Original text by Mark Hayes in regular font, rejoinder by Vicky Chick in italics

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, i.e. 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, *Keynes made no mention of dealers/wholesalers intermediating between production and sale and shouldering all the burden of the uncertainty of market sale – both price and quality. This is, as far as I know, a device unique to you. It leaves the producers in a position of certainty. The uncertainty heaped on the middlemen in your treatment goes unremarked – hoping, perhaps, that we will forget. Nor do you say where these dealers appear in Keynes's aggregation scheme – because they don't appear. But where do they appear in yours?Who bears the losses/takes the profit when expectations are falsified?* this is an analytical device required precisely to allow the construction of a (Hicksian) determinate model. In practice, an individual entrepreneur 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. *If so, why did you invent the dealers?*

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*. *Agreed. Never in contention between us.*

On Keynes's "wobbles" in Chapter 3 [*between expected and actual demand*], see my above response to Geoff. The disequilibrium in Chapter 5 is only that of short-period equilibrium relative to long-period. *I don't understand what you mean here. Ch 5 deals with the revision of expectations when the expectations of the recent past have not been met. There is nothing in it about the long period. Or do you mean simply that l.p. expectations are given and s.p. expectations change, so they change relatively to one another? –if so, this is an unnecessarily confusing qualification.*

I find the distinction you make between 'determinacy' and 'equilibrium' altogether very difficult, especially as a reading of Keynes. *Are you denying that values of variables can be determined outside of market clearing? If so, where does that leave involuntary unemployment?* 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; *Effective demand* ***is*** *subjective. For me that is not a problem, just a fact of life. Producers have no way to know what tomorrow's market for their goods will be like; therefore they must make their best guess (form their expectations). they use all the evidence at their disposal (a la the Treatise on Probability) but ultimately they must form a view – and that necessarily involves subjective elements – in Treatise terms both the selection of the evidence and the weights are matters of choice* one point of effective demand is as valid as another so long as entrepreneurs believe it (very post-modern!). *I believe that what you mean by 'valid' is 'true', that is, what the (future) market will actually reveal (rational expectations). You are trying to make objective what can ony be subjective, and to deny the possibility that action can be taken without prior knowledge of 'the truth'. But the whole point is that 'the truth' is not available.* 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. *What more could he be claiming than what he has said, which is precisely that entrepreneurs determine employment in accordance with their expectations.*

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. *I should drop the 'once and for all' ambition. This debate has been going round like a broken record for decades.* The mainstream have taken refuge in Walrasian rational expectations equilibrium and thrown the baby out with the bath-water. *That is an ironic view, considering that your construction is either rational expectations or one-period (and therefore vacuous) equilibrium.* Pigovian stationary state theory is another form of that approach. *Pigovian stationary state theory is, because he* ***started*** *with an assumption of equilibrium and only determined equilibrium values. This point is well explored in my 'what were the children doing: the Keynesian revolution through the eyes of Ambrosi', paper delivered to SCEME workshop, Stirling, 20 Sept. 2008 – a paper to which you heard delivered and to which you have evidently paid close attention. His was Equilibrium Theory (as defined in Chick and Caserta,* Provisional Equilibrium and Macroeconomic Theory, in P. Arestis, G Palma and M C Sawyer, eds, *Markets, Employment and Economic Policy: Essays in Honour of G C Harcourt*, Vol 2, Routledge, 1997, pp 223-237*). That is different from a theory which has an equilibrium but also is determinate in disequilibrium. That is why my notion of repetition over time, though it looks similar to the stationary state, is methodologically very different.* 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. *See comment just above. Keynes's equilibrium is neither the stationary state as conceived by 'the Classics' nor neoclassical market clearing.* 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, *agreed* 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.*I would agree if (and only if) you said optimisers w.r.t. their expectations.* 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, *I'm not ' extending' – this definition has been around for a long time* is to take a large, dangerous *why dangerous? on what criterion? what are you afraid of?* and I think unnecessary *for you, maybe; for me it is fundamental* step, certainly as a reading of Keynes. E*quilibrium in a single period is a vacuous concept; see Vercelli. Either equilibrium endures for some time or it is meaningless – just another word for a mathematical solution to simultaneous equations, and therefore redundant. No other discipline that I know of defines equilibrium in a timeless framework.*

