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THE 'MINSKY MOMENT'♣ A CRITIQUE AND A RECONSTRUCTION♦

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♣ In spite of current enthusiasms for the 'Minsky Moment', I remain sceptical. Part of this scepticism – but an important part – is due to the education, instruction and scholarship of my critical friend and colleague, Stefano Zambelli, who – alas – remains irresponsible and does not agree to take the blame for the remaining infelicities.

♦ An earlier, briefer, version of this paper is to be published as: "Credit Crisis Theory *Redux*: A Critique and a (Re)Construction", in: **Economic Crisis in Ireland: Alternative Interpretations**, Edited by Sephen Kinsella and Anthony J. Leddin, Blackhall Press, Dublin, 2010. An expanded, more mathematical, version of this paper is to be published in **The OUP Handbook on Post-Keynesian Economics**, edited by Geoff Harcourt and Neville Norman, Oxford University Press, Oxford, 2011.

Dedicated to the Memory of George Shackle

Post(humous)-Keynesian Par Excellence♦

"The functions of credit have been a subject of as much misunderstanding and as much confusion of ideas as any single topic in Political Economy. ……. Credit has a great, but not, as many people seem to suppose, a magical power: it cannot make something out of nothing."

Mill, 1898, p. 309.

♦ On the occasion of two important anniversaries of the introduction of the innovative concept of *Potential Surprise*, with which he tried to formalise *incomplete* – not *imperfect* - knowledge, without reliance on the worn out concepts of official probability theories, to encapsulate the distinction between risk and uncertainty (see, Shackle (1939, 1949)). That post Keynesians, who have relentlessly emphasised so-called '*Keynesian uncertainty*', '*systemic uncertainty*', and so on, and unreservedly asserted that this cannot be formalised by any notion of probability – even including Keynes's own 'logical' theory espoused in a *Treatise on Probability* (Keynes, 1921) – never consider the possibility (sic) of using Shackle's fertile framework, remains a mystery to me. Not even 'Modern' Behavioural Economists' have found it useful to envision the 'kaleidoscopic world' of George Shackle as providing a lens through which officially non-rational behaviour can be formalised. Somewhere between Shackle's *bounded uncertainty* (Shackle, 1966, pp. 74-5) and Simon's *bounded rationality* (Simon, 1955), there is the rich world of *Algorithmic Behavioural Economics*, spawning *incompleteness*, *unknowability* and *indeterminism*, that could provide decision theoretic foundations for an *empirically based macroeconomic crisis theory*.

§ I. A Preamble

> **"Professor Leontief does not accept [that instability is an unrealistic hypothesis] and maintains that we may utilize *dynamical systems that are unstable throughout and cites capitalism as an example*."**
> **Goodwin**, 1953, p. 68; italics added.

In his regular column in the *FT*, on January 4, 2009, Martin Wolf heralded the New Year acknowledging the relevance of the 'Minsky Moment'. Apparently after taking the US off the Gold Standard, President Nixon is supposed to have paraphrased a variant of Milton Friedman's *Time* magazine enunciation[1] as: '*I am now a Keynesian in economics*'. With friends such as these, who needs enemies, is the old cliché!

The Friedman observation was the beginning of the end for Keynesian economics in its *Neoclassical Synthesis* form and the rise of what eventually came to be *New Classical*[2] economics, although one had to traverse the transition regimes of fix-price macroeconomics and variations of monetarism and New Keynesian economics, before capitulating to the world of Lucasian fantasies. .

If such histories repeat themselves, the spectre of what may come after New Classical economics, when high priests of orthodoxy announce on public pulpits, the age of Minsky and the return of Keynes, boggles and terrorises the mind of those of us who have been outliers to orthodoxy for a quarter of a century. It may well be time, then, as one who has been an old-fashioned Keynesian throughout the New Classical age, to take a critical look at Minsky and the Keynesian foundations of the 'Minsky moment'.

This paper is a first attempt at such a critical study. It is largely non-technical, although I intend supplementing this with a companion piece that substantiates the technical details in some detail.

Charles Kindleberger's immensely readable and justly famous narrative of 'the big ten financial bubbles', spread over the past four centuries, is an interpretation of these crises in

[1] '*In one sense, we are all Keynesians now; in another, nobody is any longer a Keynesian*', **Time**, 31/12, 1965.
[2] I am never sure whether to refer to this school as New Classical, Newclassical, new classical or newclassical; my 'unsureness' may well be reflected in a usage that flips from one form to another, in one and the same essay. Ditto for New Keynesian economics and Post Keynesian Economics!

terms of 'a model developed by Hyman Minsky' (Kindleberger, 2005, p. 21). That this one single model, assuming it is possible to encapsulate Minsky's various variations on a theme within the framework of 'a model', developed on the basis of the experiences of advanced capitalist economies during the latter two thirds of the twentieth century, could be used to make sense of four centuries of episodes of financial *turbulence*[3], is remarkable, and almost beyond belief.

I have always felt Hyman Minsky's singular contribution to crisis theory, in credit economies of the 'capitalist' type, to be in the grand tradition of Rudolf Hilferding's classic *Das Finanzkapital* – but without the Austrian capital theoretic foundations, nor the Marxian monetary theoretic underpinnings of that early 20th century classic. Nor, indeed, are the policy inferences by these two pioneers of capitalist financial crises, and their institutional underpinnings, very similar[4].

