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*Searching for Balassa Samuelson in Post-War Data**

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Summary

The Balassa Samuelson effect is a central result in trade theory. It is also fundamental to our understanding of what occurs during economic growth. As it turns out, the positive relationship between real income and the price level predicted by Balassa Samuelson occurs only after 1970. Why does Balassa Samuelson hold for recent years but not earlier? We provide an empirical explanation for this puzzle. Our point of departure is the observation that measurement error in comparative GDP data biases standard tests against finding a Balassa Samuelson effect. Allowing for measurement error, we find that Balassa Samuelson is present in the data for all post-war decades.

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Draft One

*We thank Alan Taylor for helpful comments.

Introduction

Recent years have seen fresh interest in the celebrated Balassa Samuelson effect of Balassa (1964) and Samuelson (1964) where growth is accompanied by faster rates of technological progress in traded sectors and a rise in the price level.¹ This work includes Bergin, Glick and Taylor (2006), Fitzgerald (2003), Ghironi and Melitz (2005), amongst others. The new literature marks a major advance on previous work as it supplies the missing micro foundations for Balassa Samuelson. In particular, it uses trade costs, heterogeneous firms and imperfect competition to explain why technological progress is faster for traded sectors. Research in this area received a second boost from the empirical findings of Bergin, Glick and Taylor (2006) henceforth Bergin *et al.* Bergin *et al.* (2006) show that Balassa Samuelson exists for recent decades only. They term their findings the long run price puzzle. Since economists have long assumed that Balassa Samuelson is a constant of the growth process, their results have far reaching implications.²

Why is Balassa Samuelson a recent phenomenon? This is an important question given the central role of Balassa Samuelson for trade and growth theory. We provide an empirical resolution for the puzzle. Our point of departure is the argument that measurement error in comparative GDP data has biased previous empirical tests against Balassa Samuelson. After adjusting for measurement error, we find that Balassa Samuelson is present for all post-war decades.

¹ Although formalized by Balassa (1964) and Samuelson (1964) the ideas behind Balassa Samuelson go back to Ricardo and earlier see Officer (1982). Rogoff (1996) surveys traditional approaches to Balassa Samuelson while Taylor and Taylor (2004) look at recent developments.

² This is particularly the case for key applied questions such as Purchasing Power Parity and international income comparisons.

We proceed as follows. Section two outlines why comparative GDP data sets contain measurement error. We demonstrate that such errors can explain why the data provide so little support for Balassa Samuelson before 1970's and why Balassa Samuelson strengthens over time. In addition, we show that we can get around measurement error by testing Balassa Samuelson with data on *nominal* GDP. Building on these results, section three provides new evidence on Balassa Samuelson. After adjusting for measurement error in our estimation procedures, we find the Balassa Samuelson effect is present in cross-sections of the Penn Tables for all post-war decades. In common with Bergin *et al* (2006), however, we find that Balassa Samuelson has strengthened over recent decades.

Section four provides a panel test of Balassa Samuelson. For the most part, the findings confirm the earlier results. Section five provides two final extensions. First, we provide a Monte Carlo simulation that adjusts for the potential endogeneity of nominal income. Second, we crosscheck our findings by looking the benchmark income comparisons of the International Comparison Project (ICP). The results from both exercises are broadly supportive of Balassa Samuelson. Section six sums up. We argue that Balassa Samuelson is a robust feature of the post-war data. In addition, we suggest that the recent strengthening of Balassa Samuelson is the inevitable consequence of globalization.

2. *Testing Balassa-Samuelson when GDP data contain measurement error*

Standard tests of the Balassa Samuelson effect relate the price level to income and a set of other explanatory variables. Consider equation (1) where p_{it} and y_{it} are logs of the relative price level and real income per capita of country i measured in terms of the US at time t ; \mathbf{Z} is a vector of other explanatory variables, $\boldsymbol{\alpha}$ is a vector of coefficients, and ε_{it} is a disturbance term. By relative price levels, we mean price indices that compare price levels across economies at a point in time. The Balassa Samuelson effect requires that β is positive meaning that the price level increases with income.

$$(1) \quad p_{it} = \mathbf{Z}_{it} \cdot \boldsymbol{\alpha}_t + \beta_t y_{it} + \varepsilon_{it}$$

The usual source for cross-country GDP and price data are the Penn World Tables.³ Using Penn data, Bergin *et al* (2006) show that Balassa Samuelson first appears in the data during the early 1960's and then strengthens over time. For our purposes, we see their results as posing two related puzzles. First, why is there such a weak relationship between real income and the price level before the 1970's? Second, why does Balassa Samuelson continue to strengthen? We argue in this section that both of these puzzles are explainable by GDP measurement error.

To understand the sources of GDP measurement error, we must outline how Penn constructs its GDP estimates.⁴ Penn starts with a benchmark income comparison. This is 1996 for version 6.1 of the Penn Tables used in this paper. Penn obtain their 1996 GDP

³ A second strand of the literature directly tests the claim of differential productivity growth see Canzoneri, Cumby and Diba (1999) or Choudri and Khan (2004).

