Stabilizing an Unstable Economy: On the Choice of Proper Policy Measures
(Anonymous version, November 3, 2009)

What the paper is about

The paper seeks to study some selected aspects of the real-financial interaction at the macroeconomic level. To this end it goes beyond partial mechanisms and sets up a model that integrates product, labour and financial markets, the latter with money, government bonds and equities of firms as imperfectly substitutable assets. The sectors considered are worker households, asset owner households, nonfinancial firms, and a fiscal and monetary authority. Even if one might ask for more sectors or assets, this is already a very ambitious endeavour. After all, the full model in its reduced form comprises nine differential equations.

Methodologically, three kinds of stability statements are made. First, the (unique) steady state of the full model is claimed to be stable if certain behavioural parameters are low (or high) enough, where for the mathematical proof(s) the reader is referred to other work. In particular, this gives some hints where one might suspect sources of instability. Second, the authors offer some numerical examples of the transition from stability to instability when one of the parameters is subjected to *ceteris paribus* variations, and an example of the global dynamics if an (unspecified) downward wage rigidity is introduced. Third, some analytical stability results are obtained in lower-dimensional submodels, where the other variables are frozen at their steady state values. Most interesting, and certainly debatable, is here a modification of the original monetary policy rule that allows the central bank to buy and sell on the stock market in a countercyclical manner.

The paper is sufficiently substantial to be included in the *Journal Articles*—though after a thorough revision. Below a number of conceptual issues and points for clarification are listed that may be taken into account in this respect (more detailed issues of presentation are dealt with elsewhere). However, there remains a basic problem of the paper, which is the question as to whether its treatment of equities in the financing of investment is still appropriate; see point 1b in the next section. It is an editorial decision whether this problem is considered so serious that it denies the paper sufficient economic significance, or whether it assigns a higher priority to its general methodological merits.

Conceptual issues and problems of significance

1. In my opinion, the most fundamental problem is the role of equities in the model. There are two aspects to them, where the second aspect is more serious than the first.

    (a) Equity prices are simultaneously determined at the same level as the other macroeconomic variables in the model, which determine them *via* the portfolio equilibrium of asset holders and which in turn are directly or indirectly influenced by Tobin's (average) q. If the model exhibits oscillatory behaviour, which is one (here implicit)) perspective of the authors, all of the variables would fluctuate at the same frequency. In contrast, in the real world stock prices fluctuate at a higher frequency, or the upward and downward "trends" over several years (fortunately) do not seem to carry over so directly to the real sector.

    (b) Issuing new shares is the only source of financing fixed investment of firms.[1] Since there is neither debt financing nor even retained earnings, a growing economy requires equities to be rising over time. This is in stark contrast to (at least) US reality, where just the opposite has taken place over the last two or three decades. Thus, the ratio of net issues of nonfinancial corporate equities to fixed investment was roughly -15% over the 1980s, -8% in the 1990s, and -14.5% between 2000 and 2006.[2]

    It may be concluded from these observations that the treatment of equities is not just a convenient simplification in the model; one may also wonder how reliable its results and conclusions are in the end . Might be that by and large they are adequate, but then for the wrong reason!?

2. While the real interest rate channel is the centrepiece of the new macroeconomic consensus, it is practically absent in this model. Instead of the interest rate i on short-term government bonds or its (negative) spread to the (expected) rate of profit, fixed investment (as it is made clear enough)

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[1]The influence of undesired changes in inventories is only present for accounting consistency, and supposedly of minor importance.

[2]I found these numbers in a working paper by P. Skott and S. Ryoo, "Macroeconomic implications of financialization", University of Massachusetts, Amherst 2007 (published in *Cambridge Journal of Economics*, 32 (2008), 827–862). I do not know the corresponding statistics for the euro zone.

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is here determined by Tobin's q (in addition to an accelerator argument). The interest rate still enters aggregate demand, but only in a very indirect way, namely, when q and i are simultaneously determined in the temporary portfolio equilibrium.

(a) Nevertheless, if nowadays monetary policy is not described by some sort of Taylor rule (but instead by a constant growth rate of the money supply), this deserves an explanation, unless justification (which is missing).

(b) As a consequence of the previous observation: if someone does not particularly like the properties of the model or the conclusions drawn from them, he or she could easily put the model aside with the argument that the role of the interest rate has been unduly neglected. Hence the question: what kind of justification is there for the present neglect of the real interest rate channel, or counterattack against its prevalence in contemporary macroeconomic theory?

3. In the computer simulations, it seems to me that the slope coefficient of the wage Phillips curve is much higher than what is known from empirical estimations. Unfortunately, other numerical parameters or references to them are not given (although this should be common scientific practice).

However, the more fundamental problem is this: what is the economic significance of a (very sketchy) demonstration that "the high dimensional dynamics may have many sources of instability" if it based on simulations with numerical parameters that—so a moderately skeptical reader might suspect—are possibly fairly exotic, or arbitrarily selected? Could it not be that, over a wide range of "reasonable" parameter values, the steady state position is always stable? Or always unstable, and only some "unreasonable" parameter values would stabilize it. In other words, are there some criteria on the basis of which the present numerical parameters have been chosen?[3]

4. While the Tobinian portfolio equilibrium approach employed in the paper describes market clearing conditions in terms of the demand and supply of

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[3]This question may also be directed to a number of post-Keynesian models, in particular, the so-called stock-flow consistent models. As I see it, it should not be escaped for too long if heterodox theory wants to claim that it is competitive with respect to the new-Keynesian DSGE models, where currently a lot of estimation work (however convincing) is being done.

the stocks of the financial assets, footnote 2 mentions work of some of the authors that uses flow-oriented equations. Since the financial sector is in the centre of the analysis, the question of which method to choose is a crucial issue. One may therefore want one or two remarks on the grounds of which merits the authors now prefer the stock approach.

Problems for clarification

1. The determination of the capital gains $\hat{p}_e$ is nowhere made explicit. Only the (credible) assertion is made (on p. 11) that this "leads to very complicated expressions". The amendment that they "are here only made implicitly" is unclear to me. One may guess from its treatment later on that this matter is dealt with in a precise way in Köper (2003)—but is it? Also footnote 31 in the proof of Theorem 7 is potentially misleading. Is it to say that $\hat{p}_e$ could be calculated "in principle", but is not in this proof because it is too complicated?

2. The so-called Tobin tax introduced in equation (90) for the stock market is insufficiently explained. Does it really enter the determination of portfolio equilibrium, and how? Do only the chartists have to pay it? Besides, the property that "it subsidizes capital losses" (p. 30) seems to make it a rather academic specification.

3. One wonders what becomes of the portfolio equilibrium conditions if (in section 8) equities are also held by the central bank. In particular, if β_h, ε_h are the fractions of bonds and equities that are held by the asset owner households, then it seems that b and q in equations (63, 64) become $\beta_h\, b$ and $\varepsilon_h\, q$. But the ratios β_h, ε_h will change overtime?! Are they statically endogenous or are they dynamic variables proper? After thinking about that on p. 33 and then reading on, I found some remarks in that direction on p. 35—but they were too sketchy for me.

4. Concerning the countercyclical monetary policy of the central bank it would be clearer in the abstract and the introduction if it were said that the "assets" to be sold or bought by it are equities of the (nonfinancial) corporate sector. It would also be more modest if in the general descriptions of its stabilizing effects in the introduction or conclusion it were added that, so far, this has

only be established in a three-dimensional submodel (though this is not a minor achievement).