Orthodox interpretations of Hyman Minsky's important contributions to crisis theory, not without considerable support from the *master*[5], have placed him squarely as someone who completed the Keynesian vision of capitalism's financial fragility, albeit with a liberal infusion of Fisherian elements. The methodological conundrum here is, of course, the fact that the Keynesian vision of a multi-equilibrium[6], unstable, capitalist economy, does not sit too comfortably with a Fisherian commitment to a uniquely stable equilibrium system, with or without credit. Moreover, Keynes's agents are *behaviourally* rational; Fisher's

[3] Benoit Mandelbrot has suggested that Osborne Reynolds was inspired to use the word '*turbulent*' in his pioneering papers on fluid motion, in the period 1872-1894, from a reading of Macaulay's 'History', where, referring to a report in the London Gazette of February 12, 1684/5, he writes: 'In the City of London, lately so *turbulent*, scarcely a murmur was heard.' (Chapter IV, Pt. I). However, my own study of the Reynolds papers of the period has not succeeded in substantiating Mandelbrot's conjecture.

[4] One other parallel to Minsky's visions of the 'financial fragility' engendered in capitalistic evolutions, if not a greatly similar analysis of credit-based crisis, was the remarkably prescient study by Albert Gailord Hart (1995, [1938]). Indeed, Perry Mehrling, in his *Introduction* to the 1995 r-issue of Hart (ibid) points out that 'the idea for the reissue of *Debts and Recovery*' was 'urged by Minsky himself' (op.cit, pp. xiv-xv). There is a particularly poignant reason to mention this parallel since almost immediately after his *Debts and Recovery* of 1938, Hart began a sustained and serious study of 'Swedish' period and expectational analysis and, indeed, was fully aware of Shackle's work which I am trying to celebrate in this essay (see Hart (1951, [1940]). However, any further detailed exploration of the connection and parallels between Minsky and Hart must wait for a different exercise, mainly due to space constraints.

[5] By the 'master', I am referring to Minsky himself.

[6] As a matter of fact, after considerable instruction from my friend, Stefano Zambelli, I have come to interpret the analytical core of Chapter 17 of the GT as advocating a *Non-equilibriu*m Monetary Production economy, not least due to the Sraffian nature of that chapter.

intertemporally optimizing agents, on the other hand, provide a significant foundation for orthodoxy and its relative insensitivity to crisis theory[7].

Here lies an unresolved – indeed, virgin territory – issue: the *behavioural* foundations for one or another of the Minsky-based models of crises. Much lip service is devoted – not least by the master himself – to '*Keynesian Uncertainty*', '*Fundamental Uncertainty*', '*Systemic* Uncertainty', even 'Knightian uncertainty', and so on. However, to the best of my knowledge, there is not a single Minsky-based model of crises, of a formal, mathematical, kind, based on any formalization of these ostensibly 'pregnant' concepts. That old horse, the tiresome dichotomy between 'risk' and 'uncertainty' is invoked, before the former is dismissed as irrelevant and, thus, any and all reliance on probabilistic mechanisms to formalize intertemporal dynamics is also eschewed. At least that is the official stance, although the praxis is a muddle, even by the *master* in some of his joint, formalized, work.

Minsky-based models are said to encapsulate 'Keynesian', 'pervasive', 'Fundamental' or whatever uncertainty' - or is it 'systemic uncertainty'- with fashionable appeals to the *General Theory* and its much maligned *chapter 12*[8]. The resulting *ad hoc* models are, then, claimed to have the capacity to replicate all kinds of patterns; worse, even to be able to be so general that their one or another specialization – usually for parameter variations – is a realization of every conceivable model, non-orthodox or not.

Minsky also emphasises the instability of a capitalist economy based on advanced credit mechanisms and institutions. The catch phrase, of course, is '*stability is … destabilizing*'[9]. However, other pioneers of emphasising the role of credit in the accumulation and growth processes of an advanced capitalist economy, primarily Schumpeter and Keynes, would have agreed with Leontief's trenchant observation on the pervasive instability of such an economy,

[7] Except in so far as they can be interpreted as *rational* bubbles, of one sort or another!

[8] I shall simply use the phrase 'Keynesian uncertainty', assuming the reader will give it the appropriate context, in the rest of this paper.

[9] 'Stability – or tranquillity – in a world with a cyclical past and capitalist financial institutions is destabilizing.' (Minsky, 1978 [1982]), p. 101. Encapsulating this idea dynamically is quite simple – even using the 'forced' version of Goodwin's pioneering non-linear business cycle model (Goodwin, 1951, particularly using equation (5e), p. 12) – via *homoclinic loops* and their *bifurcations*. Again, to the best of my knowledge, no one has attempted this kind of complete endogenous modelling of a 'Minsky Crisis' to give formal content to the 'catch phrase'. The attempt by the *master* (see Delli Gatti, et.al., 1994), is completely *ad hoc*, as is the Ferri-Minsky (1992) attempt. Indeed, both of these contributions comprehensively misrepresent the nonlinear macrodynamic literature, even of the 1950s, let alone anything that came later.

with or without credit, i.e., with or without 'financial institutions'. Nonlinear and evolutionary endogenous growth and cycle models, both of the Schumpeterian and Keynesian variety, are a testimony to this proposition, although none of these kinds of models have been developed far enough to substantiate the full force of Leontief's interesting suggestion.[10]

Instability, *multiple equilibria*, *multiple credit regimes* and '*pervasive Keynesian uncertainty*' are the methodological quartet that seem to characterize a Minsky model of crises in advanced capitalist economies with well-developed, and constantly evolving, *institutions* and *mechanisms of credit*. So far as I can see the distinctive contribution by Minsky to the vast and noble literature on credit crisis theory, from Mill to Minsky, is the explicit tripartite characterization of behavioural regimes in advanced, credit-based, capitalist economies, in terms of *Hedge*, *Speculative* and *Ponzi*[11] epochs, in addition to exhortations to incorporate 'Keynesian uncertainty', both at the individual and the institutional level. However, neither of these features has ever been defined with any kind of systematic formalizations, in any one integrated mathematical model, even by the faithful.