⁴ Neary (2004) provides theoretical foundations for the Penn procedures.

benchmark for country i by deflating nominal income by the relative price level as calculated with detailed price and expenditure data from the International Comparison Project (ICP) of the United Nations. Penn calculates domestic price levels relative to “world prices” where world prices are weighted national prices using weights based on shares in world expenditure (see Kravis (1984) for more details). The GDP benchmark for country i is thus in 1996 world prices. To generate GDP series for other years, Penn projects its GDP benchmarks backwards and forwards using data from domestic national accounts. Income per capita for country i relative to the US is the ratio of the projected series for country i to projected US income. This gives relative income per capita in 1996 world prices.

The Penn GDP data contain measurement error. By measurement error, we mean that the Penn estimate of relative GDP per capita differs from “true” relative GDP in 1996 world prices. Measurement error comes from three sources. First, there is sampling error in the ICP price surveys. These errors are likely to be small. The second source of error is the extrapolation procedure used to project the Penn benchmark comparisons outside the 1996 benchmark. Because of data constraints, Penn projects GDP using data on consumption, government spending, investment and the external balance.⁵ As noted by Kravis (1984), much greater levels of dis-aggregation are necessary to project GDP. Finally, and most importantly, the quality of GDP data is low for developing economies. The Penn Tables ranks the quality of its estimates from A to D. Most developing economies receive a C or D. We suspect that these data are especially poor for the 1950’s and 1960’s.⁶

⁵ An important difference between the Penn Tables and the national accounts is that Penn calculates growth rates in world prices while the national accounts measures growth rates in domestic prices. As a result, the growth rates of the Penn GDP series are not the same as GDP growth rates from the domestic national accounts see Nuxoll (1994).

⁶ The Penn Tables uses official exchange rates to convert price levels into dollars. This is a further source of measurement error. From the 1950’s through the 1980’s, exchange controls and multiple exchange rates

What are the consequences of measurement error for tests of Balassa Samuelson? As we show, measurement error leads to biased and inconsistent least squares estimates of β . In particular, when we estimate (1) with cross-sectional data, the resulting least squares estimates of β tend to increase over time.

To illustrate these effects we focus on a simple version of (1) that ignores variables other than real income. Suppose that the following relationship holds between the price level and real income per capita measured relative to the US:

$$(2) \quad p_i^* = \alpha + \beta y_i^* + \varepsilon_i$$

where p^* is the “true” price level without measurement error and $y_i^* = Y_i - p_i^*$. Suppose that the measured price level contains error such that (3) holds where v_i is measurement error with mean zero and normally distributed.

$$(3) \quad p_i = p_i^* + v_i$$

Using the definition of real income, we can write:

$$(4) \quad y_i = Y_i - p_i = Y_i - (p_i^* + v_i) = y_i^* - v_i$$

were endemic in the developing world see Reinhart and Rogoff (2004). For many developing countries, official rates often bore little resemblance to the exchange rate applied to most transactions. Official exchange rates will thus distort the Penn price level measures. In contrast, the Penn GDP volume measures are in constant 1996 world prices. They are not influenced by exchange rates.

Throughout, we assume that nominal income is measured without error. Thus, by construction, any error in p will produce an equal and offsetting error in y . Using (2), (3) and (4), we obtain the following:

$$(5) \quad p_i - v_i = \alpha + \beta(y_i + v_i) + \varepsilon_i$$

Rearranging, we get:

$$(5a) \quad p_i = \alpha + \beta y_i + w_i \quad \text{where } w_i = \varepsilon_i + (1 + \beta)v_i.$$

From (5a), the independent variable, y_i , is correlated with the error term, w_i . Consequently, the standard assumptions break down and the least squares estimation of (5a) produces biased and inconsistent estimates of β .

What is the nature of the bias? We provide the probability limit for the OLS estimator of β with measurement error in (6) where σ_v^2 and $\sigma_{y^*}^2$ are variances of the measurement error, v_i , and real income, y^*_i , respectively. (See Appendix 1 for a derivation)

$$(6) \quad \text{plim } \hat{\beta} = \frac{\beta \cdot \sigma_{y^*}^2 - \sigma_v^2}{\sigma_{y^*}^2 + \sigma_v^2} = \frac{\beta - \frac{\sigma_v^2}{\sigma_{y^*}^2}}{1 + \frac{\sigma_v^2}{\sigma_{y^*}^2}}$$

If β is positive, measurement error will bias the OLS estimate $\hat{\beta}$ downwards. As the variance of measurement error σ_v^2 increases, the estimated β can be negative. As noted

earlier, measurement error for the Penn Tables is likely greatest for the 1950's and 1960's, suggesting that the estimated β will tend to have smaller values for these years. Since measurement error decreases as we move closer to the benchmark year, 1996, the estimated β will increase over time.⁷ Thus, measurement error can explain both aspects of the long run price puzzle. It explains why Balassa Samuelson is weak for the early post-war decades and it explains why it gets stronger over time.

How do we test Balassa Samuelson when there is GDP measurement error? One option, suggested by the working paper version of Bergin *et al* (2006), is to estimate (1) with benchmark data. By benchmark data, we mean GDP comparisons based on detailed price and expenditure data. There are no projection errors for benchmark data by construction and overall measurement error is small. As discussed in Section Five, the drawback of benchmark data is that they are available for isolated years with no coverage of developing economies before recent decades.⁸

Alternatively, we could pursue econometric solutions to measurement error. The problem with this approach is that it requires information on the variance of measurement error.⁹

⁷ The bias due to official exchange rates is different. Assume that the average exchange rate applied to international transactions is \underline{e} . Assume further that this rate differs from the official market rate e . In this case, equation (1) becomes (1*).

$$(1^*) \quad p = \alpha + \beta y + (e - \underline{e}) + \varepsilon$$

It follows from (1*) that official exchange rates introduce an omitted variable bias. In practice, the differences between e and \underline{e} are greater for poorer economies. It is straightforward to show that this will also bias estimates of β from (1) downwards.

