Geoff

In response to the claim that Keynes switches between expectation and expenditure in his summary propositions at the end of Chapter 3, pp. 28–29:

I am not disputing that expenditure matters (that is what aggregate demand is ultimately about), however Keynes is here concerned always with expected proceeds. Thus in proposition (2), D_1 is the amount expected to be spent on consumption. In proposition (3), D_2 is the amount expected to be "devoted to new investment" (NB not simply spent, because of user cost, positive or negative). Furthermore in each case it is the amount actually expected, ie the intersection between aggregate supply and demand, the effective demand, for consumption- and capital-goods respectively, not simply the aggregate demand. This is made clear by the definition of their sum, D, as *effective* demand. Keynes is a good Marshallian, his prices and quantities are *always* equilibrium values.

Thus he can proceed to write in proposition (4), to the consternation of many authors as you know, including Moggridge on p. 385 of CW VII, that "*Since* $D_1 + D_2 = D = \phi(N)$, … it follows …" (emphasis added)

which is taking an equilibrium condition as given, as he goes on to emphasise in proposition (5), "the volume of employment in equilibrium depends on …"

In summary, a close reading of the text provides no evidence that Keynes has switched to considering actual rather than expected expenditure at the end of Chapter 3. He does this later, in Book III, when considering actual consumption out of actual income.

Vicky

You put your finger on the nub of the matter when you state that you do not need the tacit assumption for the determination of output and employment but you do need it, if you are going to call Chapter 3 an equilibrium chapter.

It is a fair criticism that I am more attached than you to competitive equilibrium, whereas you wish to protect the emphasis on uncertainty in production. There is no dispute that production involves uncertainty and the moment one starts to think seriously about user cost, or the liquidity premium on the finance needed for production, it is clear that one could easily lose the (Hicksian) determinacy I see in Keynes. You recognise that the device I attribute to Keynes for dealing with uncertainty in production is the inclusion within 'entrepreneurs' of employers and dealers. I would stress that for me, and probably for him, this is an analytical device required precisely to allow the construction of a (Hicksian) determinate model. In practice, an individual enterpreneur may perform both roles simultaneously, as with the distinctions between the various types of demand for money (GT p. 195), or the earnings of management and the return to capital-assets, without loss of generality.

Furthermore there is the whole area of uncertainty about production processes (efficiency is not fixed and immutable in the real world) but Keynes's acceptance of the assumption of an aggregate supply function shows that he regards this as second-order and perhaps within the realm of stochastic risk. I expect we can agree that this is not the main concern of *The General Theory*.

On Keynes's "wobbles" in Chapter 3, see my above response to Geoff. The disequilibrium in Chapter 5 is only that of short-period equilibrium relative to long-period.

I find the distinction you make between 'determinacy' and 'equilibrium' altogether very difficult, especially as a reading of Keynes. The 'hypothetical demand' approach developed by you, and to which you refer as adopted by Andy Denis, involves the difficulty that effective demand becomes subjective; one point of effective demand is as valid as another so long as entrepreneurs believe it (very post-modern!). For you it is enough that entrepreneurs determine employment in accordance with their expectations, that they make a decision; I think Keynes is claiming more than this.

I thoroughly agree that at the root of this debate is the continuing varieties of usage of the term 'equilibrium'. Indeed part of my objective is to flush this out, once and for all. The mainstream have taken refuge in Walrasian rational expectations equilibrium and thrown the baby out with the bath-water. Pigovian stationary state theory is another form of that approach. A large part of my concern is to differentiate Keynes's use of equilibrium from what he calls 'Classical' (you would call neo-classical) and indeed from the more strictly Classical version that you and Geoff and most Post Keynesians now appear to have adopted. By the latter, I mean the notion of equilibrium as a state of rest, of repetition, of tranquillity. This is, as you quite rightly say, distinct from the notion of optimization.

So I agree that Keynes's equilibrium is not the Walrasian, but the difference is that entrepreneurs alone determine employment, taking into account their expectations of the actions of consumers and investors. So the owners of labour and other factor resources *per se* are indeed powerless, as you say. Yet you are quite right that I still think Keynes's entrepreneurs are definitely optimizers. In that sense, Keynes is neo-classical and not classical (using both these terms in your sense).

The mathematical solution of a simultaneous system is in itself only a representation, the question is, of what? The relevant analogy is with statical mechanics, a balance of forces, which has a definite meaning, in this case as you recognise, a balance between supply and demand. To extend equilibrium to mean a state of rest through time, is to take a large, dangerous and I think unnecessary step, certainly as a reading of Keynes.

You call a state of involuntary unemployment determinate, but not an equilibrium, because entrepreneurial expectations are wrong and they may revise them in future. Ironically, here you are closer to Hicks than I am! In what sense are expectations 'wrong'? Only in the Walrasian sense. We need to dump the concept of equilibrium as optimal allocation of factor resources, without dropping the concept of equilibrium as optimization. In other words, we retain profit maximisation but lose universal utility maximisation (cf GT Chapter 2 Classical Postulates). In the process we reclaim equilibrium as an explanation of observable variables.

I need to read Vercelli (1991) – I have it in my bag now – but it seems to me a mistake to regard Keynes as classical rather than neoclassical (in your sense) and to interpret the GT through the lens of the Treatise. Keynes deals with his own past self explicitly on GT pp. 49-50, 124.

Marshall's biological 'trees in the forest' analogy refers, I think, to (his) long period. Keynes's model is consistent both with short-period profit (i.e. proceeds) maximisation and a state of affairs where firms are making abnormal net profits (or losses). This is partly through the fact that we probably never reach long-period employment (in Keynes's sense) before expectations change, so that the process of competing away these deviations from the 'normal' is never completed. Also I think the 'degree of competition' subsumes the whole issue of entry and exit into industries by particular entrepreneurs and limits the competing away of super-profits. I agree that in this area we have reached the limits of equilibrium models.

In suggesting that the move from micro to macro is insoluble, are you not denying that Keynes achieved this very thing? When Keynes refers to 'the' interest rate, there is an observable set of market interest rates. This is not the same as treating 'the' state of expectation as a complex of unobservable individual expectations.

On polypoly and the preservation of uncertainty, in denying that small firms can take prices, you neglect the real-world existence of short-term forward markets for producible goods, captured by the concept of production to order, dealers, etc. Part of the difficulty may be the difference in our interpretations of Keynes's day, which you equate with the production period and I do not. There is no room in your construction for a distinction between forward prices established today and the actual spot prices realised at the end of the production period. Once again, you are here closer to Hicks than I am!

I do think there is a single consistent model at the analytical core of *The General Theory*, and it is the Chapter 3/5 Principle. In what sense do you regard Keynes as changing models? I do not regard the gradual unfolding, the explanation as endogenous of variables initially held exogenous, as a change of model, as opposed to an extension. The obvious examples are D_2 and the money-wage. Nor do I regard the 'Amadeo switch' from expectation to expenditure as a change in model: it merely bases the expectations of Chapter 3 on the realised results of Chapters VI-VII and Book III. Yet, reading your 1998 (Sharma) paper again, I do not think you mean anything very different.