A brief, highly potted summary of the high points of crisis theory, from Mill to Minsky is discussed in the next section. The purpose is only to place in context the much vaunted 'Minsky moment' and, perhaps, also to demystify much of the present hype. In §3 a characterization of Minsky's modelling desiderata, for understanding crises – perhaps also for taming them - are presented and discussed. I suspect the purpose of §3 is critically to expose the inadequacy of any and all current attempts at encapsulating the rich vein of suggestions in Minsky's many writings, not all of them always displaying accurate doctrine historical traditions; nor even showing much historical knowledge of the exact underpinnings of Minsky's own claims and attributions.

[10] In my own current work, I have been able to construct a dynamical system 'unstable everywhere', to be interpreted as an endogenous model of the business cycle.

[11] I must first have come across 'Ponzi schemes', *in a strictly economic context*, in the early years of my economics education – say in 1973 or 1974 – through a fascinated reading of Galbraith's famous and eminently readable book (Galbraith, 1954). However, *the mechanism of a Ponzi scheme* entered my intellectual conscience and world, growing up in old Ceylon, through a reading of the various exploits and vicissitudes of Margayya, the man with no initials, in R.K. Narayan's, *Malgudi* novel, *The Financial Expert* (Narayan, 2001, [1952], especially, *Part Four*), in the late 1950s.

In §4 I suggest a 'new' vision for classical behavioural economics, incorporating and conjoining Shackle's world of *bounded uncertainty* with Simon's more developed world of *bounded rationality*, as a foundation for modelling crises, whether of a Minsky-type or not.

The final, brief, section outlines how and where one can go, to advance the formal suggestions of §4.

I would like to add two important – at least from my point of view – caveats on two Keynesian elements: '*animal spirits*' and the '*banana paradox*'. The former is almost indelibly linked to any and every interpretation and comment on the celebrated Chapter 12 of the *GT*; the latter, which is essentially, a parable to highlight '*the fallacy of composition*', appeared in the *Treatise* (Keynes, 1930, pp. 176-8).

As for the former, with the notable exception of Robin Matthews (1984, [1991]), very few – if any, and certainly not anyone from the 'Minsky stables' – have tried to link the origins of the use of the phrase 'animal spirits' to Keynes's early, undergraduate, essay on Descartes (cf, Matthews, op. cit, pp. 105-6)[12]. This fact, should be coupled to the two coincidences of: (a) Keynes purchasing Descartes' *Les Passions de l'âme* (translated, unfortunately as, '*animal spirits*', see Harrod, 1951, p. 483), just around the time he was drafting the first versions of Chapter 12; and (b) Richard Kahn's remembrance, reported in Matthews (op. cit, footnote 2, p. 104), that:

> "Chapter 12 was apparently written less carefully and in a more light-hearted spirit than most of the *General Theory*. It was not subjected to the scrutiny of the group of younger colleagues assembled by Keynes to help him … ."

I am not sure the significance attached to Chapter 12, by Minsky and his followers are for all the right reasons; indeed, they may well be for misleading reasons. But in this they are not more culpable than οἱ πολλοί '(*hoi polloi*).

[12] Matthews, acknowledging his indebtedness to Dr Gay Meeks, suggests that Keynes, most plausibly, may have been inspired by Hume, to use this phrase in the sense in which it was meant to be interpreted in Chapter 12. However, my own – admittedly less than exhaustive 'Keynes scholarship' – view is that Keynes first came across the term in Descartes, but had it strengthened in his mind when writing *A Treatise on Probability* (Keynes, 1921), where Hume plays an important role. I believe it is time these connections are studied more deeply and the tangled origins sorted out more clearly.

Secondly, the 'fallacy of composition' is the '*sine qua non*' of macroeconomics. This was what motivated Wicksell's analytical scheme and, then, those of his most tenacious Swedish followers, Lindahl, Myrdal and Lundberg (cf. Lundberg, 1996, p. 31)[13]. More importantly, this is the fundamental analytical fulcrum in Keynes's own transition from the orthodoxy of the *Tract* to the new visions of the *Treatise* and the *GT*. As Joan Robinson has tirelessly emphasised (cf., in particular, Robinson, 1964, p.338), Kalecki, coming from Marx (and, perhaps, Rosa Luxembourg), did not have to 'struggle' to devise a framework to encapsulate 'the fallacy of composition'. Wicksell, his followers, and Keynes, had to 'struggle', but 'struggle' they did, and successfully resolved the problem. The greatest betrayal of every kind of Keynesian has been the abandonment of 'the fallacy of composition'. Minsky and his followers are no exception to this 'betrayal'.

§ II. Brief Notes on Credit Crises in Theory – From Mill to Minsky

> **"Credit is thus *inherently unstable*".**
> **Hawtrey**, 1931, p. 10; italics in original.

The opening lines of Hart's reflections (op.cit, pp. 3-4; italics added), more than half a century after the original publication of *Debts and Recovery*, are an acknowledgement of the explicit anchoring of the analysis in that classic work in the work by Hawtrey on money and Schumpeter on business cycles:

> "The analysis underlying the 1938 edition of *Debts and Recovery* was based on a mixture of the ideas of Ralph Hawtrey and Joseph Schumpeter, who offered the most advanced theories of money and business cycles of the time. For understanding debt problems, the most important idea in Hawtrey's monetary theory concerned the *inherent instability of credit*. Hawtrey taught that any disturbance to the economy tends to be magnified by the credit system. .. Like Hawtrey, Schumpeter viewed credit as a force of instability, but unlike Hawtrey, he welcomed the instability as the mechanism through which the economy constantly transforms itself."