⁸ There are no ICP benchmarks for developing economies before 1967.

⁹ In his study of convergence, De Long (1988) adjusts for GDP measurement error with a maximum likelihood procedure by making assumptions about the relative variance of measurement error; see also Temple (1998). This is especially difficult in our case as the errors change over time.

The third option tests Balassa Samuelson with *nominal* income per capita as in equation (7) where Y is relative nominal income in terms of the US. Equation (7) is a transformation of (1). It has a long history dating to the classic papers of Balassa (1964, 1973). Of late, it has attracted less attention.¹⁰ As we shall see, (7) has advantages over (1) when GDP data suffer from measurement error.

$$(7) \quad p_{it} = \mathbf{Z}_{it} \cdot \mathbf{a}_t + b_t Y_{it} + e_{it}$$

where

$$\mathbf{a}_t = \boldsymbol{\alpha}_t \cdot \left(\frac{1}{1+\beta} \right), \quad b = \frac{\beta}{1+\beta}, \quad e_{it} = \frac{\varepsilon_{it}}{1+\beta}$$

By definition, $Y = p+y$. Since measurement errors in p and y have the same magnitude while differing in sign, the errors cancel for nominal income.¹¹ If equation (7) satisfies the other classical assumptions, the OLS estimator for b is consistent and yields unbiased estimates of the Balassa Samuelson effect.

While (7) eases measurement error it does so at the cost of introducing a second bias working in the opposite direction. The bias arises as p , the dependent variable, is also used to construct nominal income on the right-hand-side of (7). The endogeneity of nominal income leads to a classic simultaneous equations bias. In our case, this will bias the b coefficient upwards leading to an overstatement of the Balassa Samuelson effect. Sections four and five

¹⁰ An exception is Prados de la Escosura (2000) who estimates (7) with ICP benchmark data. His work covers mainly developed economies.

¹¹ Official exchange rates create fewer problems for (7) because the relative price level and nominal income are calculated using the same (official) exchange rate.

propose various techniques to adjust for the endogeneity of nominal income. First, however, we take a closer look of the relationship between income and the price level in Penn data.

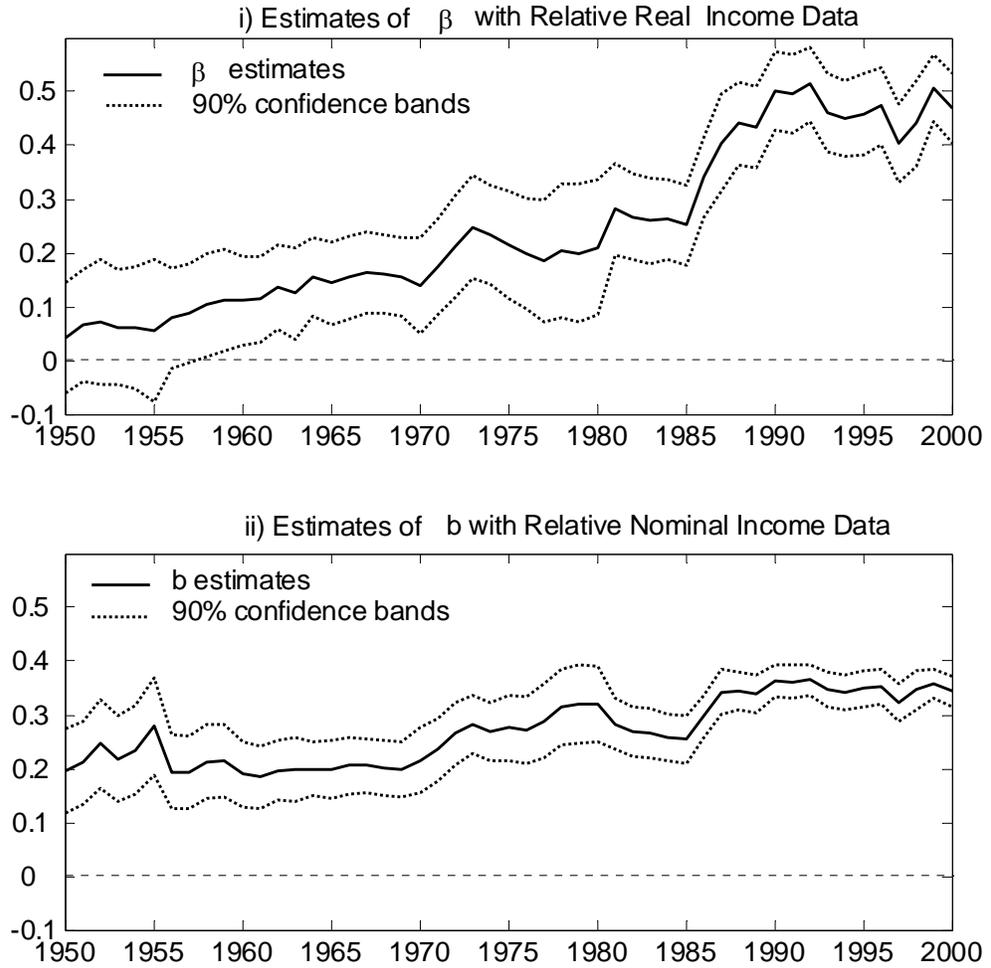
3. *The Evidence from Cross Sections of the Penn Tables*

