelson model to explain why PPPs tend to be lower (relative to market exchange rates) in poorer countries, it is assumed that the more developed the country the higher its labor productivity in traded goods, but that productivity for non-traded goods does not vary systematically with level of development. A higher marginal product of labor in traded goods production comes with a higher wage rate, which is also binding on the non-traded goods sector (given that labor is freely mobile), implying a higher price of non-traded goods in more developed countries and thus a higher overall price level. By the same reasoning, low real wages in poor countries entail that non-traded goods tend to be cheaper. The ratio of the purchasing power parity rate to the market exchange rate will thus be an increasing function of income.

This argument helped reinforce the (considerable) international statistical effort that has gone into the development of purchasing power parity exchange rates, led by the *International Comparison Program* (although PPPs existed before

[7]For example, Clague (1985) shows that natural resource endowments will influence the price level index at a given level of GDP per capita. However, such endowments can be treated as country-level fixed effects for the present purposes.

[8]An alternative explanation was proposed by Bhagwati (1984) based on factor endowments, leading (labor-intensive) services to be cheaper in poor countries.

Balassa–Samuelson). The PPP rate expresses a currency's value in terms of its purchasing power over commodities, both traded and non-traded, relative to the numeraire currency (almost invariably the US$). The PPP is based on the prices actually paid for goods; the exchange rate does not directly enter into its calculation (though of course it will matter indirectly, via domestic prices and spending patterns).

The Balassa–Samuelson model offers a theoretical explanation for the Penn effect. Balassa (1964) found evidence that richer countries tend to have higher price levels in data for 12 countries. Since then most empirical tests of the Penn effect have used cross-sectional data from the ICP. Every round of the ICP appears to have confirmed the Penn effect.[9]

The economic mechanism postulated by the Balassa–Samuelson model should also hold over time, as long as the data are in reasonable accord with the assumptions of the model. As a poor country develops, its productivity in the traded goods sector will rise, as will the real wage rate, and so its PPP will move closer to its exchange rate. There is some supportive evidence in time series data for specific developed countries, notably Japan, but not others (Rogoff, 1996). Past tests of whether the implications of the Balassa–Samuelson model (including the Penn effect) hold over time have been largely confined to developed countries.[10]

Arguments can be made for and against the Balassa–Samuelson assumptions. A key assumption is that richer countries have higher relative productivity in traded goods. Balassa (1964) presented (influential) evidence supporting that assumption. But technology has changed considerably since 1964, entailing greater potential for productivity growth in the services sector. Take, for example, India's booming business services sector. This sector has seen very high growth since the early 1990s, facilitated by the availability of skilled labor and changes in information technology.[11] Superficially this does not sound much like the Balassa–Samuelson model. However, it should be noted that this change has also come with a transformation of many business services into internationally traded commodities. So it can be argued that India's rising productivity in services is in fact consistent with Balassa–Samuelson. By this view, it is the presumption that services are non-traded that is now questionable, given technological change.

However, even if growth does come with rising productivity for traded goods, the way labor markets work in reality may not pass this effect fully onto wage rates in the non-traded goods sector. This could happen if there are impediments to labor mobility. For example, labor hiring in the traded-goods sector may be subject to (explicit or implicit) contracts favoring incumbents, leaving the services sector as a residual employer. Or there may be specialized skill requirements, which effectively restrict entry to the traded-goods sector in poor countries with limited human capital. A wedge between wage rates in the two sectors could also arise if the traded-goods sector is the "formal" sector, which is taxed, while services are informal, and un-taxed. Then we may find a persistent wage gap (with

[9]Amongst others, see Summers and Heston (1991), Heston *et al.* (1994), Rogoff (1996), World Bank (2008a), and Deaton and Heston (2010).

[10]The only exception I know of is Choudhri and Khan (2005), who find evidence consistent with Balassa–Samuelson effects in panel data for 16 developing countries.

[11]See Kotwal *et al.* (forthcoming) for evidence on this point.

$W_T > W_N$), creating a potential disconnect between relative prices and relative labor productivities between the two sectors, thus breaking the Balassa–Samuelson effect. Whether that actually happens depends on how the relative wage rate (W_T/W_N) is in fact determined. However, the key point is that with market imperfections it is an open question whether the PPP will start to approach the exchange rate in poor but growing economies, or whether it continues to lag. Given market frictions, one might also conjecture that the DPE only starts to emerge when the growth rate is sufficiently high.

There are also measurement errors in the PPPs. While these are largely unobservable, one clue is that not all of the countries that participated in the 2005 round had participated in 1993.[12] Most of the PPPs for these non-benchmark countries were estimated econometrically by the ICP team using regressors observed for both sets of countries.[13] Any bias in those estimates will be reflected in the subsequent changes observed when the country participates properly in the ICP's price surveys. This too could dull the effect of economic growth on price levels.

3. Modeling the Changes in PPPs

To see how well the changes over time in price levels can be explained by the Penn effect, I have assembled a data file of the price level index (PPP for GDP over exchange rate) and GDP per capita at PPP for the 2005 and 1993 ICP rounds for all countries (developed or developing). These are the World Bank's estimates, rather than Penn World Tables (PWT).[14] I will also study the changes in PPPs between 1985 (using PWT) and 2005.

Let PPP_{ri} denote the PPP exchange rate for country i in year r using ICP round r and let E_{ri} be the corresponding market exchange rate. (I follow convention in defining both exchange rates using the U.S. as the base country. Thus both give the local currency equivalent of US$1.) By definition, the price level index is $P_{ri} \equiv PPP_{ri}/E_{ri}$. This is of course a relative price, interpretable as the inverse real exchange rate. Also let Y_{ri} denote GDP per capita in US$ at the market exchange rate. Thus $Y_{ri} = GDP_{ri}/E_{ri}$ where GDP_{ri} is GDP in local currency units.