Hawtrey's monetary theory was, in its essentials, an elaboration of Wicksellian monetary theory, but without the refinements bestowed upon it by Lindahl and Myrdal. Schumpeter's business cycle theory was based on a complex process of synchronisation between the

[13] In a personal conversation with Myrdal, in 1981, when I visited him in Stockholm, the great man told me that he tried to explain the '*banana paradox*' to Jacob Viner, during a 'walk along Lake Geneva' – but failed 'utterly and comprehensively to convince Viner of the relevance, importance or even the meaning of the concept.'(cf.also, Myrdal, 1982, p. 167).

Kitchin, Juglar, Kuznets and Kondratieff cycles, and their adaptation to technological change, mediated by entrepreneurs who are allowed to bid resources away from an existing equilibrium state, by the credit system.

Thus, the source of the instability of a credit-based capitalist system was identical, in both Hawtrey and Schumpeter, and both had an equilibrium benchmark. But the mechanism by which the system traversed the equilibrium path, cumulative in a Wicksellian sense in Hawtrey, fluctuating growth in the case of Schumpeter, were entirely devoid of any expectational mechanism, at least in their respective formal theories.

This missing link in the Wicksellian system was provided by Lindahl and Myrdal, in one way, and Keynes, in another way. Somehow, one would have expected Minsky to acknowledge this tradition – in addition to the entrepreneurial, innovational, underpinnings of Schumpeter's unstable equilibrium theory of the business cycle - in the way he developed the rich tapestry of his framework to study 'financial fragility' and the 'stabilizing destability' of credit-based capitalist economies.

Yet, it will not be an exaggeration at all to say that Hyman Minsky's noble progenitors – leaving aside Marx and Hilferding – are Mill (1898, especially Bk. III, Chapter XI), Wicksell (1898), Fisher[14] (1911, especially Chapter 4), Schumpeter (1912,[1934], especially Chapter III), Hawtrey (1931), Currie (1933), Keynes (1936), Lindahl (1929, [1939])) and Myrdal (1931, [1939]). Minsky, however, acknowledges his explicit debt only to Fisher and Keynes, with an occasional nod towards Schumpeter[15]. With hindsight at my disposal, this is not surprising. Minsky has not bothered to anchor his theories in the noble traditions of theories of credit crises. This is a pity and may be one of the reasons for the relative neglect of his work in textbooks and policy circles, till the recent resurgence of interest in the form of 'Minsky moment' incantations.

[14] Although the consensus, amply encouraged by Minsky himself, seems to be that the significant progenitor is Fisher (1933).

[15] Guardedly, but very deliberately, I choose not to refer explicitly to Schumpeter's *magnum opus* on **Business Cycles** (1939) here. This is because the Schumpeterian mechanism(s) – to the best of my knowledge – play no *significant* role in any of Minsky's formal models of the business cycle.

In this brief section I simply want to record the tradition, perhaps to develop it further and deeper at a later date; or, perhaps, provide hints to others who may be better equipped to pursue the necessary scholarship and make the important links and draw the necessary implications.

Even in the classic tradition noted above, there needs to be a sub-classification. There were those, like Mill, Marshall, Schumpeter, Fisher and Hawtrey, who subscribed to a fundamental equilibrium vision of a capitalist economy, even when credit-based, and the disequilibria and instabilities were transitory, even if policy may be required to re-settle the economy on its self-adjusting, self-equilibrating, path.

On the other hand, the Wicksell[16] vision, developed intensively and to great depths by Lindahl and Myrdal, was of a monetary macroeconomy that was inherently unstable and had no endogenous, self-equilibrating, properties. Moreover, the benchmark monetary equilibrium in a Wicksellian economy, at the hands of Lindahl and Myrdal, was not reducible to the real equilibrium of a Walrasian economy (cf. in particular, Myrdal, op.cit, pp. 35-6).

That there is no reference at all to Hawtrey (op.cit), or to any of Laughlin Currie's work in the 1930s, in any of Minsky's writings is more than mysterious. In particular, it seems to be a pity that there is no attempted anchoring and links to Hawtrey's notion of '*credit deadlocks*' nor to Currie's important scheme for so-called '100% money'. The latter scheme, as elegantly argued by Roger Sandilands (2010, p.336):

> "[W]as indeed a political non-starter, but its main logic – the need to gain firm control over bank reserves for effective control of the supply of money, hence the business cycle – was the inspiration for the 1935 Banking Act that was to establish a true central bank for the United States and shift the power base of the Federal Reserve System from New York to Washington."

[16] Despite Patinkin (1952), there is no evidence whatsoever in Wicksell (1898) or, more especially, in Lindahl (op.cit) or Myrdal (op.cit), that the cumulative process was intrinsically stable. Moreover, Wicksell may have been a neoclassical economist in value theory – albeit underpinned by a deep grounding in Austrian capital theory – but he was, despite assertions to the contrary, first mythologized by Ohlin (1936), no quantity theorist. As for Ohlin's understanding of 'Swedish' monetary theory, as it was developed by Wicksell, Davidson and their immediate successors and followers in Sweden – primarily Lindahl, Myrdal, Hammarskjöld and Lundberg – at that time, the interchanges between Hamamrskjold, Lindahl and Lundberg are most revealing. These private letters between the three are deposited in the 'Lindahl archives', with which I worked intensively in 1986. Some of the relevant – caustic - exchanges, pertaining to Ohlin, and the opinions of Hammarskjöld, Lindahl and Lundberg, on Ohlin's mastery of monetary theory, are reported in Velupillai (1988).

Most importantly, of course, from the point of view of the 'Minsky moment', there is his Keynesian allegiance. To this, Minsky has paid tireless attention, although as I shall try to show, the key concept of 'Keynesian uncertainty' never rises above the proverbial 'lip service' in the constructed models – by Minsky or his acolytes. Thus, I have doubts about the repeated invoking of the message of 'Ch.12 of the GT' in Minsky scholarship. To this I must add a further scepticism on the claims that the other Minsky anchoring, on 'Ch. 17 of the GT', which is also questionable and is discussed in the next section.