This section applies (1) and (7) to Penn Tables data. We focus on the cross sectional relationship between income and the price level using a balanced panel of fifty-eight economies with data from 1950 to 2000.¹²

The first panel of Figure One presents the least squares estimates of β from (1) by year for each cross section. We also provide confidence bands at the ninety percent level. The results are identical to Bergin *et al* (2006). They show no Balassa Samuelson effect for the 1950's. This is the first part of the long run price puzzle. After the early 1960's, we see the expected positive relationship between the price level and real income. As pointed out by Bergin *et al* (2006) Balassa Samuelson strengthens almost monotonically. By 2000, the elasticity of the price level with respect to real income, β , is 0.5. This compares to an elasticity of 0.15 for 1970 and 0.05 for 1950. The rise in Balassa Samuelson is the second part of the long run price puzzle.

¹² To ensure comparability with Bergin *et al* (2006) we take our data from Penn Tables version 6.1. This version has no 1950 data for Chile, the Dominican Republic, Ecuador, Greece, Paraguay, Sweden and Taiwan. We assume that the 1950 values for relative income and relative prices equal their 1951 values. This explains why we have a slightly larger sample than Bergin *et al* (2006). After 1997, we have no data for Taiwan, Guyana and the Congo Democratic Republic.

Figure 1: Simple Regression Results from Penn Tables Data



Panel (ii) provides the least squares estimates of (7) where we regress price on nominal income ignoring, for the moment, potential endogeneity. With nominal income, the Balassa Samuelson effect is present for all decades. Thus, measurement error can explain the first part of the long run price puzzle. The second aspect of the puzzle remains, however, as Balassa Samuelson strengthens over time. The elasticity of the price level with respect to nominal income, b , averages 0.25 for the 1950's and the 1960's. Since $\beta = b/(1-b)$, the b coefficient of 0.25 yields an implied elasticity of the price level with respect to real income,

that is β , of around one third.¹³ By the late 1990's, b has increased to 0.35 giving an implied β of 0.54 showing that Balassa Samuelson has greatly strengthened since the 1950's.

What lies behind these results? Here it is worth pointing out a fundamental feature of the Penn data. As it turns out, price level differences between rich and poor economies widened after the 1980's. One way to see this is to look at the average price level, measured relative to the US, for the ten poorest economies. This is around sixty per cent of the US for the 1960's. By the 1990's, it is down to thirty percent with most of the decline occurring after 1985.¹⁴ Since there is no corresponding decline in relative income, simple regressions of prices on either real or nominal income as in Figure One will attribute the fall in relative price levels for poor economies to a strengthening of Balassa Samuelson.

To sum up, GDP measurement error can explain the first part of the long run price puzzle. This conclusion is, however, tentative. First, our approach leaves out key variables such as commercial policy and changes in the external terms of trade that influence price levels. Second, the results are biased in favor of Balassa Samuelson since nominal income is endogenous. The next section addresses both concerns with a panel model.

¹³ Formally, the recovered β from (7) is not the same as the β estimated in (1). This is true even without measurement error as the recovered β from (7) minimizes the sum of squares of the v 's while the β estimate from (1) minimizes the sum of square of the ϵ 's. We ignore this distinction in what follows.

¹⁴ A second way to see this is to note that equations (1) and (7) are stable for developed economies over the post-war period. Both yield an estimate of β close to 0.5 for the entire postwar period. The secular increase in β is thus due to developing economies. Prados de la Escosura (2000) also finds that (7) is stable over time for his sample developed economies.

4. *Panel Results*

This section provides a panel model to incorporate a time series dimension in our tests of Balassa Samuelson. The panel estimates also provide solutions to the endogeneity of nominal income and omitted variables. As it turns out, our previous results continue to hold.

We focus on (7) as our preferred specification. As noted earlier, the simple version of (7) from the previous section ignores other variables most notably commercial policy and changes in the external terms of trade that influence the price level.¹⁵ Our findings could therefore be due to omitted variable bias. Unfortunately, there are no satisfactory data on these variables. How do we account for omitted variables? Dynamic panel models provide one solution. Such models, however, are inappropriate for our case since they downplay cross-sectional variation in prices and income. Such variation is essential when testing Balassa Samuelson.¹⁶ A further drawback of dynamic panel model estimators is that they can exacerbate measurement error bias (see Hauk and Wacziarg (2004)).¹⁷

Our solution is to add regional dummies for Asia, Latin America and Africa.¹⁸ The regional dummies are crude but they allow us to account for shocks that are specific to these regions, particularly commercial policy and changes in the external terms of trade by sacrificing some cross-country variation in income and prices. If anything, the dummies will bias the results against finding Balassa Samuelson.

¹⁵ Edwards (1989) surveys the large literature in this area. Prados de la Escosura (2000) provides a more recent discussion.

¹⁶ One way to see the importance of cross sectional variation in the levels of prices and income is to note that there is no relationship in Penn data between relative income and the price level in differences.