The basic empirical model for changes in the (log) price level index is as follows:[15]

$$\ln(P_{05i}/P_{93i}) = \alpha + \beta \ln(Y_{05i}/Y_{93i}) + \varepsilon_i. \tag{1}$$

[12]The specific countries are Albania, Angola, Bhutan, Bosnia and Herzegovina, Brunei, Burkina Faso, Burundi, Cambodia, Cape Verde, Central African Republic, Chad, China, Colombia, Comoros, Democratic Republic of the Congo, Cote d'Ivoire, Cyprus, Djibouti, Equatorial Guinea, Ethiopia, Gambia, Ghana, Guinea-Bissau, India, Israel, Lesotho, Macao China, Macedonia, Malta, Mauritania, Mozambique, Niger, Paraguay, Rwanda, South Africa, Sudan, Togo, and Uganda.

[13]See World Bank (2008a). The log GDP per capita in 1993 PPP was regressed on log GDP per capita at market exchange rates and the log of the secondary school enrollment rate This is equivalent to regressing the log of the price level index on these same two variables (though with different parameters of course). (China's PPP for 1993 was estimated by a different method, as noted later.)

[14]There are methodological differences between the World Bank's PPPs and those in PWT; see Deaton and Heston (2010) for a useful overview of the differences.

[15]Note that both the regressor and the regressand in (1) are in the same units; both $P_{ri} \equiv PPP_{ri}/E_{ri}$ and $Y_{ri} = GDP_{ri}/E_{ri}$ are in current local currency units deflected by the nominal exchange rate.

A number of remarks will help motivate and interpret this model. First, equation (1) can be interpreted as the time-differenced version of the widely used double-log model in the cross-country literature on the Penn effect, incorporating a year effect but common slope (though this will be relaxed later).[16] Second, the test for the dynamic Penn effect based on the regression coefficient in (1) is robust to the choice of base country; changing the latter would simply change the constant term. Nor would deflating for inflation in the base country change the result, as the relative GDP deflator for the base country would also go into the constant term. Third, unlike the cross-sectional specification, estimates of (1) will be robust to any (time-invariant) country characteristics that jointly influence the level of prices and GDP. Fourth, if $\beta > 0$ then there is evidence of a DPE. Fifth, if $\alpha > 0$ (<0) then the 2005 ICP schedule of price levels is higher (lower) than that for 1993 at given GDP per capita.

I will treat the growth rate as exogenous in equation (1). This can be questioned. It has been argued by Rodrik (2008) and Korinek and Servén (2010) that policies promoting a high real exchange rate—implying a lower price level index—can promote longer-term economic growth by stimulating exports. To some extent, these can be thought of as long-run policies that would be within the country fixed effect for the level of the price index, and so be swept away in the time-differenced specification in (1). However, any *changes* in exchange rate policy that impact on growth would still leave a bias. If a higher real exchange rate does in fact promote growth, then the OLS estimate of (1) will underestimate the true value of β. Correcting for this bias would yield even stronger evidence in favor of the existence of the DPE. Note also that the existence of the DPE implies a bias in OLS growth regressions using the real exchange rate as a regressor (as in Rodrik, 2008).

The basic specification in (1) will be augmented in three ways. First, one can think of equation (1) as the restricted form of the following equation:

$$\ln(PPP_{05i}/PPP_{93i}) = \alpha + \beta \ln(Y_{05i}/Y_{93i}) + \gamma \ln(E_{05i}/E_{93i}) + \varepsilon_i. \quad (2)$$

It is of interest to see if the restriction that $\gamma = 1$ (implying equation 1) cannot be rejected. One can, however, question a causal interpretation of equation (2); indeed, the "law of one price" implies the market exchange rate is determined by the *PPP*, although the fact that price level indices are less than unity for many countries discredits this as a model of exchange rate determination, at least in the short term.[17] Nonetheless, it would clearly be worrying if one could reject the null that $\gamma = 1$.

Second, I will allow the DPE parameter to vary with initial GDP. I expect a negative interaction effect, on the grounds that it is the initially poorer countries where higher growth comes with the type of structural change that would put upward pressure on the price level index.

[16]The most common specification for the Penn effect in the literature expresses the log of the price level index as a linear function of the log of GDP per capita evaluated at market exchange rates. Note that this is equivalent to the alternative specification sometimes found in which the log of GDP at PPP is a linear function of the log of GDP at market exchange rates; the slope parameters in these two specifications sum to unity.

[17]Rogoff (1996) reviews the literature.

Third, I will test whether the relationship is any different when the 1993 PPP was derived from actual price surveys—in which case we refer to the country as a 1993 "benchmark country" (following common practice in the ICP literature).

Fourth, I will test a nested model encompassing the above specification and the inflation-adjustment method used by the World Bank's *World Development Indicators* to update PPPs over time between ICP rounds. By this method, the extrapolated PPP for GDP for date t (>1993), using the 1993 ICP round (say) as the benchmark, is given by:

$$(3) \qquad P\hat{P}P_{ti} \equiv PPP_{93i} \frac{DEF_{ti}/DEF_{93i}}{DEF_{tUS}/DEF_{93US}},$$

where DEF_{ti} is the GDP deflator (or CPI when updating the consumption PPP) for country i at date t (where $i = US$ denotes the U.S. deflator). The encompassing test entails adding a term in $\ln(DEF_{05i}/DEF_{93i})$ to equation (2). If one cannot reject the joint null that the coefficient on this extra variable is unity, while $\beta = \gamma = 0$, then the inflation-adjustment method is supported.