There is also the question of whether any one complete cycle of 'manias and panics' is unique in its characterising features or whether any one such episode is one of a definable 'genre', as assumed by, for example, Kindleberger. Hence the attempt by the latter to provide a coherent narrative of ten such episodes, spread over about four centuries, allegedly using the one framework of a 'Minsky model'.

Recent orthodox literature seems to have feet in both strands of thought. For example, Tirole (2002), seems to 'believe' in an interpretation financial crises that will allow him to sit comfortably on the fence (*ibid*, p.1):

> "No two crises are identical. At best we can identify a set of features common to most if not all episodes.

But scholars more knowledgeable in the historical literature are more nuanced in their attitude to this issue (cf. for example, White, 1990).

Just for the record, I should also mention the hardcore orthodox approach to an analysis of financial crises as 'bubbles', rational or not, with or without rational expectations. I don't think Minsky was even remotely influenced by this technical literature, nor do I think his technical repertoire was adequate to make sense of this seemingly sophisticated approach. However, I hasten to add that I myself do not think this literature is either mathematically sophisticated or even technically interesting, let alone conceptually or empirically meaningful.

It may, however, be useful to mention that work by, for example, Schiller (1981, [1993]), has inspired some interest in trying to provide *behavioural* economic foundations for macroeconomic financial crises. This is a satisfactory development, at least from my point of

view of trying to fill one important missing link in Minsky's attempt to anchor his vision in 'Keynesian uncertainty', with Shackle's notion of *boundedly uncertain* decisions in the face of '*potential surprises*'.

Even a cursory doctrine-historical point of view, in reading and trying to understand Minsky's kaleidoscopic visions of crises, may suggest that somewhere between Wicksell – as interpreted by Lindahl and Myrdal – Keynes and Shackle/Simon, there lies a fruitful combination and compounding of analytical, conceptual and technical building blocks that may, yet, launch a rigorous, behaviourally and institutionally founded, theory of crises that will do justice to Minsky's attempted theorising.

§ III. Minsky's Precepts for Modelling Endogenous Crises: A Critique

> **"Keynes' *General Theory* viewed the progress of the economy as a cyclical process; his theory allowed for transitory states of moderate unemployment and minor inflations as well as serious inflations and deep depressions. … In a footnote Keynes noted that 'it is in the transition that we actually have our being'. This remark succinctly catches the inherently dynamic characteristics of the economy being studied."**
> **Minsky**, 1978 [1982], p. 97.

I shall assume that Minsky's study and modelling of 'the inherently dynamic characteristics' of a credit-based capitalist economy is one that is always in 'transitory states of being', never 'becoming' stable or unstable, but always tending to the one or the other. Technically, from the point of view of dynamical systems theory[17], this means that the tripartite Minsky-regimes (see below) are always in one or another 'basin of attraction' of a dynamical system, without ever reaching (or ever 'being' at) the system's attractors.

The conceptual underpinnings of Minsky's desiderata for modelling crises in credit-based capitalist economies seem to have been culled out of selected contributions by Irving Fisher, Maynard Keynes, Michael Kalecki (1971) and Dudley Dillard (1955), although there are also some stray Schumpeterian elements dotting the Minsky vistas.

[17] By this I mean differential or difference equations, usually non-linear, the kind of systems that have often been utilised to model the dynamics of Minsky's visions.

Papadimitriou and Randall Wray (2008, p. xii; italics in the original), have provided an admirably succinct encapsulation of the vast canvas that was constructed by Minsky to understand the unstable macroeconomic dynamics of credit-based capitalist economies:

> "Minsky borrowed his 'investment theory of the cycle' from John Maynard Keynes. Minsky's cycle theory derived from combining two things: the famous exposition found in Keynes's Chapter 12 of the *General Theory*, which focuses on the inherent instability of investment decisions as they are made in conditions of fundamental uncertainty, and the approach taken in Chapter 17 to valuation of financial and capital assets. …. While Minsky credited Keynes for pointing the way toward analyzing the process of *financing* investment, he found it necessary to go much further. Thus Minsky's contribution was to add the 'financial theory of investment' to Keynes' 'investment theory of the cycle. … Since financing investment is the most important source of the *instability* found in our economy, it must also be the main topic of analysis if one wants to *stabilize the unstable economy*."

In answering the question 'why does investment fluctuate' (Minsky, 1982, pp.105-6), Minsky postulates his famous 'three types of financial postures': *Hedge finance*, *Speculative finance* and *'Ponzi' finance*. The 'path-dependence' – i.e., history-dependence – of any current state of the economy, in transition, is characterised by the evolving mix of these three types of financial postures.

The transition from one or another of these ideal types to another is when 'Keynesian uncertainty' kicks into action, although it is not clear, in Minsky's voluminous writings – nor in any of those by Minsky scholars – how this is played out by the interaction between individual and systemic reactions. In other words, how an individual's or an institution's decision processes leave the domain or pure risk analysis – and, hence, perhaps in the world of orthodoxy, expected utility maximization (EUM) and the efficient market hypothesis (EMH) – and enter the domain of 'Keynesian uncertainty'. Neither the transition from one pure regime to another, nor the evolution of the dynamics in the Speculative or 'Ponzi' regimes, underpinned by behaviour (of individuals and institutions) based on 'Keynesian uncertainty' has, to the best of my knowledge, ever been formalized.

Now, the economic reason for the transition 'from an initial financial tautness', say in the Hedge finance regime, is that financial flows signal a tightness in the intertemporal flows of the income generating process. This signal of a tautness 'is transformed into a financial crisis' and the transition to the next regime is initiated. At this point Minsky's interpretation of the Kaleckian macroeconomic pricing process plays its crucial role.