¹⁷ Barro (1997, 2000) strikes a cautionary note about fixed effects and dynamic panel techniques generally. His observations are borne out by the Monte Carlo study of Hauk and Wacziarg (2004).

¹⁸ Durlauf, Johnson and Temple (2004) endorse the use of regional dummies in similar circumstances to ours.

The pooled version of (7) is (8).

$$(8) \quad \mathbf{p}_t = \mathbf{X}_t \mathbf{d} + \mathbf{v}_t \quad \text{for } t=1, \dots, T$$

where \mathbf{p}_t is a $N \times 1$ vector of prices, \mathbf{X}_t is a $N \times k$ matrix of explanatory variables, \mathbf{v}_t is the error terms at time t , and \mathbf{d} is the $k \times 1$ vector of parameters. We pool using an OLS panel estimator with a Newey-West autocorrelation and heteroscedasticity consistent covariance estimator. To extend the Newey-West procedure to the panel case, we assume that:

$$(9) \quad E(v_{it} v_{is}) = \begin{cases} \sigma_{ii, |t-s|} = c_{|t-s|} \neq 0 & \text{for } |t-s| \leq L \\ 0 & \text{otherwise} \end{cases}, \text{ for all } i\text{'s}$$

where L is some positive integer. The formulation assumes that there is no heteroscedasticity across countries but that the error term for each country is autocorrelated. Given these assumptions, the asymptotic covariance estimates of the OLS estimator are:

$$(10) \quad \text{Asy. Var}(\mathbf{d}) = (\mathbf{X}'\mathbf{X})^{-1} \Pi (\mathbf{X}'\mathbf{X})^{-1}$$

where:

$$\begin{aligned} \Pi = & (\mathbf{X}' \cdot \text{kron}(I_T, \Psi^0) \cdot \mathbf{X}) + 2w_1 (\mathbf{X}^{(2,T)'} \cdot \text{kron}(I_{T-1}, \Psi^1) \cdot \mathbf{X}^{(1,T-1)}) \\ & + 2w_2 (\mathbf{X}^{(3,T)'} \cdot \text{kron}(I_{T-2}, \Psi^2) \cdot \mathbf{X}^{(1,T-2)}) + \dots \\ & + 2w_L (\mathbf{X}^{(L+1,T)'} \cdot \text{kron}(I_{T-L}, \Psi^L) \cdot \mathbf{X}^{(1,T-L)}), \end{aligned}$$

\mathbf{X} is the $NT \times k$ matrix of stacked explanatory variables, $\mathbf{X}^{(t,s)}$ is the stacked explanatory variables starting time t and ending s , I_T is the T-dimensional identity matrix and $\Psi^s = E(v_t \cdot v_{t-s})$ and $w_s = 1 - 1/(L+1)$. Note that *kron* is the kronecker product operator.

We report the results of the panel estimation in Table 1. We choose $L = 4$. Higher values for L do not alter the significance of the parameter estimates. We report the results with regional dummies and without.

The first panel gives the results without dummies. The results are consistent with earlier findings. The Balassa Samuelson effect is present for all decades and increases over time.¹⁹ For recent years, the implied elasticity of the price level with respect to income suggested by the panel estimates of b is around one half. This is close to the results from the cross-sections of the Penn Tables.

¹⁹ We obtain similar results to those provided in Figure 1 for pooled versions of (1).

Table 1
Panel Estimates with Robust Standard Errors

	b	se	R-square	Number of countries
1950-1960	0.220	0.030	0.29	58
1960-1970	0.200	0.022	0.40	58
1970-1980	0.289	0.026	0.49	58
1980-1990	0.314	0.008	0.71	58
1990-2000	0.363	0.012	0.87	55
1950-1970	0.211	0.019	0.36	58
1970-2000	0.333	0.011	0.74	55
1950-2000	0.305	0.010	0.64	55
With Regional Dummies				
1950-1960	0.388	0.038	0.46	58
1960-1970	0.288	0.037	0.48	58
1970-1980	0.392	0.041	0.61	58
1980-1990	0.342	0.028	0.75	58
1990-2000	0.319	0.022	0.89	55
1950-1970	0.353	0.028	0.45	58
1970-2000	0.358	0.019	0.76	55
1950-2000	0.377	0.016	0.68	55

Notes: The estimates use an OLS panel estimator with a Newey-West autocorrelation and heteroscedasticity consistent covariance estimator. The second panel includes regional dummies.

The second panel gives the results with regional dummies. The dummies do not change the support for Balassa Samuelson for early decades. On the contrary, they strengthen the case for Balassa Samuelson. Note also that b is relatively constant over time with this specification.

As we have seen, the endogeneity of nominal can lead to an overstatement of b . As a solution, Table 2 provides instrumental variable estimates. We use the fifth lag of nominal

income as our instrument. This is the appropriate lag given our previous choice of four lags to adjust for autocorrelation.²⁰ Table 2 provides the results.