4. Results

I will mainly use the PPPs for GDP for 2005 and 1993, both of which were estimated by the World Bank in collaboration with other international agencies.[18] There are a number of differences in the data and methods used by these two ICP rounds.[19] The 1993 ICP was the first round for which the Bank estimated global PPPs. I will also make some comparisons with the 1985 PPPs from PWT, though it should be noted that there are a number of deeper methodological differences with the World Bank's PPPs (as summarized in Deaton and Heston, 2010).

Figure 2 gives the empirical density functions (using normal kernels) for 1993 and 2005. The price level index is below 100 percent for the bulk of the data in both years. Even so, the index rose for 74 percent of countries. Furthermore, it rose by 10 percentage points or more in almost half the countries (44 percent to be precise). For 2005, the (unweighted) mean price index across the 133 countries that participated in the 1993 ICP was 59 percent (median 49 percent) as compared to 53 percent in 1993 (median 41 percent). The mean $\ln(P_{05i}/P_{93i})$ is 0.162, with a standard error of 0.026 (n = 133).

So an upward revision to the price level is generally indicated. However, the bulk of this was for countries with initially low price levels. Indeed, the cumulative distribution functions implied by the densities in Figure 2 are virtually indistinguishable for price levels above 60 percent.

Regressing $\ln(P_{ri})$ on $\ln(Y_{ri})$, the data confirm the static (cross-sectional) Penn effect within each round, as widely reported in the literature; the regression coef-

[18]The 1993 PPPs for OECD countries were based on price surveys done in 1996, backcast to 1993. I will test robustness excluding the OECD.

[19]There was an increase in coverage of the ICP price surveys from 117 countries in 1993 to 146 in 2005. Compared to 1993, the 2005 ICP used stricter product specifications, more rigorous data validation methods, and a larger number of "ring countries" for which common prices were collected, to enable aggregation to a set of "global" PPPs. For further discussion, see World Bank (2008a).

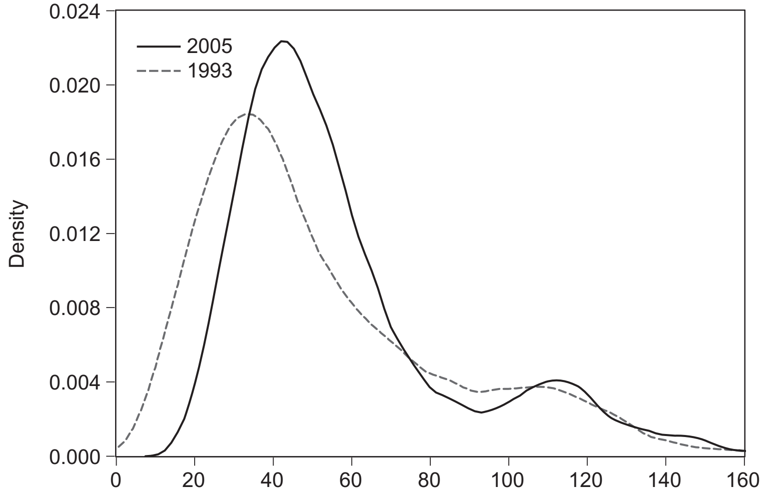

Figure 2. Kernel Densities for Price Level Indices in 1993 and 2005

Source: Author's calculations.

ficient of $\ln(P_{05i})$ on $\ln(Y_{05i})$ is 0.216, with a White standard error of 0.013 (n = 144);[20] Figure 3 plots the data from the 2005 ICP. For the 1993 round, the regression coefficient is 0.293 (SE = 0.012; n = 134), while for the 1985 round it is 0.275 (SE = 0.024; n = 56). The attenuation of the Penn effect between 1993 and 2005 is consistent with the fact that the proportionate increases tended to be larger in initially poorer countries, as can be seen in Figure 4.[21]

Turning to the changes over time, let us begin by testing the homogeneity restriction that $\gamma = 1$ in equation (2). The estimate of the unrestricted model in equation (2) is (with White standard errors in parentheses):[22]

$$(4) \qquad \ln(PPP_{05i}/PPP_{93i}) = \underset{(0.044)}{-0.029} + \underset{(0.053)}{0.290}\ln(Y_{05i}/Y_{93i}) + \underset{(0.014)}{1.011}(E_{05i}/E_{93i}) + \hat{\varepsilon}_i$$
$$R^2 = 0.987;\ n = 125$$

The restriction clearly performs well. Given that this is a regression for changes rather than levels, it is also notable that almost 99 percent of the variance is accounted for. Clearly, the fit is very good.

On imposing $\gamma = 1$, the regression coefficient of $\ln(P_{05i}/P_{93i})$on $\ln(Y_{05i}/Y_{93i})$ is $\hat{\beta} = 0.283$ (SE = 0.054; n = 132) with $\hat{\alpha} = -0.019$ (SE = 0.043) and $R^2 = 0.212$. This is close to the cross-sectional estimate of the Penn effect, suggesting that latent country characteristics are not an important source of bias in past tests for the Penn effect using cross-sectional data.

[20]Using GDP at PPP instead, the regression coefficient of $\ln(P_{05i})$ on $\ln(Y_{05i}^{PPP})$ is 0.237 (SE = 0.020), while for the 1993 ICP it is 0.343 (SE = 0.027). Note that White standard errors are used when relevant in this paper.

[21]The slope of the regression line is –0.059 with a standard error of 0.014.

[22]The regression was very similar for the non-OECD sample.