You call a state of involuntary unemployment determinate, but not an equilibrium, because entrepreneurial expectations are *NO – may be* 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. *NO, absolutely not. In your system, where the future is collapsed into the present (a form of rational expectations) producers cannot be wrong – only those poor, ignored dealers. In mine, though producers act on their best guess, that guess may turn out not to be what they expected. It all comes down to true uncertainty.* We need to dump the concept of equilibrium as optimal allocation of factor resources, *I do not invoke that concept* without dropping the concept of equilibrium as optimization. *See comment on optimisation above.* In other words, we retain profit maximisation *as you know, in MAK I defend profit maximisation,* ***w.r.t. expectations*** but lose universal utility maximisation (cf GT Chapter 2 Classical Postulates). In the process we reclaim equilibrium as an explanation of observable variables. *Ah, so here we have it – no variables that are observed are, in your system, the result of a disequilibrium. Equilibrium and determination are the same. Hicks and Lucas would agree with you but I do not. An equilibrium has never been an* ***explanation*** *of anything: it is an* ***outcome*** *of the interaction of postulated forces. Giocoli's distinction between the* *'systems of forces' (SOF) view and the more recent 'system of relations' (SOR) view. might clear this up for you (Giocoli, Modelling the Rational Agent, Edward Elgar 2003) .*

I need to read Vercelli (1991) *and my exchange with Hicks and the subsequent piece in HER, and Giocoli* – 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. *on Probability? or on Money? I use both. see above for TP. And in TM he speaks of wanting a theory which holds good in disequilibrium as well as equilibrium.* Keynes deals with his own past self explicitly on GT pp. 49-50, 124. *I am aware of these passages but I don't see their relevance to our present discussion. Nor am I quite sure what aspect of TM you think I am using as a 'lens'.*

Marshall's biological 'trees in the forest' analogy refers, I think, to (his) long period. *Both Marshall and Keynes used 'long period' in two different ways: the equilibrium which would occur if only a 'shock' occurred, everything else constant, and allowed to work itself fully out (Keynes's 'long-period employment', and the working of the system when a change in the capital stock is allowed to affect the outcome (Marshall) or aggregate supply (Keynes). I though we agreed this in the seminar. For the purpose of this paragraph I assume you mean the second sense.* 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). *Absolutely. Without 'abnormal' profits, Z and what I shall call the demand-for-labour function, aware of Davidson's strictures, would have no length. They would only be points.* This is partly through the fact that we probably never reach long-period employment (in Keynes's sense) *Now I think you mean the first 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. *Even if we reached l.p. employment in K's sense, the capital stock has not been allowed to change – or rather to affect supply -, so there is no way to embody the entry and exit of firms, which is how 'abnormal' profits are eliminated. Normal profits are only a fluke in the s.p. This is a much more fundamental (and far simpler) explanation.*

In suggesting that the move from micro to macro is insoluble, are you not denying that Keynes achieved this very thing? *I said, or meant to say, that there is no perfect fit. Keynes made compromises to achieve some sort of fit. Neoclassics make their own compromise (the representative agent). I think K's compromises are easier to defend, but the is a matter of persuasion.* 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. *But the difference lies in observability, not in the mode of speech used to capture a complex picture.*

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. *And I neglect them (consciously) for a reason, viz., that K. neglected them, in order to stress expectations, rather than 'facts' as the determinants of output and employment.* 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. *If we disagree on the meaning of the 'day' it is because for you it is. I think, a period in which nothing can differ between its beginning and its end, whereas for me it can. How much of that falls to your (rationally expecting) dealers?* 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. *Of course there is. To neglect something does not mean there is no room for it.* Once again, you are here closer to Hicks than I am! *Doubtful.*

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. *To me, a model **is** defined by what is exo/endogenous etc. It is much narrower than a theory. In Ch 3/5, investment is exogenous, money is absent, interest rates of exogenous. All these things change later. Different models, with temporary and partial closures, one theory. On this see*

Chick, On open systems, *Revista de Economia Politica (Brazilian Journal of Political Economy*) 24 (1), January-March 2004, 3-16. ISSN 0101-3157. http://www.rep.org.br/pdf/93-1.pdf and Chick and Dow, Formalism, logic and reality: a Keynesian analysis, *Cambridge Journal of Economics* 25 (6), November 2001, pp. 705-722. ISSN 0309-166X Online at http://ssrn.com/abstract=873650. DOI: 10.1093/cje/25.6.705

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. *I would disagree both with your interpretation of Amadeo and that my Sharma paper is 'not very different' from your interpretation. You cannot 'base' expectations at time t on realised results at some future time, since the latter are unknown. Not even your miraculous dealers can do that.*

It is now clear that, for you, no variable can be determined (= observed) unless it is an equilibrium value. this is why you attribute explanatory force to equilibrium, where, I argue, it doesn't belong, and why you cannot understand (your word – I would choose accept) the difference between determination and equilibrium. But what, then, do you make of the proposition that in 'disequilibrium' in most markets, 'the short side of the market dominates' – that is, is the observed volume of sales? Or to put it another way, in conditions of wartime or planned-economy shortages, are you denying (a) that there was disequilibrium in the sense that more would have been bought if it had been available and (b) that goods were sold and that the data on these sales could be collected?

Vicky