Table 2
Panel Estimates with Instrumental Variable Approach

	b	se	r-square	Number of countries
1955-1964	0.189	0.028	0.33	58
1965-1974	0.219	0.024	0.47	58
1975-1984	0.277	0.026	0.53	58
1985-2000	0.346	0.012	0.85	55
1955-1974	0.205	0.019	0.40	58
1975-2000	0.329	0.012	0.76	55
1955-2000	0.296	0.011	0.66	55
With Regional Dummies				
1955-1964	0.304	0.04	0.45	58
1965-1974	0.275	0.042	0.56	58
1975-1984	0.313	0.047	0.62	58
1985-2000	0.276	0.022	0.86	55
1955-1974	0.299	0.029	0.50	58
1975-2000	0.303	0.022	0.78	55
1955-2000	0.330	0.018	0.69	55

Notes: We pool using an OLS panel estimator with a Newey-West autocorrelation and heteroscedasticity consistent covariance estimator.

The previous results continue to hold: Balassa Samuelson is present for all decades. As we might expect, instrumenting for nominal income reduces b . For the final decades, the b coefficients with instruments imply a β value in the 0.38-0.45 range, smaller than the implied estimate of 0.5 without instruments.

²⁰ The counterpart of (10) for instrumental variables replaces all front matrices \mathbf{X} by the instrumental variable $\tilde{\mathbf{X}}$ in all matrix products. For example, replace $\mathbf{X}'\mathbf{X}$ by $\tilde{\mathbf{X}}'\mathbf{X}$, replace $(\mathbf{X}' \cdot \text{kron}(I_T, \Psi^0) \cdot \mathbf{X})$ by $(\tilde{\mathbf{X}}' \cdot \text{kron}(I_T, \Psi^0) \cdot \mathbf{X})$.

5 *Some Extensions*

The previous section used instrumental variables to adjust for endogeneity of nominal income. This section develops an alternative procedure based on a Monte Carlo simulation. We then crosscheck the results with benchmark data from the International Comparison Project (ICP).

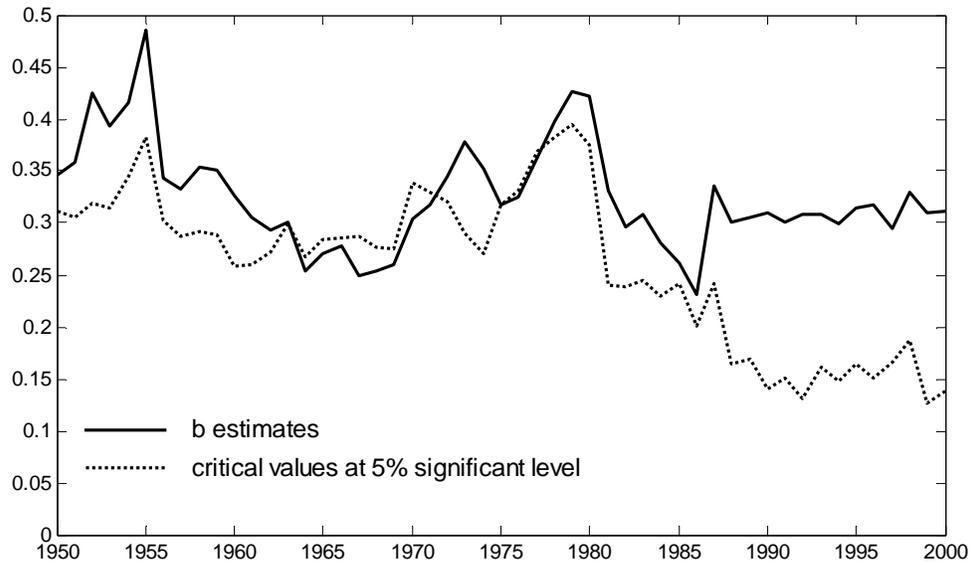
a. A Monte Carlo Simulation

The expected value of b from (7) is not zero due to the endogeneity of b . We use a Monte Carlo simulation to explore this bias. The simulation works by applying least squares estimation to simulated nominal income data where we impose the condition that there is no Balassa Samuelson effect. Repeating the simulation five thousand times, we determine the distribution of the b estimates from (7) for each year. By comparing the critical values from the simulated distribution to our estimates of b , we determine their statistical significance. Appendix Two provides a full account of the procedure.

Figure Two provides the critical values for the b distribution at the upper five percent level derived from the Monte Carlo simulations under the null that the Balassa Samuelson effect is zero. For comparison, we provide the b estimates by year estimated from the cross sections of the Penn data. For reasons explained in the appendix, we include regional dummies in the cross-sections.

When the estimated b coefficient exceeds the critical value in Figure 2 we reject the null hypothesis that β is zero. For most years, the results suggest a statistically significant Balassa Samuelson effect. The only cases where we do not reject the null are for the middle 1960's.

Figure 2: Estimates of b and Simulated Critical Values



b. Benchmark Data

The final confirmation uses benchmark data from the International Comparison Project. The OEEC provide benchmark GDP and price level comparisons for 1950 and 1955 covering nine developed economies. Published ICP data covers 1967, 1970, 1973, 1975, 1980, 1985, 1990 and 1996.²¹ The benchmarks are largely free from measurement error. What do they tell us about Balassa Samuelson? As it turns out, this is a more difficult question than it might seem. The problem is OEEEC/ICP data do not cover the 1960's and cover only a small number of developed economies for the 1950's. The 1950's data lack the variation in prices and income necessary to test Balassa Samuelson. After 1970 as only the 1975, 1980 and 1996 rounds have developing economies. In

²¹ If exclude Germany and Yugoslavia from the study due to boundary changes, the number of countries for each year are 1950(8), 1955(8), 1967(5), 1970(15), 1973(15), 1975(32), 1980(34), 1985(20), 1990(21), 1996(95). Our data sources are: 1950 and 1955 from Gilbert and Kravis (1958), Table 5 page 30. 1970, 1973 and 1975 from Kravis, Heston and Summers (1975, 1982). 1980, 1985 and 1990 is from Maddison (1995). 1996 is from Heston and Aten (2002), Table 2 page 31.

addition, the ICP data are available only in Fisher ideal form whereas the Penn estimates are in 1996 world prices. As a result, the ICP results are not directly comparable with those from the Penn Tables.