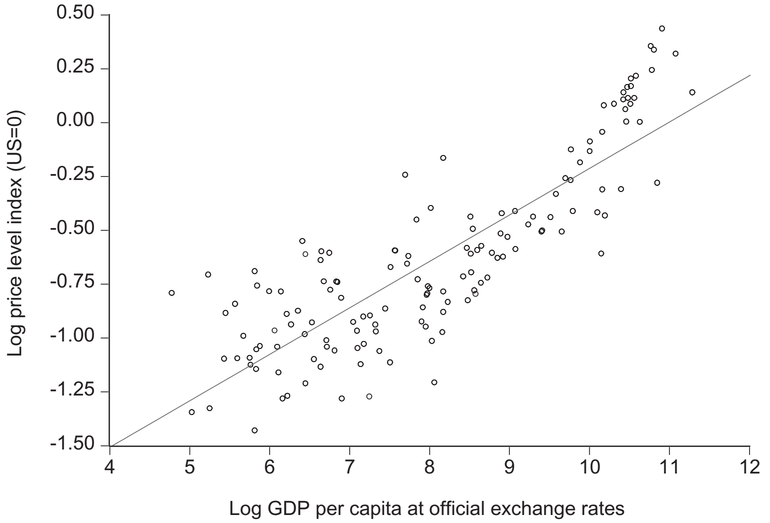

Figure 3. Static Penn Effect, 2005
Source: Author's calculations.

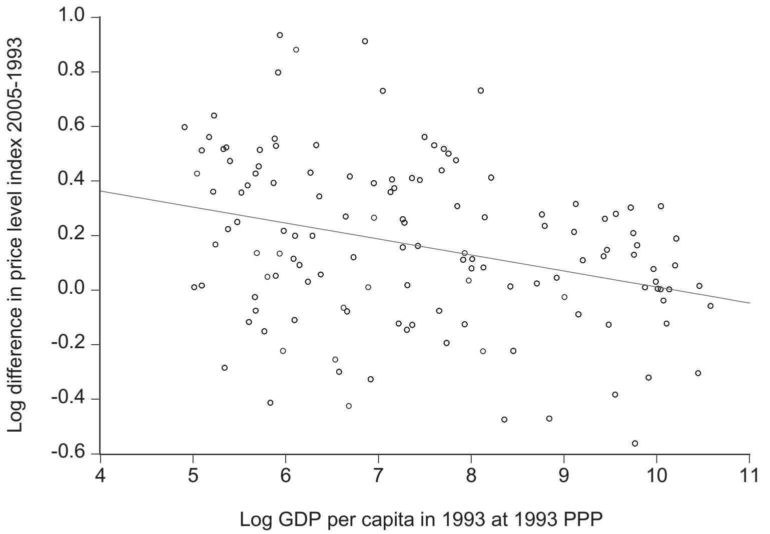

Figure 4. Larger Upward Revisions to Price Levels in Initially Poorer Countries
Source: Author's calculations.

Figure 5 plots the relationship between changes in the log price level indices and growth rates. The expected change in $\ln(P_{05i}/P_{93i})$ is zero when $\ln(Y_{05i}/Y_{93i}) = 0.066$, although the latter number is not significantly different from zero (SE = 0.140). Thus one cannot reject the null hypothesis that the price level index

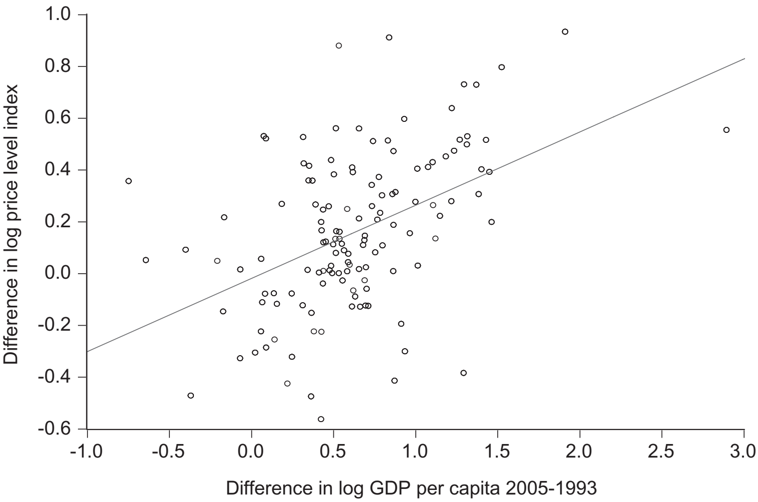

Figure 5. Dynamic Penn Effect, 1993–2005: Full Sample

Source: Author's calculations.

conditional on GDP per capita is unchanged between the 1993 and 2005 ICP rounds.

Recall that this sample includes both developed and developing countries. The data suggest that the DPE is stronger in poorer countries, as can be seen from the following regression indicating a significant negative interaction effect:

$$(5) \qquad \ln(P_{05i}/P_{93i}) = \underset{(0.046)}{0.008} + (\underset{(0.143)}{0.604} - \underset{(0.020)}{0.049}\ln Y_{93i})\ln(Y_{05i}/Y_{93i}) + \hat{\varepsilon}_i$$
$$R^2 = 0.246;\ n = 132$$

Note that this is not simply a difference between OECD and non-OECD countries. Indeed, the elasticity of the price level to economic growth is actually higher for the OECD sub-sample; the regression coefficient of $\ln(P_{05i}/P_{93i})$ on $\ln(Y_{05i}/Y_{93i})$ is 0.511 for the OECD countries (SE = 0.083; $R^2 = 0.661$; n = 24) while it is 0.272 (SE = 0.055 ($R^2 = 0.200$; n = 108) for non-OECD countries. I will return to discuss this interaction effect further.

Nor is the DPE simply picking up an "Asia-Pacific effect." The DPE is still evident if one adds a control for those countries for which the 2005 ICP was implemented by the Asian Development Bank.[23] (The regression coefficient on $\ln(Y_{05i}/Y_{93i})$ changes little and the Asia dummy is not significantly different from zero.)