But long before Kalecki, Wicksell's immediate Swedish followers – particularly Lindahl (19390 and Myrdal (1939) – had devised a similar scheme, under the much-maligned forces of 'Keynesian uncertainty', to generate unstable, disequilibrium monetary economic trajectories. More importantly, it was this development that inspired George Shackle's pioneering work on non-probabilistic decision theory in the face of incompleteness of knowledge, a situation far more coherent and amenable to precise formalization with the tools of modern, non-orthodox, mathematical analysis. I shall return to these issues in the next section.

Finally, to the tripartite financial regimes and the Kalecki-type pricing rule, was added the methodological precept of 'stability … is destabilizing', in every transition regime. It is understood that every economy is always in a transition regime, and every transition regime is a mix of the pure regimes, even when the 'Ponzi' financial regime rules.

Five critical caveats need to be mentioned, at least cursorily, at this point. Firstly, there is the question of nonlinear dynamics in Minsky's work and in the attempts by many of his followers and admirers to model 'Minsky crises' nonlinearly. Secondly, there is the question of policy for 'stabilizing an unstable economy'. Thirdly, there is the thorny issue of 'equilibrium'. Fourth, there is the crucial question of the correct domain and range for the economic variables in any version of Minsky-type models. Finally, Minsky's understanding of 'orthodox' theories, whether macroeconomics or microeconomics, at their frontiers.

There is no evidence whatsoever, at least to this writer, that Minsky ever understood the mathematics of the nonlinear macrodynamic models that emerged from what is generally acknowledged to be the pioneering works of Kaldor (1940), Hicks (1950) and Goodwin (1951). At a most banal level, there is the repeated reference to the 'ceiling-floor' models of Hicks and Goodwin and the absurd claim that the Hicksian trade cycle model is 'linear'. There are no *exogenous* 'ceiling' and 'floors' in any of Goodwin's many nonlinear macrodynamic models. Hicks has two regimes, one with entirely endogenously determined, unstable equilibrium; and in the other, also an unstable equilibrium, only one of the exogenous constraints is, in fact, active; the second one, usually the 'ceiling' is endogenous. All the way from Minsky (1957, 1959) to Minsky (1965), Ferri-Minsky (op.cit) and Delli

Gatti, et.al., (1994), there is a series of misrepresentations of the structure, mathematics and economics of the pioneering nonlinear macrodynamic models[18].

Thus he – and his followers – were, unfortunately, unable to realize that the identical endogenous mechanisms generating the unstable, disequilibrium, nonlinear dynamics could have been harnessed to model, endogenously and nonlinearly, a complete Minsky model of a three-regime crisis, with the Kaleckian pricing rule and transition regimes that encapsulate the idea of 'stability … is destabilizing'.

Where such models remain inadequate is where every formal attempt – again, to the best of my knowledge – to model *Minsky Crises* as formal (*ad hoc*, nonlinear) dynamical systems: has failed: to endogenise 'Keynesian uncertainty'. Not even the admirably concise, nonlinear, attempt by Taylor and O'Connell (1985) or its more pedagogical and clearer version in Taylor (2004, Chapter 9, §7, pp. 298-305), escape the *ad hockery* of enlightened curve shifting.

Secondly, on policy for 'stabilizing an unstable economy', there was the noble 'Swedish tradition', emanating from Wicksell, but most comprehensively developed by Lindahl and Myrdal. Apart from a curiously unerudite, passing footnote, in Ferri-Minsky (op.cit)[19], there is no evidence at all that Minsky took the trouble to familiarise himself with the classic framework of an unstable credit economy that Wicksell developed, and Lindahl and Myrdal completed in the form of a dynamic, disequilibrium, macroeconomy with an unstable monetary equilibrium that is in no way related to the real equilibrium of orthodox theory.

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[18] There is the preposterous assertion, in Minsky (1965), p. 258, that:

> 'Various ceiling models of cycles or cyclical growth have appeared. In all except one, Kurihara's model, the rate of growth of the ceiling is exogenous.'

So far as I can see, this is just a blind paraphrasing of the incorrect claim – incorrect as to technical accuracy – in Kurihara (1960), p.8 and footnote 5 on the same page. Had they understood the difference between an autonomous planar nonlinear differential equation and its forced version, it would have been impossible for Kurihara, and, hence, Minsky to make such absurd claims. It is a pity – at least for someone like me, who is fundamentally in sympathy with a Minskyan vision of credit-based capitalist economic dynamics.

[19] 'Sweden, which had a particularly sophisticated group of economists in the 1930s and a knowledgeable political leadership in their Social Democratic Party, may have knowingly introduced the welfare state.', ibid, footnote 23, p. 89. Surely, one would have expected a sustained advocate of active policy to 'stabilize an unstable (monetary) economy' to be more scholarly in studying the one actual example of theory and policy meshing admirably in the precise sense of Minsky? There is ample literature, even by the Swedes themselves, of this rich interaction (see, Myrdal, 1982, Lundberg, 1996, and the many references therein).

Thirdly, there is the issue of *equilibrium*. Minsky's economies are in their transition configurations, within the 'basin of attraction' of some attractor, whether stable or not does not matter. Thus, when approached from the point of view of global, endogenous, capitalist dynamics, a Minsky model must naturally encapsulate multiple equilibria. Are the destabilizing financial forces generated during the transition to a stable equilibrium – i.e., the genesis of a pure Speculative regime is an endogenously evolving dynamic process during the time the economy is in the basin of attraction of the Hedge regime? This is formally impossible within the framework of dynamical systems theory, without a plethora of unattractive *ad hockeries*[20]. Why not simply give up on 'equilibrium'? My conjecture is that Minsky's reading of Chapter 17 of the *GT* was heavily indebted to Dillard's interesting, but incomplete, interpretation. Minsky, therefore, was not able to discern the Sraffian point in that important chapter: that *every configuration of the economy is some equilibrium*, making the notion vacuous (Keynes, 1936, especially p. 242)[21]. If every configuration of the economy is equilibrium[22], there are no transition paths; nor is there any sense in the distinction between stable and unstable equilibria!