What do the benchmark data show? Estimating equation (1) for 1950 and 1955, we find no statistically significant effect. This is not surprising given the limited variation in prices and income. On the other hand, the Balassa Samuelson effect is present for other rounds.

To summarize the data, we estimated (1) and (7) by pooling across the various OEEC/ICP rounds.²² Depending on the specification, equation (1) provides estimates of β in the 0.25-0.35 range. These are lower than the Penn results reflecting differences between the Fisher Ideal measures of the OEEC/ICP and the 1996 world prices of the Penn Tables. For equation (7), the benchmark data provide coefficient values for b clustered in the 0.28-0.32 interval implying a β in the 0.39 to 0.50 range. The benchmark data are thus broadly consistent with our earlier findings in the sense that Balassa Samuelson appears to be present in the data and the β implied by the OLS estimates of b from (7) is consistently higher than the β obtained from (1). To our surprise, when we formally tested whether the β 's differed for year, we find little evidence that this is the case suggesting that Balassa Samuelson is present in the data and stable over time.

²² Following the procedures of Kravis (1976) and Maddison (1995), we calculate relative GDP with revised data on nominal GDP. This means that our estimates differ from the original ICP Fisher GDP benchmarks. For the most part, the differences are small.

6. *Summing Up*

The Balassa Samuelson effect is fundamental to our understanding of what occurs during economic growth. Yet, standard data sets only show evidence of a positive relationship between real income and the price level for recent decades. We attribute this result to GDP measurement error. After taking account of measurement error in real GDP and the endogeneity of nominal income, we find evidence supporting Balassa Samuelson for all post-war decades. These results hold for the Penn Tables. They also hold for the benchmark data from the International Comparison Project. We conclude that Balassa Samuelson is a robust feature of the post-war data.

In common with Bergin et al (2006), we find that Balassa Samuelson is strengthening over time. As we have pointed out, this occurs because of a decline in relative price levels for low-income economies. Why have price levels for poor economies fallen relative to developed economies? One possibility is globalization. Since the late 1980's, the developing world has seen remarkable increases in openness. Wacziarg and Welsh (2003) building on earlier work by Sachs and Warner (1995) categorize economies as open or closed for each year from 1950 to 2000. Only twenty percent of the economies in our sample are open for 1950. By 2000, ninety-five percent are. This explains the price falls, as we would expect trade liberalization and openness generally to lower the price level. The timing also fits as the rise in openness occurs mostly after the mid 1980's. In the end, the strengthening of Balassa Samuelson over recent decades may turn out to be yet another by product of globalization.

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Appendix 1: The Measurement Error Bias in Tests of Balassa Samuelson

This section derives an expression for the bias in tests of Balassa Samuelson due to GDP measurement error. For convenience, we repeat equations from the text.

Assume that the following relationship holds between the relative price level and relative income in terms of the US:

$$(1a) \quad p_i^* = \alpha + \beta y_i^* \text{ where } y_i^* = Y_i - p_i^*$$

Suppose, however, we observe p_i and y_i such that

$$p_i = p_i^* + v_i \text{ and } y_i = Y_i - p_i = Y_i - (p_i^* + v_i) = y_i^* - v_i$$

Assuming that the error term, ε , obeys the usual restrictions we get (2a).

$$(2a) \quad p_i = \alpha + \beta y_i + w_i \quad \text{where } w_i = \varepsilon_i + (1 + \beta)v_i.$$

Note that the independent variable, y_i and error term, w_i , are correlated. The OLS estimator

$\hat{\beta}$ is:

$$\hat{\beta} = \frac{\left(\frac{1}{n}\right) \sum_i (y_i - \bar{y})(p_i - \bar{p})}{\left(\frac{1}{n}\right) \sum_i (y_i - \bar{y})^2} = \frac{\left(\frac{1}{n}\right) \sum_i \left(\left(y_i^* - \bar{y}^* \right) - v_i \right) \left(\beta \left(y_i^* - \bar{y}^* \right) + \varepsilon_i + v_i \right)}{\left(\frac{1}{n}\right) \sum_i \left(\left(y_i^* - \bar{y}^* \right) - v_i \right)^2}$$

$$= \frac{\left(\frac{1}{n}\right)\left(\beta\sum_i\left(y_i^* - \bar{y}^*\right)^2 - \beta\sum_i\left(y_i^* - \bar{y}^*\right)v_i - \sum_i\varepsilon_i v_i - \sum_i v_i^2\right)}{\left(\frac{1}{n}\right)\left(\sum_i\left(y_i^* - \bar{y}^*\right)^2 - 2\sum_i\left(y_i^* - \bar{y}^*\right)v_i + \sum_i v_i^2\right)}$$