There is evidence of an "ICP participation effect," whereby the relationship between the price level changes and growth rates differs between the benchmark ($D_{93i} = 1$) and non-benchmark ($D_{93i} = 0$) countries from the 1993 ICP, as is evident in the following regression:[24]

[23]Essentially this excludes OECD countries in Asia but includes the Pacific Islands.

[24]As a further check on the restriction that $\gamma = 1$, on re-estimating in the form of (3), the augmented specification corresponding to (6), gave $\hat{\gamma} = 1.003$ (SE = 0.011, $R^2 = 0.991$).

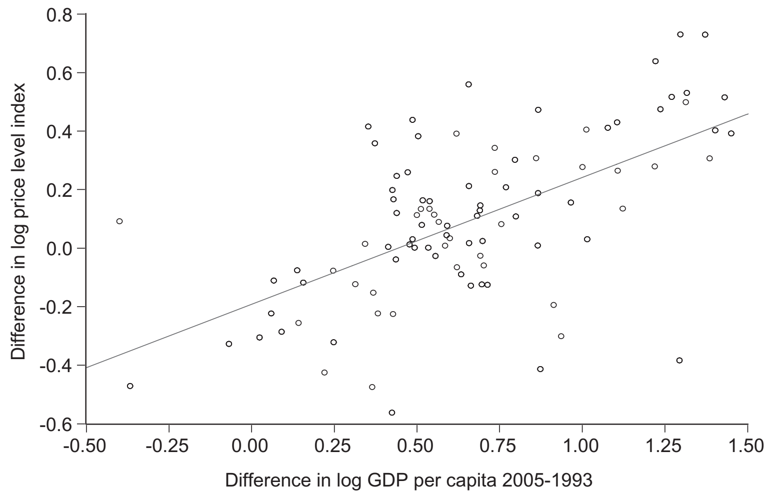

Figure 6. Dynamic Penn Effect, 1993–2005: 1993 Benchmark Countries Only

Source: Author's calculations.

(6)
$$\begin{aligned}\ln(P_{05i}/P_{93i}) &= D_{93i}[\underset{(0.046)}{-0.192} + \underset{(0.066)}{0.433}\ln(Y_{05i}/Y_{93i})] \\ &\quad + (1-D_{93i})[\underset{(0.051)}{0.247} + \underset{(0.061)}{0.193}\ln(Y_{05i}/Y_{93i})] + \hat{\varepsilon}_i \\ R^2 &= 0.428;\ n = 132\end{aligned}$$

An F-test rejects the restricted form in which the model is the same when $D_{93i} = 1$ as $D_{93i} = 0$; $F(2,128) = 23.516$; $p < 0.001$. Differentiating the DPE between the 1993 benchmark and non-benchmark countries doubles the share of the variance in $\ln(P_{05i}/P_{93i})$ that is explained.

It can be seen from (6) that the DPE is stronger for benchmark countries. Figure 6 plots the relationship for those countries on their own. We now find that the estimated expected value of $\ln(P_{05i}/P_{93i})$ is positive when $\ln(Y_{05i}/Y_{93i}) \geq 0.443$, and this switch point is significantly different from zero (SE = 0.058). Only when the growth rate (annualized log difference) exceeds 3.7 percent do we find upward pressure on the price level.

So, amongst the 1993 ICP participants one finds that the 2005 price level indices are actually *lower* at given GDP per capita than those of 1993. To put the point another way, these results suggest that it is economic growth in developing countries that explains the upward shift in price level indices (downward shift in real exchange rates) implied by the 2005 ICP, rather than statistical factors such as the stricter quality standards in the 2005 ICP's price surveys. The statistical-comparability problem appears to stem largely from the subset of 2005 ICP countries that had not participated in the 1993 ICP round.

Why is the Penn effect so much weaker for non-benchmark countries? Some explanations can be suggested. First, it might be conjectured that the ICP participation effect reflects the fact that the above model does not include all regressors used to predict the 1993 PPP for the non-benchmark countries.[25] The simplest way to check this is to see if the results change when one controls fully for the econometrically-estimated price level indices for the non-benchmark countries in 1993, by re-estimating (6) in the following form:[26]

$$\text{(7)} \qquad \begin{aligned} \ln(P_{05i}/P_{93i}) = {} & D_{93i}[-\underset{(0.046)}{0.192} + \underset{(0.066)}{0.433}\ln(Y_{05i}/Y_{93i})] \\ & + (1-D_{93i})(\underset{(0.168)}{2.766} + \underset{(0.023)}{0.154}\ln Y_{05i} - \ln P_{93i}) + \hat{\varepsilon}_i \\ & R^2 = 0.484;\, n = 132 \end{aligned}$$

The Penn effect is still weaker for the non-benchmark countries. So this is not the explanation.

Second, it might be conjectured that the ICP participation effect stems from non-linearities in the Penn effect, given that the non-benchmark countries tended to be poorer.[27] The following regression encompasses both the ICP participation effect and the negative interaction with initial GDP:

$$\text{(8)} \qquad \begin{aligned} \ln(P_{05i}/P_{93i}) = {} & D_{93i}[-\underset{(0.049)}{0.188} + (\underset{(0.167)}{0.677} - \underset{(0.024)}{0.032}\ln Y_{93i})\ln(Y_{05i}/Y_{93i})] \\ & (1-D_{93i})[\underset{(0.053)}{0.266} + (\underset{(0.253)}{0.549} - \underset{(0.041)}{0.063}\ln Y_{93i})\ln(Y_{05i}/Y_{93i})] + \hat{\varepsilon}_i \\ & R^2 = 0.444;\, n = 132 \end{aligned}$$

The negative interaction effect is still evident, though less precisely estimated (which is not too surprising given the tendency for ICP non-participants to be poorer). A significant difference between the benchmark and non-benchmark models is still evident.[28]

It appears more likely that the answer lies in the nature of the (unobserved) selection process for participation in the 1993. It appears that all countries were invited to participate in the 1993 ICP. However, the latent selection process determining why some countries chose not to participate could well have been based on variables that are correlated with the strength of DPE. When interpreted in terms of the Balassa–Samuelson model, it appears that non-participants had characteristics that made their productivity differences for tradables less responsive to the differences in their GDP per capita. It can be conjectured that many non-participants lacked the public-institutional capacity for implementing the ICP's surveys. Furthermore, it can be expected that weak statistical capacity is probably correlated with weak institutions more generally, including weak states. If that institutional capacity is cooperant with labor in the production of traded goods—such that the marginal product of that labor is lower when institutions are weaker—then we can see that non-participation in the ICP could signify a weaker relationship between GDP and the relative productivity of labor in the traded-goods sector, and (hence) a weaker Penn effect in the data.

