1 Responses to the first referee report

The report was very useful and clearly demonstrated deep insight into the issue discussed in our paper.

Commenting on the choice of capital stock variable: As also the other referee was concerned about our choice of capital stock variable to replace the simulated variable used by Peter Ireland, we begin with this issue. We agree completely that this choice is both extremely difficult and important. Capital stock is notoriously difficult to measure even with some minimum level of objectivity. The measure we have used here is from the OECD database Economic Outlook and is defined as "Private fixed total capital formation". In a previous version of the paper we also tried "Gross fixed total capital formation".[1] From these measures it would of course be possible to create K_t as in (3) using the $\delta = 0.975$. However, the correponding variable would be very close to I(2) and would be excludable from the cointegration relations from the outset as none of the other variables are close to I(2). This makes it extremely difficult to estimate a traditional Cobb-Douglas function and there might not be other solution than to use capital formation instead.

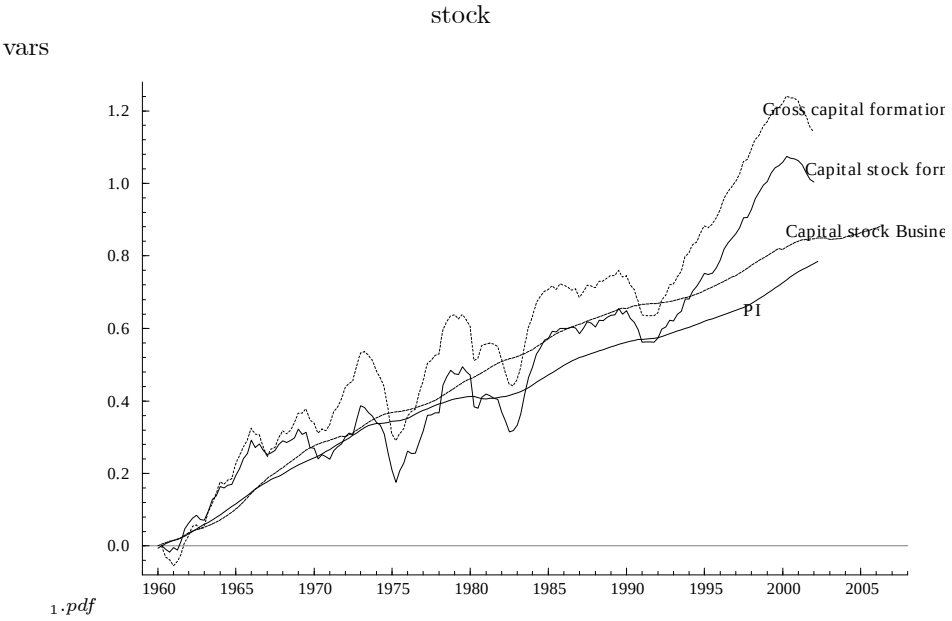

Figure 1 shows the graphs of Ireland's capital stock variable together with "log capital stock of the business sector, log private fixed capital formation and log gross fixed capital formation. All series are per capita and all are normalized by subtracting the first observation (1960:2) from the series to facilitate comparison. We note that the PI simulated per capita capital stock variable exhibits less growth over the sample compared to the three measured series.

[1] In the present version of the paper the latter name has been used by mistake.

Thus, imposing the RBC assumption of identical linear growth rates on the data generates a variable which is different from any of the measured ones. Furthermore, gross capital formation deviates most from PI's measure whereas the capital stock of the business sector looks most similar in this respect. Thus, it might be a good idea to check the sensitivity of the results with respect to the latter measure.

To conclude: cumulating any of the two capital formation series would give us a series that would be totally different from PI's capital stock variable. To use the capital stock of the business sector might be a good idea, but the question remains why the growth of this variable is smaller than private fixed capital formation.

Commenting on Point 1. We do not think that the relevant choice is between econometric modelling with and without theory. The crucial thing in our view is to discuss how theory is used in empirical modelling. I'll cite from Kevin Hoover (2006):

> "The Walrasian approach is totalizing. Theory comes first. Empirical reality must be theoretically articulated before it can be empirically observed. There is a sense that the Walrasian attitude is that to know anything, one must know everything. " .... "There is a fundamental problem: How do we come to our a priori knowledge? Most macroeconomists expect empirical evidence to be relevant to our understanding of the world. But if that evidence only can be viewed through totalizing a priori theory, then it cannot be used to revise the theory."..."The Marshallian approach is archaeological. We have some clues that a systematic structure lies behind the complexities of economic reality. The problem is how to lay this structure bare. To dig down to find the foundations, modifying and adapting our theoretical understanding as new facts accumulate, becoming ever more confident in out grasp of the super structure, but never quite sure that we have reached the lowest level of the structure."

