

“Comment on ‘China’s Current Account and Exchange Rate,’  
by Yin-Wong Cheung, Menzie Chinn and Eiji Fuji”

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When one reads in the second paragraph of this nice paper, “there are very few studies that simultaneously assess the Chinese exchange rate and trade/current account balance” one’s first reaction is: “that is true; I wonder why analysts haven’t addressed them together; and this will be a useful contribution.” And the paper does turn out to be a very useful contribution; the authors do their usual careful job with the econometrics, while linking directly to some of the most important questions in international macroeconomic policy today.

One doesn’t have to read much further, however, before being reminded why quantitative research on the Chinese exchange rate and trade balance has been stunted. There are reasons to be pessimistic about getting good results econometrically. First, as the authors say, “the data pertain to an economy experiencing rapid structural change.” (p.27) Second, the exchange rate has usually been de facto fixed, in the past under a dual exchange rate system and even today supported by capital controls. Neither the domestic financial market, nor international capital flows, nor the exchange rate itself are determined by market forces. Flexibility in the nominal exchange rate has been so low and the current probably “misalignment” so high, that there is little hope in estimating an econometric equation to determine the exchange rate. According to some theories one gets the same real exchange rate regardless the regime: if nominal flexibility is suppressed, then fundamentals show up in the price level instead. But we know from lots of evidence that, in practice, if a country like China holds the nominal exchange rate fixed at a time, it will prevent or at least delay real adjustment from occurring. This is especially true if the authorities sterilize their reserve inflows, as the People’s Bank of China has been remarkably successful at doing. One need not pass judgment on the wisdom of Chinese policy, let alone call it “manipulation,” to realize that the RMB has been sufficiently insulated from market forces that models designed for other countries are unlikely to work well in this context.

Cheung, Chinn and Fuji (CCF) estimate the Renminbi to be undervalued on the order of 50% in log terms. This is close to some other estimates PPP-based relationships, such as my own 45% (in log terms), relative to the line representing the Balassa-Samuelson relationship. In other words, it is not just that China’s absolute prices are at current exchange rates only an estimated 23% of prices in the United States, but they are 45% below even after taking into account what is normal for countries at

China's stage of development, as measured by income per capita. Admittedly the extant range of estimates is far larger once one ventures beyond the B-S/PPP approach.

Large as the CCF point estimate is, probably their most important message is that the standard error on their calculation is so large that this undervaluation is not statistically significant. I hadn't done this particular test myself, but one probably shouldn't be surprised. It continues in the train of negative findings regarding models of exchange rate determination that has been the dominant tradition ever since Meese-Rogoff. And one can eyeball from the graphs that the  $R^2$  of the relationship is sufficiently low that most countries will find themselves with the "normal range of variation" around the Balassa-Samuelson line.<sup>1</sup>

Note that this is very different from saying that the Balassa-Samuelson relationship itself is not statistically significant. It is significant, in the results of CCF, as in my results and those of many others before us. What then should one make of the undervaluation estimate for the RMB? I will come back to this later.

Sticking to the paper itself for now, there are two directions to go, from the finding of statistical insignificance. The first is to note that the absence of statistically significant predictive power, or even stable explanatory power contemporaneously, undermines the argument of those who would say that the RMB is clearly undervalued based on economic fundamentals. "These findings highlight the great degree of uncertainty surrounding empirical estimates of "equilibrium real exchange rates," thereby underscoring the difficulty in accurately assessing the degree of CNY undervaluation." One could point out that the disagreement among economists, using different economic models, i.e., the tendency of some (though a minority) of prominent international economists to say that the RMB is very fine where it is thank you, further undermines the claim that any objective mode of inquiry would find the RMB clearly undervalued.<sup>2</sup>

This is a very important point, as the "misalignment" or even "manipulation" is the position of US politicians of both parties and could well soon result in Congress passing the aggressive sort of legislation that it has been threatening for three years. It is also in some sense now the position of the IMF, explicitly in its recent Article IV consultations and implicitly in the research agenda associated with the Multilateral Consultative Group on exchange rate surveillance. And if one cannot come up with a clear unambiguous answer to the question for the RMB, what hope is there for other exchange rates, where it is typically far more difficult still to pronounce the currency unambiguously undervalued or overvalued. (Often, as for the US dollar, one criterion like PPP can point in one direction while others like the current account balance or overall balance of payments point just as clearly in the opposite direction. This is not the case with China.)