[25]The missing variable is the secondary school enrollment rate.

[26]Note that when $D_{93i} = 0$, P_{93i} is predicted based on the observed covariates used by the ICP.

[27]The non-benchmark countries in the 1993 ICP had an (unweighted) mean GDP per capita of \$2,268, as compared to \$6,923 for benchmark countries.

[28]The difference also persisted when I allowed for non-linearity in the underlying cross-sectional double log model for the Penn effect, by adding $\ln(Y_{05i})^2 - \ln(Y_{93i})^2$ to the differenced model in (1).

The ICP participation effect helps explain why the increases in the price level index tended to be higher in countries with lower GDP per capita in 1993, as seen in Figure 4. Simply adding a control for benchmark countries brings the slope of the regression line in Figure 3 down from –0.059 (SE = 0.014) to –0.037 (SE = 0.014). Adding the 1993 GDP to equation (6) gives:[29]

$$\begin{aligned}(9)\quad \ln(P_{05i}/P_{93i}) = {} & D_{93i}[\underset{(0.132)}{0.011} + \underset{(0.064)}{0.428}\ln(Y_{05i}/Y_{93i}) - \underset{(0.014)}{0.026}\ln(Y_{93i})] \\ & + (1-D_{93i})[\underset{(0.165)}{0.676} + \underset{(0.063)}{0.193}\ln(Y_{05i}/Y_{93i}) - \underset{(0.025)}{0.066}\ln(Y_{93i})] + \hat{\varepsilon}_i \\ & R^2 = 0.461;\ n = 132\end{aligned}$$

We see that the coefficient on $\ln(Y_{93i})$ is substantially higher (in absolute value) amongst the countries that did not participate in the ICP in 1993.

It is also of interest to see how well one can explain the changes between the 1985 and 2005 PPPs. Here there are further comparability problems, stemming from the differences between the World Bank's methods and those of PWT. Even so, the corresponding estimate of equation (3) has similar explanatory power (in obvious notation):[30]

$$\begin{aligned}(10)\quad & \ln(PPP_{05i}/PPP_{85i}) = -\underset{(0.111)}{0.168} + \underset{(0.079)}{0.417}\ln(Y_{05i}/Y_{85i}) + \underset{(0.025)}{0.991}\ln(E_{05i}/E_{85i}) + \hat{\varepsilon}_i \\ & R^2 = 0.981;\ n = 54\end{aligned}$$

Using instead the period 1985–93, one obtains:

$$\begin{aligned}(11)\quad & \ln(PPP_{93i}/PPP_{85i}) = -\underset{(0.092)}{0.034} + \underset{(0.099)}{0.475}\ln(Y_{93i}/Y_{85i}) + \underset{(0.047)}{0.948}\ln(E_{93i}/E_{85i}) + \hat{\varepsilon}_i \\ & R^2 = 0.948;\ n = 55\end{aligned}$$

Imposing the restriction that $\gamma = 1$ the regression coefficient of $\ln(P_{05i}/P_{85i})$ on $\ln(Y_{05i}/Y_{85i})$ is $\hat{\beta} = 0.431$ (SE = 0.075; n = 55; $R^2 = 0.488$) and $\hat{\alpha} = -0.194$ (SE = 0.104). So it appears that the DPE is even stronger over 1985–2005, despite the differences between PWT and the World Bank's methods. Figure 7 plots the data for this longer period (with fewer observations, and confined to 1985 benchmark countries). The switch point (at which $\ln(P_{05i}/P_{85i}) = 0$) in expectation) is at $\ln(Y_{05i}/Y_{85i}) = 0.451$ (SE = 0.168), corresponding to an annual growth rate of 2.3 percent.

5. China's Controversial PPP Revisited

The new PPP for China from the 2005 ICP attracted much attention, given that it implies that the country's GDP per capita at PPP for 2005 was 40 percent lower than we thought prior to release of the 2005 ICP results, at $4,091 rather

[29]The weighted mean coefficient on GDP in 1993 is –0.039 (SE = 0.002), as compared to a regression coefficient without controls of –0.059 (SE = 0.014). Also note that estimating in the form of equation (2), the coefficient on the log difference in the exchange rate is again not significantly different from unity; $\hat{\gamma} = 0.995$ (SE = 0.010; R^2 rises to 0.991).

[30]The negative interaction effect with the initial (log) GDP was also evident using the 1985 PPPs, but for brevity this discussion is confined to the simpler version of the DPE.