Peter Ireland has clearly followed the Walrasian approach in the sense of postulating from the outset what the relevant theory is. Our approach is clearly Marshallian (or rather post-Walrasian) in the sense that we prefer to look at the data from the point of view of as many competing theories as possible and then try to use the cointegrated VAR approach to extract as much information as possible from the data. As demonstrated in Sections 6 and 7 of our paper the information set chosen by PI is far from sufficient to allows us to 'reach the super structure'. The analysis gives us 'some clues that a systematic structure lies behind the complexities of the economic reality', but it also suggests very strongly that we have to dig a lot deeper before we can be more confident in our conclusions. Based on the present version (and we admit that the choice of measurement for capital stock is a problem) we have basically obtained the following results:

1. The first period is better explained by the chosen data than the second.

2. In the first period we find a plausible, trend-stationary Cobb-Douglas production function, but in the second period the linear trend is econometrically excludable and the estimated Cobb-Douglas coefficients are implausible. We found that labour was clearly nonstationary and that there were two stochastic trends, one seemed to be the cumulation of shocks to consumption the other to labour.

Which clues can be dugged out from these results? We will illustrate a few points.

1. The trend-stationarity of the Cobb-Douglas function in the first period might suggest that TFP in fact could be approximated by a linear trend.

2. The fact that cumulated empirical shocks to labor acted almost as a stochastic trend in this system might suggest that it may capture some elements of 'labour augmented technological progress' (though this is a very tentative conclusion).

3. Thus, the first superficial digging in the first period might suggest that (1) the linear trend capturs both the linear growth in TFP and technological progress, (2) demand shocks have triggered the long business cycles together with (3) shocks to labor productivity (intensity).

4. The digging into the second period seems to suggest that the linear growth trend can no longer be approximated with a linear trend, but that cumulated shocks to consumption and labor seem to be pushing the system similarly as in the first period. Thus, demand shocks and labor productivity shocks may in fact play a similar role as before.

5. An interesting question is what should replace the insignificant linear trend in the second period. This prompts us to ask in what sense the two periods are different enough to explain the change in trend behavior. To us it seems obvious that the major difference is to be found in the degree of globalization, worldwide capital deregulation and increasing international competitiveness. This suggests that the second period, in particular, should not be modelled in the context of a closed economy, prompting for an extention of the economic model and the data. The persistent movements of the two cointegration relations in the second period might suggest that real exchange rates might do the job (as these have similarly exhibited a lot of persistence in the second period).

When we ask the question 'What does the data tell when allowed to speak freely?' we try to dig out as much information as possible at the background of as many theories as possible given the present quite limited information set. This is clearly contrary to using just one theory but certainly not the same as using no theory. We make no claim that we now know the 'truth' only that the analysis has provided a number of clues that might be useful when deciding how and where to continue digging.

Commenting on point 2. We both agree and disagree with the referee on this point. Yes, the VAR estimates can be, and often are, very sensitive to all kind of things, in particular the specification of deterministic components and the choice of sample period. This, however, is only a reflection of the fact that an economy is a very complicated and dynamically evolving entity. The idea that some highly stylized, simplistic, economic theory models would be able to catch the essence of this complicated reality decade after decade after decade is in our view absurd. Therefore, when using the VAR model to extract as much information from the data as possible it is mandatory to secure that all the empirical choices being made follow scientifically valid principles. For example, specification of deterministics (contant, trend, dummies) have to be made as objectively as possible (and not in order to influence the results in a desired direction), the choice of sample should as far as possible define reasonably constant parameter regimes, etc. The advantage of following such strict rules is that in the end one can claim that the VAR model actually represents the information in the data. Hence, empirical models that are inconsistent with the VAR results must then have imposed inadmissble restrictions on the data. A relevant question is, of course, whether it is at all possible to achieve such a goal. A long experience with cointegrated VAR models (probably several hundereds of various applications) suggest that it is possible to achieve a fairly high degree of reliability when strictly following statistically sound procedures[2]. In our view, the greatest threat to VAR modelling is the profession's fixation on starting with one prior model (the Walrasian approach) and then define success as the ability to make the data conform to this model. We dare say that many scientifically questionable choices have been made in the fulfillment of such endeavours. Ireland is just one example among numerous other.

Comments on the specifics: Most of the comments here are very valuable and need to be taken account of when revising the papers. Some of them have been discussed above. The question of a broken linear trend in 5. may need a comment: A broken linear trend could easily have been included in the model given that the problem was two different linear trend slopes between the two periods. As the problem seemed to be more complicated (a linear trend in the Cobb-Douglas production function in the first period, but something else in the second) we doubt very much that a broken trend would do the job. Something else seems to be at stake and the best choice seemed to split the sample.

[2] The econometric prinicples of such an empirical methodology are spelled out in Juselius (2006).