

## **The paradox of monetary profits, MS 384**

### *Potentially significant?*

The paper is interesting because it mixes preoccupations that arise from various economists and traditions in research, that the authors try to integrate, regarding the realization and measure of monetary profits.

The authors then move on to the role of finance in generating changes in income, and in particular on how this should be taken into consideration in the so-called stock-flow consistent approaches. As the authors point out in their conclusion, their concern is not with how national income ought to be calculated; rather their concern is with the measure of sectoral income that is needed to get a grasp on how financial markets will influence future production.

### *Is the analysis correct?*

My comments will be devoted to this second part of the paper, which starts in section 5. I should say that I am sympathetic with the purpose that the authors wish to achieve, but I disagree with several of their claims. I believe that the paper can be modified in a way that would more truly represent the views of current proponents of the stock-flow approach.

The authors seem to oppose the views of Patterson and Stephenson (1988) to that of Godley and Lavoie (2007) and Dos Santos and Zezza (2008). However, in their critique of the latter group of authors, I think that the authors are themselves confused about two issues when they criticize Godley and Lavoie: how to measure national income and have integrated balance sheets on the one hand, and how to take into account the impact of the financial sector on behavioural equations on the other hand.

There is no difference whatsoever between Patterson and Stephenson (1988) and Godley and Lavoie (2007). The balance sheet of the former (Table 1, p. 791) is identical to the balance sheets proposed by the latter. In both cases, assets are evaluated at market prices, so that net wealth of the consolidated economy is equal to the value of tangible capital assets (see also 4th line of page 492). The full-integration matrix of Godley and Lavoie (p. 44) confirms that the approach advised by the two sets of authors is the identical. Table 2 of Patterson and Stephenson is identical to it: an increase in the prices of equities lead to a *reduction* in the adjusted income of the firm sector (under the assumption that firms hold no equities). Note that Lavoie and Godley (p. 30) also point out that proper accounting of the balance sheets, from a national accounts point of view, has no bearing on behaviour: "Indeed, in the book, no behavioural relationship draws on its definition".

Godley and Lavoie in their behavioural equations do exactly what the authors suggest: they take into account capital gains or losses. This is clear as early as chapter 5, where there is a long discussion about the Haig-Simons (quasi-Hicksian) definition of household income and the role of wealth on consumption behaviour. In the Lavoie and Godley model (JPKE, 2001-2), consumption depends explicitly on capital gains on the stock market. The only critique that one

could address to Godley and Lavoie is that they do not incorporate in their models the fact that firms also hold equities issued by other companies. This is the main difference between what the authors are doing and what Godley and Lavoie are proposing. Otherwise, there is a long discussion about measured profits when the prices of inventories change (a kind of capital gain), and Godley and Lavoie (ch. 8) go out of their way to explain that what counts from the point of view of firms and for their production decisions are profits measured by taking into account stock appreciation and not the profits as measured by national accountants.

The authors claim “despite their aim to investigate finance, they [Godley and Lavoie] do not have any experiments illustrating the consequences of such changes in the price of equity” (p. 17). But this is incorrect. There is such an experiment in chapter 11, pages 431-435 (and there was also a similar experiment in the Lavoie and Godley JPKE paper of 2001-2). In their models, financial markets “do have an impact on the real sphere of income generation”, which is what the authors are looking for!

On page 23, the authors propose an alternative approach. Perhaps it can work. But note that their table 5 is not consistent with Patterson and Stephenson. Second, it is not sure that it could be extended to the government sector and its securities. Third, the notation is a bit confusing.  $E_s$  stands for equities issued in the past. But what we really have is a number of securities  $E$  issued at some average historical price, say  $p_a$ .  $E$  and  $E_s$  in the total column ought to be the same variable. The capital gain is in the difference between the average historical price and the current price. Fourth the price of capital goods can also increase (faster than the average price of real GDP), so that there should also be an entry for tangible capital goods in the revaluation matrix. Finally, I must say that I am totally confused about the last line in the revaluation matrix (Table 7):  $p\Delta E^s - \Delta E^s$ . Also is this  $E^s$  different from  $E_s$  ?