I now come to an issue that may have the air of an exotic 'objection': the relevance of real variables and real analysis in formalising the dynamics implicit, say, in a balance-sheet constructed for an abstract Minsky-type economy, say as in Table 9.3 in Taylor (2004, p. 299). The numbers that enter such balance-sheets can, at best, be rational values (both positive and negative). But the dynamical system that is supposed to reflect the evolution of

[20] The most imaginative metaphor I can think of, for this situation, is the second of the twelve labours of *Hercules*, the one against the *Lernaen Hydra*. It will not do to simply cut off head after head, when Hydra sprouts two new heads for each one cut off. Hercules had to devise an innovative strategy, of the kind that Lindahl and Myrdal devised, disciplined by the theory of economic policy, to maintain an inherently unstable monetary economy in place.

[21] Minsky's indebtedness to Dillard's reading of Chapter 17 of the GT is most clearly expressed in Minsky (1985, especially pp. 7-8). No reading of Chapter 17 of the GT can be complete without placing it in the context of Sraffa's masterly critique of Hayek, where the concept of the 'own rate of interest' was first developed (Sraffa, 1932). It is this notion that formed the fulcrum around which the whole of the argument of Chapter 17 was formed. No wonder, then, that distinguished Keynes scholars, from Dillard and Lerner (for example, Lerner (1952)), to Patinkin and Leijonhufvud, have not made much sense of this important chapter. None of these Keynes-scholars have ever taken the time and trouble to understand Austrian capital theory and its deep critique by Sraffa (1931), and, therefore, missed the essential *monetary* point in Chapter 17. I am eternally grateful to Stefano Zambelli for drilling this crucial point into my obdurate mind.

[22] It was in the famous footnote in Chapter 17 of the GT, which Minsky lays stress on as the one where Keynes stressed the importance of transition regimes, that Keynes made the reference to Hume as the progenitor of the equilibrium concept in economics (p. 343, footnote 3; italics added):

> "[H]ume began the practice amongst economists of stressing the importance of *the equilibrium position* as compared with the ever-shifting transition towards it, though he was still enough of a mercantilist not to overlook the fact *that it is in the transition that we actually have our being*: …"

the economy represented in the balance-sheet – say, as depicted in Figure 9.8 *(ibid*, p.302) 'resides' in the unrestricted two-dimensional Euclidean space. Any facile response that the answer to this conundrum is to work with difference equations, or a discrete dynamical system, misses the point. Of course, this is an objection to all 'unrestricted' dynamical system modelling in economics. I'll return to this theme in the next section.

Finally, Minsky's understanding of the frontiers of orthodoxy, whether it be macroeconomics or microeconomics, had entered a time-warp at a point around the time when the neo-classical synthesis, fix-price macroeconomics, Friedmanite monetarism, and all kinds of revived New Keynesian bastardization of old Keynes had all been properly buried and their deaths officially proclaimed by the New Classicals. Minsky was fighting old wars and lost battles and banging against frail and irrelevant walls, invoking Hahn's pathetic irrelevancies against claims by any and every kind of macroeconomist who was trying to found the subject on general equilibrium theory.

Indeed, the new orthodoxies that were defining the new frontiers could even claim to encapsulate two of the Minsky finance regimes[23] – *Hedge finance* and *Speculative finance* - quite comfortably, so long as neither Minsky nor his acolytes provided a formal mechanism in which to frame 'Keynesian uncertainty', seemed to have escaped the attention of many of the 'Minsky moment' enthusiasts.

Unless and until a proper formalization of 'Keynesian uncertainty', at the level of the individual and the institution is not forthcoming from the Minsky corners, orthodoxy will not take too seriously the other three pillars of the Minsky edifice: multiple equilibria, Kalecki-type pricing and 'stability … is destabilizing'.

§ IV *Bounded Uncertainty* Meets *Bounded Rationality* – Towards a *Behavioural* Rejuvenation of Crisis Theory

> **"[The] solution of the problem of reconciling uncertainty and imaginative experience .. I have .. called the focus-hypothesis solution. . Creative decision, … if real, .. is**

[23] Orthodox theory, *even in its non-monetary growth versions*, banishes '*Ponzi schemes*' by decree, at the very basic level of the rational agent's intertemporal budget constraint (see, Romer, (2006), pp. 53-54, especially equations (2.6) & (2.10).

> **performed in the face of uncertainty.. . It is only *bounded* uncertainty that will permit him to act creatively. . [and] Probability must be abandoned in favour of *possibility* … ."**
> **Shackle**, 1966, pp. 82-90; italics in the original.

Shackle introduced his concept of *Potential Surprise* first in his seminal EJ paper of 1939 and made his intentions clear at the very outset (Shackle, op.cit, p. 443; italics added):

> "My concept of 'potential surprise' is *something very different from that of mathematical probability for which I wish to substitute it*. It is purely subjective."

I have long conjectured that the origins of Shackle's concept of *potential surprise* came about as a result of his deep and enthusiastic studies of Myrdal's *Monetary Equilibrium*, of which he wrote, in the 2nd edition of Shackle (1968; pp. xv, xvii):

> "[*Monetary Equilibrium* is] perhaps the most undervalued work of economic theory ever written. …To me having no German, Myrdal's ideas became known only through a lecture course given by Mr Brinley Thomas in 1935 [at the London School of Economics]. … I emerged [from the lectures] with only an inkling of what Myrdal had said, but the idea of *ex ante* and *ex post*, of the vital role of expectation, had struck fire in my thoughts."