Where

$$\text{plim}\left(\frac{1}{n}\right)\sum_i\left(y_i^* - \bar{y}^*\right)v_i = \text{plim}\left(\frac{1}{n}\right)\sum_i\varepsilon_i v_i = 0$$

since y_i^* , ε_i and v_i are independent. Let $\sigma_{y^*}^2 = \text{plim}\left(\frac{1}{n}\right)\left(\sum_i\left(y_i^* - \bar{y}^*\right)^2\right)$. Using the Slutsky

theorem to find the probability limits of $\hat{\beta}$, we get

$$\begin{aligned} \text{plim}\hat{\beta} &= \frac{\beta \cdot \text{plim}\left(\frac{1}{n}\right)\left(\sum_i\left(y_i^* - \bar{y}^*\right)^2\right) - \beta \cdot \text{plim}\left(\frac{1}{n}\right)\left(\sum_i\left(y_i^* - \bar{y}^*\right)v_i\right) - \text{plim}\left(\frac{1}{n}\right)\left(\sum_i\varepsilon_i v_i\right) - \text{plim}\left(\frac{1}{n}\right)\left(\sum_i v_i^2\right)}{\text{plim}\left(\frac{1}{n}\right)\left(\sum_i\left(y_i^* - \bar{y}^*\right)^2\right) + 2 \cdot \text{plim}\left(\frac{1}{n}\right)\left(\sum_i\left(y_i^* - \bar{y}^*\right)v_i\right) + \text{plim}\left(\frac{1}{n}\right)\left(\sum_i v_i^2\right)} \\ &= \frac{\beta \cdot \sigma_{y^*}^2 - \sigma_v^2}{\sigma_{y^*}^2 + \sigma_v^2} = \frac{\beta - \frac{\sigma_v^2}{\sigma_{y^*}^2}}{1 + \frac{\sigma_v^2}{\sigma_{y^*}^2}} \end{aligned}$$

Appendix 2: Obtaining the Simulated Distribution for the b estimates

This appendix describes the Monte Carlo simulations from Section Five. To explain our procedures, we repeat equation (1) below as equation (1b) where ε_{it} in (1b) is assumed to be normally distributed with mean zero and standard deviation, σ_{ε_t} .

$$(1b) \quad p_{it} = \mathbf{Z}_{it} \cdot \boldsymbol{\alpha}_t + \beta_t y_{it} + \varepsilon_{it}$$

We focus is on b estimated from from equation (7) in the text, given here as (7b).

$$(7b) \quad p_{it} = \mathbf{Z}_{it} \cdot \mathbf{a}_t + b_t Y_{it} + e_{it}$$

Our objective is to obtain the distribution of the b coefficient when the Balassa Samuelson effect is zero, that is under the null that $\beta = 0$ in equation (1b). Recall that b will not equal zero in this case given that nominal income is endogenous.

The simulation data comprises the following: the observed exogenous variables, \mathbf{Z} ; simulated price data generated by equation (1b) under the null hypothesis that Balassa Samuelson is zero; simulated nominal income constructed to include the same (simulated) noise term as the price level.

The parameters for the simulation come from unconstrained estimates of the system using actual data. In estimating β , we assume that the 1996 ICP benchmark data is error free. We choose 1996 for two reasons. First, it is the benchmark year of the Penn Tables. Second, we have benchmark data for all our economies from Heston and Aten (2002). To implement the simulation we also have to assume that β is constant over

time.²³ We calculate b from β using the fact that $b = \beta/(1+\beta)$ as in (7b). To be consistent with this assumption, we use regional dummies in our estimation of (7b) as this produces estimates of b that are also constant. Given b , we regress $p_{it} - b \cdot Y_{it}$ on \mathbf{Z}_{it} to estimate \mathbf{a}_t .

In the next step, we obtain the “predicted” price at time t , as $\hat{p}_{it} = \mathbf{Z}_{it} \cdot \mathbf{a}_t + bY_{it}$. We then use the sample standard deviation of $p_{it} - \hat{p}_{it}$ at time t , that is the actual minus the predicted price level, as our measure of σ_{ε_t} .

Next, we obtain the simulated price levels. First, we generate ε_i for time t with mean zero and standard deviation σ_{ε_t} using a random number generator. We simulate price from $p_i = \mathbf{Z}_{it} \cdot \mathbf{a}_t + \varepsilon_i$, where $\mathbf{a}_t = \mathbf{a}_t(1 + \beta)$. This imposes a zero Balassa Samuelson effect. To obtain the distribution of b , the simulated nominal income must contain the same noise terms as the simulated price data. To ensure this is the case, we generate “basis” real income $y_{it}^b = Y_{it} - \hat{p}_{it}$. From this, simulated nominal income is $Y_i = y_{it}^b + p_i$, where p is the simulated price level.

The final step uses the simulated p_i , Y_i and \mathbf{Z}_{it} to estimate (7a) by least squares. We repeat this procedure 5000 times to get the distribution of b estimates.

²³ Alternatively, we could use the price predicted by estimating (7b) directly as “predicted” prices and the Penn real income measure as “basis” real income even if the actual real income is subject to large measurement errors. Because we use the β estimate of 1996 to generate the predicted prices for all t , our measure of the standard deviation σ_{ε_t} is larger, resulting in a wider confidence band for b than the alternative. Thus, our procedure is more likely to accept the null of no Balassa-Samuelson effect.