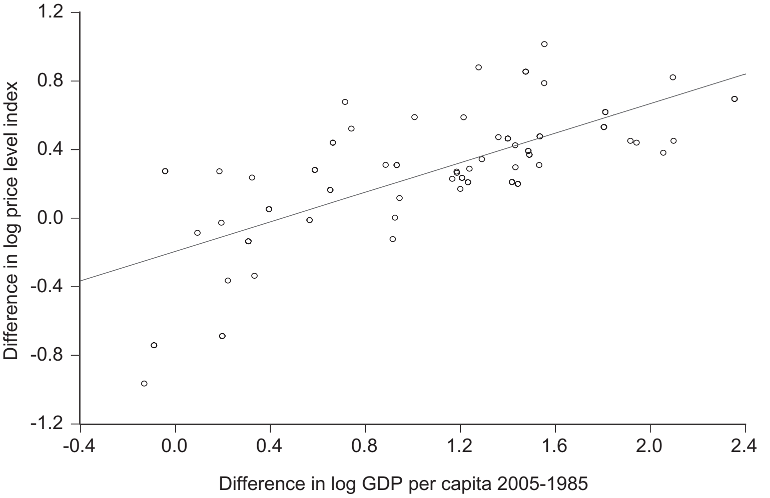

Figure 7. Dynamic Penn Effect, 1985–2005
Source: Author's calculations.

than the prior estimate for 2005 of $6,760 (World Bank, 2008b). Just before the release of the 2005 ICP's results, China's price level index for 2005 was deemed to be 25 percent, up from 19 percent in 1993. The price surveys from the 2005 ICP implied a price index of 42 percent.

Some observers questioned whether China's new PPP was credible. Bhalla (2008) argued that, when combined with the official growth rates, the new PPP implied that China was too poor to be believed in (say) 1950; in Bhalla's words, the World Bank's numbers imply that "most Asians (were) dead in 1950." Maddison and Wu (2008) and Deaton and Heston (2010) raise similar objections, leading Maddison and Wu to claim that the new PPP for China is "weird" and "implausible."

It is far from clear whether these extrapolations back in time constitute a sound basis for validating the new PPP for China. Ravallion (2010) points out that the new PPP only implies that China was as poor (in terms of GDP per capita) in 1950 as the Democratic Republic of the Congo is today, and that about 400 million people in the world (40 million of them in China) currently live below that income level; they survive, albeit at very meager levels of consumption. Nonetheless, given the size of this data revision, the subsequent controversy, and the importance of the global importance of the Chinese economy, it is of interest to look more closely at the data for China.

How much of the observed change in China's price level index implied by the 2005 ICP is accountable to the DPE? If the model fully accounted for the (large) change in China's price level index (given the country's high rate of growth) then we would find that China is close to the regression line. This is clearly not the case. Adding a dummy variable for China to equation (6) or (8), the coefficient is 0.278 and this is significantly different from zero (SE = 0.077); it is 0.239 (SE = 0.083) when added to (7).

However, while economic growth does not fully account for the change in China's level, it accounts for a sizable share. The observed change in the log of China's price level index is 0.795, so the model can account for 65–70 percent of the change in China's price level index between 1993 and 2005.

This assessment is affected little by allowing for overestimation of China's growth rate. Maddison (2007) has claimed that China's long-run growth rate is overestimated by possibly two percentage points per year (though also see Holz's (2006) comments on Maddison's assumptions). Even cutting two percentage points off China's annual growth rate and re-estimating the regressions,[31] the China coefficient is still 0.320 (SE = 0.066) in equation (6) and 0.282 (SE = 0.071) in equation (7).

The rest of the change in China's price level could well stem from the sampling bias in the 2005 ICP's price surveys for China. The 2005 round of the ICP was the first time that China had officially participated in the ICP; priors had been based on an estimate of the country's PPP for 1993 that was not based on a 1993 price survey, but rather was an updated version of an older PPP for China from non-ICP price data.[32] On looking more closely at how China's price surveys were done for the 2005 ICP, Chen and Ravallion (2010b) point to sampling biases that would lead to an overestimation of the level of prices. However, their proposed correction still implies a large increase in China's price level between 1993 and 2005.[33] The correction for that bias proposed by Chen and Ravallion (2010b), using non-ICP data on rural prices, brings China's (expenditure-weighted) price index for consumption (rather than GDP as a whole) down from 52 percent to about 45 percent, though still considerably higher than the prior estimates of around 25 percent based on the 1993 ICP (19 percent for GDP). Assuming a similar correction for the GDP price index, the combined effect with the DPE (given China's high growth rate), leaves the doubling of China's price index almost fully explained.

6. Implications for Estimating PPPs for Non-Benchmark Years

As noted in the introduction, the most widely-used method for extrapolating and updating PPPs relies solely on the inflation rate in the country in question, relative to the U.S. I find that the inflation rate is a strong predictor of the proportionate changes in PPPs; the regression coefficient of $\ln(PPP_{05i}/PPP_{93i})$ on $\ln(DEF_{05i}/DEF_{93i})$ is 0.981 (SE = 0.044), with $R^2 = 0.958$. (Note again that the U.S. inflation rate is a constant and so drops out.) However, in a nested test, the inflation-adjustment method is clearly outperformed by a model incorporating the DPE. This is evident if one adds a term in $\ln(DEF_{05i}/DEF_{93i})$ to equation (4); its coefficient is 0.094, with a standard error of 0.102, while other coefficients and their

[31]Given that the 2 percent is annual, the term in $\ln(Y_{05i}/Y_{93i})$ for non-benchmark countries was replaced by $\ln(Y_{05i}/Y_{93i}) - 0.24China$; *China* is a dummy variable for China, which also appears as a separate regressor for the purpose of this test.

[32]More precisely, the previous PPPs were derived using a bilateral comparison of 1986 prices between the United States and China as documented in Ruoen and Chen (1995).

[33]The ICP aims to collect prices from a representative sample of outlets in each country. However, this was not possible in China and the ICP only covered 11 cities.