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<sup>1</sup> I am here using the term "Balassa-Samuelson relationship" to refer to the correlation between real exchange rates and real income or labor productivity. As CCF point out, there are other theories that could explain the correlation besides the one that Balassa and Samuelson had in mind, which was faster productivity growth in tradables than nontradeables.

<sup>2</sup> Mundell, McKinnon, Cooper, and Dooley and Garber.

The econometric approach taken by the paper itself goes in the direction of concluding, “oh well, we can never reliably say what determines exchange rates; so let’s just take them as given, and go on to the other half of the topic, the effect of exchange rates, however they are determined, on trade flows.” Here statistical significance is more easily obtained, but some of the point estimates are less as one would normally expect, as the authors note. Exports are found to respond to depreciation in the usual positive way. But imports are also found to respond positively to depreciation. This is not what is normally expected (though it apparently is what Marquez and Schindler also found).

I have one thought here. It has probably already occurred to CCF (and Marquez as well), so let me phrase it as a question. We all know that Chinese trade is heavily dominated by gross exports that have high import content. Most obviously, many of the imports are raw materials or intermediate inputs that go into production generally, and especially production of exports. The econometricians have separate statistics on “parts and processing imports;” the perverse sign applies at least as strongly to “ordinary imports” as to these. But I suspect that even among “ordinary imports” a lot is re-exported after the contribution of some domestic value added. So the question is this: could the perverse sign of the effect of the exchange rate on imports reflect that component of imports that is closely tied to exports, where the sign is clear?<sup>3</sup> Have you considered adding an additional term to equation (7), so it reads:

$$im_t = \gamma_0 + \gamma_1 y_t + \gamma_2 q_t + \gamma_3 w_t + X_4 ex_t + u_i \cdot$$

The additional terms is Exports,  $ex_t$ , and the coefficient  $X_4$  represents their import content. Since exports are endogenous, the equation would have to be identified by means of foreign income  $y^*_t$  which enters the export equation. Perhaps controlling for exports would restore  $\gamma_2$ , the coefficient on the real exchange rate, to its conventional sign.

I want to return now to the question how to think about residuals from the B-S relationship. CCF say (p.12) “It is indeed a puzzle that the CNY path is different from the one predicted by the Balassa-Samuelson hypothesis.” This refers not just to the magnitude of the residuals, but to the movement over time: the failure of the Chinese currency to appreciate in real terms as the economy has experienced rapid growth in income per capita.

Let me begin by suggesting a distinction between the B-S relationship and the B-S effect.<sup>4</sup> Most discussion of B-S talks as if countries are always on the B-S line, except perhaps for minor regression errors. Specifically, the B-S effect is the proposition that for every 1% increase in labor productivity or income per capita, there is a .3% real appreciation (or whatever the estimate is); in other words, all movements are implicitly assumed to be movements along the line. But we know the residuals are large *and we have lots of well-articulated theories to explain this*, theories of real effects following from monetary disturbances, or other demand-side disturbances. These theories

<sup>3</sup> We also know that exports are much larger than imports for China; so either way, we are left with the conclusion that a real appreciation would reduce the trade surplus, as desired.

<sup>4</sup> This is in addition to the distinction vis-à-vis “the Balassa-Samuelson theory”, flatted in my first footnote.

encompass a large share of open-economy macroeconomics outside B-S. There is no reason why we have to choose between B-S and theories of devaluation or monetary policy. Both are important. The theories of devaluation and monetary policy are the obvious candidates to explain the residuals from the B-S relationship (or some share of those residuals; nothing fits perfectly).

Real exchange rates are influenced not solely by the long-term trend of the Balassa-Samuelson effect nor solely by the short-term fluctuations of monetary policy and nominal exchange rate changes, but rather are influenced by both.<sup>5</sup> A reasonable characterization is that in the long run Balassa-Samuelson factors dominate, but in the short run monetary factors can pull the real exchange rate away from the Balassa-Samuelson equation. This framework contains the powerful prediction that if a country lies substantially off the Balassa-Samuelson regression line in one year, it can be expected to return part way – not necessarily all the way – to the regression line over the subsequent decade. This claim has important implications for our ability to make predictions, and furthermore is borne out by data from the last two decades.