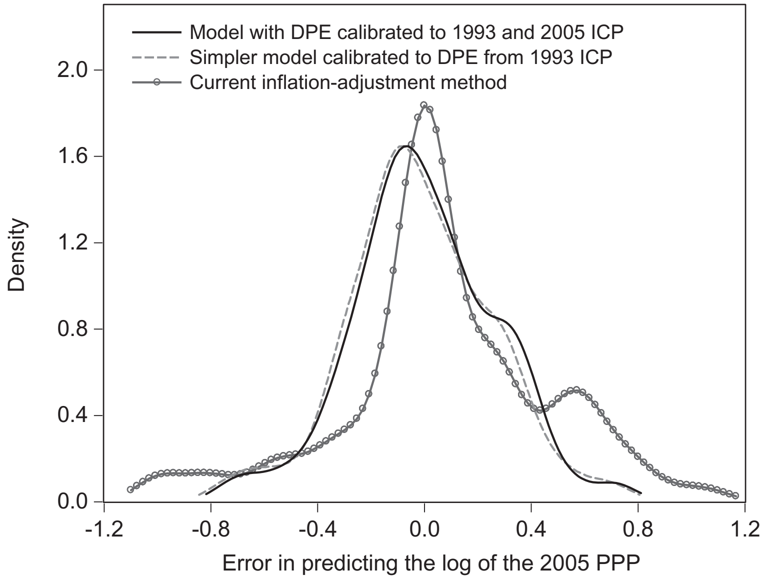

Figure 8. Kernel Densities for Errors in Predicting the 2005 PPP With and Without the Dynamic Penn Effect (DPE)

Source: Author's calculations.

standard errors change little. So one cannot reject the null hypothesis that the inflation rate has no effect, once one controls for the DPE and the change in market exchange rates.

Another way to assess these methods is to compare their errors in predicting the 2005 PPP. Figure 8 plots the empirical (normal kernel) density functions of those errors (log of PPP minus its predicted value). One density in Figure 8 is for the errors obtained using the inflation-adjustment method (based on the GDP deflator) while another is for the residuals from a regression of ln PPP_{05i} on ln PPP_{93i}, ln(Y_{05i}/Y_{93i}) and ln(E_{05i}/E_{93i}) (which naturally have zero mean); these are very close to the errors in predicting ln PPP_{05i} by simply adding ln PPP_{93i} to (4).

It can be seen that the density function of the errors implied by the inflation-adjustment method has thicker tails (large errors in both directions, but more so in the upper tail) and is not centered on zero, with underestimation at the mean; the mean error is 0.071, or roughly a 7 percent underestimation of the PPP (or 7 percent overestimation of GDP at PPP).

It can be argued that this comparison is biased against the inflation-adjustment method, since the proposed alternative is calibrated using data that include the 2005 PPPs from the ICP, which would not (of course) be available when updating the 1993 PPPs prior to release of the 2005 ICP. However, virtually identical results are obtained if one simply assumes that the PPP has an elasticity of unity to the nominal exchange rate and that the DPE coefficient is 0.293—based on the cross-sectional value estimated above using the 1993 PPPs. I also give the density function for this simple and feasible estimator in Figure 8. This does not yield zero-mean error, but the mean error is much smaller than the inflation-adjustment method (–0.017 versus 0.071), and it also trims the large errors in the tails generated by the latter method.

7. Conclusions

The substantial downward revisions to the estimates of the real GDP of developing countries implied by the 2005 ICP surprised many observers. Some have questioned the data. Concerns about the comparability of ICP data across survey rounds have loomed large, and have reinforced past practices of not mixing PPPs across ICP rounds.

This paper has tried to make economic sense of the PPP changes between ICP rounds. While not denying the comparability problems between ICP rounds, the paper reports new evidence consistent with the existence of a Balassa–Samuelson effect over time, such that the PPP rises relative to the nominal exchange rate in a growing economy. There are signs that this only starts to happen with a sufficiently high growth rate. The paper finds that this "dynamic Penn effect" is even stronger in initially poorer countries. Thus the widely-observed static Penn effect (whereby the price level index is lower in poorer countries) has been attenuated over time.

Thus the higher price level indices (lower real exchange rates) for many developing countries implied by the 2005 ICP are accountable in part to their economic growth. On its own, the dynamic Penn effect only accounts for about one fifth of the variance in the proportionate changes in the price levels over 1993–2005, though rising to one half when one allows for extra measurement errors in the PPPs not based on price surveys. It can be conjectured that ICP comparability problems and other measurement errors account for the remainder.

It turns out that China's new PPP is not as "weird" or "implausible" as some observers have claimed. Given China's high growth rate, it is not too surprising that the country's price level index rose appreciably between 1993 and 2005 (or, equivalently, that its real exchange rate declined). This paper's calculations suggest that about two thirds of that increase is accountable to the dynamic Penn effect. The bulk of the remainder may well reflect an upward bias in China's PPP due to the 2005 ICP's weak coverage of China's rural areas.

What light do these findings throw on the substantial data revisions implied by the 2005 ICP, such as illustrated by Figure 1 for real GDP? The current methods used by the *World Development Indicators* to update PPPs between ICP rounds do not allow directly for the dynamic Penn effect. Yet the extrapolations used to fill in missing PPPs in a given benchmark year are explicitly based on the static Penn effect. The results of this paper point to a better method in which the Penn effect would also be brought explicitly into the inter-temporal extrapolations for the price-level index, using market exchange rates to back out the implied PPPs for non-benchmark years. This method has been shown here to yield more reliable estimates than the widely-used inflation-adjustment method for updating PPPs between ICP rounds.

The upshot of all this is that many of the large revisions to real GDP data in Figure 1 could have been avoided by exploiting some simple but neglected insights from the original Balassa–Samuelson model, which motivated the considerable international statistical effort since the 1960s to collect price data for measuring PPPs in cross-country comparisons. That model can also help us better understand how price levels evolve over time in developing countries, and so avoid unnecessary data revisions at the release of the results of each new ICP round.

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