Here are examples of what I have in mind as possible explanations, anytime one observes a country lying well below the B-S line:

Macro influences such as:

- Devaluation in the presence of either sticky goods prices or non-traded goods
- “Exogenous” depreciation under floating, with either sticky prices or non-traded goods
- Monetary contraction/deflation, with a fixed exchange rate
- Increased demand for currency, e.g., from rapid supply-led growth, with a fixed exchange rate.

In the case of China, it is the last of these cases that is the relevant one. The RMB has been de facto fixed to the dollar for the last 10 years or so. If one goes back further, the exchange rate regime was different; but it the currency has never floated or been determined in a flexible way. Yet China has experienced tremendous growth in productivity and real income over the last quarter-century, perhaps the greatest the world has ever known. So even if the RMB had been on the Balassa-Samuelson line 30 years ago, it would not be on it today. According to some theories, the exchange rate regime makes no difference: if the exchange rate is held fixed, then the economic fundamentals that would show up as a nominal appreciation under floating will instead show up in the form of inflation. You will get the same real exchange rate either way. But this is not how the world works. The large reserve inflows may finally now be starting to show up in an overheating economy, but the effect on inflation so far has been small. I do believe that the pressures will be increasingly in this direction. But the dollar peg has greatly delayed and diminished the effect on the real exchange rate. In addition to heavy foreign exchange intervention, the authorities have made heavy use of capital controls and

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<sup>5</sup> One does not necessarily need prices of non-traded goods to be sticky – let alone prices of traded goods – to get the result that devaluations or changes in monetary policy can have transitory effects on the real exchange rate in the short run. Dornbusch (1973).

sterilization, with the result that the trend rate of increase in the money supply has not much exceeded the trend rate of growth of the real economy.

This puts the CCF finding regarding the estimated undervaluation of the RMB in a very different light. Ideally, we would add to the Balassa-Samuelson equation additional terms to capture monetary influences that in the short or medium run pull the real exchange rate away from its equilibrium value. Possible variables include real interest rates, acceleration in the real money supply, sudden nominal devaluations or revaluations. I tried some of these recently, and got nowhere. But that may reflect a failure of my specification. For example, none of these terms would capture the presumed source of the large Chinese residual: an essentially fixed exchange rate during a period of rapid supply-side growth. So we need a better specification to capture the intermediate term macro influences. But, in the meantime, my point still holds. Whether the distance between China's real exchange rate and the B-S line is statistically significant is not necessarily the point.

I am not suggesting that one should pronounce the RMB substantially undervalued based solely on a point estimate. For one thing, aside from statistical significance, I don't think any of us really knows or feels entirely comfortable with what goes into the estimated Chinese price level in the Penn World Table.

I can tell you how I think of it, when we turn from the mores of scholarly papers to the world of policy. I can think of 4 or 5 independent criteria for addressing the question whether China should, in its own interest, allow the RMB to float upwards. For most countries, some of these criteria would point in opposing directions. Correspondingly, for most countries I am not willing to proclaim publicly that I view their currencies as either overvalued or undervalued. For China, it seems to me that the all point in the same direction:

- (i) PPP/Balassa-Samuelson.
- (ii) Trade balance / balance of payments / level of reserves
- (iii) Overheating economy
- (iv) Desirability of an economy as large as China's having its own monetary policy, without relying on capital controls to continue to insulate against disturbances in the future.
- (v) Desirability of choosing a time of strength to make the move away from a peg, rather than waiting for a time of weakness or even crisis.

It is on this basis that I do take the stand that China should let the currency appreciate. (This has nothing to do with pressure from US politicians, which I regard as ill-advised for a whole other set of reasons). What the CCF paper has to offer – the estimate of undervaluation relative to Balassa-Samuelson, and estimates of what effect an appreciation would have on exports and imports -- are useful inputs to these considerations.

## **References**

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