It was, however, only after a recent serendipitous encounter with Shackle (1958)[24] that I was able to substantiate my conjecture in a reasonable way. Shackle (1958), refers to Myrdal's discussion of Investment gains and investment losses', Myrdal (1939, p.61-2; bold italics added), where the latter states:

> "For gains and losses in *revenues and costs must actually occur at some time*, and since they contain an element of ***surprise***, in so far as they have not been anticipated with full certainty, gains and losses arise in the *ex post* calculations regardless of how short the periods into which the process is divided."

My purpose here is to try to make the case, however thin the reed on which I hang my conjecture, that Shackle was motivated to devise a scheme of rational decisions under *incomplete knowledge* without relying on the probability calculus.

[24] I am, once again, indebted to my good friend Stefano Zambelli for facilitating this particular serendipity. He had Georgescu-Roegen's copy of the book in which Shackle (1958) was published and gave it to me when I was searching in my own library for my 'Shackle books', when I was preparing my lectures on **Behavioural Economics**, last autumn. I was, at first, attracted simply by the title of Shackle's article, thinking it might have something to do with the notion of liquidity in 'The Two Triads Lectures' by Hicks (1967), although the dates did not mesh. But, no! Shackle's direct inspiration was Myrdal (1939), where I finally found the 'missing' connection with Shackle's *potential surprise* and Myrdal's notion of *surprise* in 'investment gains and losses'!

Bounded uncertainty (Shackle, 1966, pp. 74-5), underpinning *creative decisions* by a rational agent facing *incomplete* knowledge, gives rise to *potential surprise*. I think this one sentence characterises the most important aspect of Shackle's theory of rational decision in the face of incomplete knowledge, not just *imperfect* knowledge, which will turn out to be a special case of the former.

Four remarks and observations are in order, before proceeding to the coupling of boundedly uncertain agents with Simon's boundedly rational ones.

First of all, Shackle (1966, p. 75), distinguishes between *listable* and *non-listable* 'distinct sequences of events' that are 'ordered' by a boundedly uncertain (rational) agent. I interpret this distinction to be equivalent to that between a *recursive* and *recursively enumerable* sequence, as in computability theory. Thus data – real and conjectured – form recursively enumerable sequences that are not recursive. This is sufficient to induce potential surprise in a formal sense.

Secondly, the definition of bounded uncertainty is preceded by noting that, Shackle (1966, p. 74):

> "[A] world where there are constraints upon the ways in which events can follow each other, yet where even a complete and perfect knowledge of these constraints would leave us ignorant of 'what will happen next';…"

Bounded uncertainty, then, is precisely this situation that a rational agent who has to make a decision, faces.

Thirdly, decisions in the face of bounded uncertainty, in Shackle's world are exactly as in Simon's world of boundedly rational agents (and institutions): they are *decision problems* in the precise sense of *metamathematics* (cf, Velupillai, 2010, Chapters 11 & 12).

Finally, Shackle is remarkably perceptive in making it clear that the sequence space and the decision space are defined on a lattice, by 'saw tooth like' functions – i.e., the domain and range of economic variables are rational valued.. However, his lack of mathematical expertise in dealing with such a space forces him to work with real variables and classical analysis (ibid, p. 91):

> "We can treat each of G ['face value; or pure desiredness-undesiredness] and y [the possibility] as a stepping-stone variable consisting of discrete values, so that these variables together form a lattice whose points will be expectation elements as defined in the foregoing."

I shall not need to fudge the issue by having to deal with illegitimate real analysis and shall retain the original assumption of discrete (in fact, rational) values for all relevant variables (again, as in Simon's world of boundedly rational agents).

The next step is to embed Shackle's agents within the behavioural, boundedly rational, satisficing agent solving decision problems. In Velupillai (op.cit), I have given a complete characterization of the dynamics implicit in Simon's boundedly rational, satisfying, agent solving decision problems. In it I have also shown the equivalence between such an agent and a formal dynamical system capable of *computation universality*. Moreover, an equivalence between such a dynamical system and a Turing Machine is also formally demonstrated. Invoking these results I can say that – formally - an institution can be represented by a system of coupled Turing Machines and an economy a system of coupled institutions. They can be represented on a (to be sure a vast) lattice, as Shackle would have desired.

This is, formally again, no different from a vast table reflecting the balance-sheet of an economy – except that the dynamics is not implicit, but (more than) explicit: it is possible to implement the lattice as a dynamical system and observe its evolution.

What was missing in my earlier Simonian constructions were the crucial elements considered by Shackle: the *boundedly uncertain, creative decision maker* facing *incomplete* knowledge, giving rise to *potential surprise*. However, Simon, when considering *Models of Discovery* (Simon, 1977), formalised as boundedly rational agents solving problems – i.e., as *Human Problem Solvers* (cf. Newell and Simon, 1972) - encapsulated the equivalent of Shackle-type considerations of both 'bounded uncertainty' and 'potential surprise' in his scheme. This is most clearly explicated in Kulkarni & Simon (1988, [1989]), especially pp. 366-367). In Velupillai (op.cit, Appendix 2 to Part IV), I have formalised Simon's Models of Discovery, with boudedly rational agents, also as (coupled) Turing Machines.

Therefore, it is easy – again, formally speaking – to incorporate the Shackle elements within the mechanism that formalises the boundedly uncertain, rational agent, satisficing in the solving of decision problems in which potential surprise is a possible element to which the computation universal dynamical system – or the Turing Machine in its computation path - must react, and proceed on its dynamic evolution.

Finally, it is easy to show that halting machines, for example finite automata, are those that correspond to agents, or a system of agents forming an institution or a macroeconomy, depict a Minskyan economy in the Hedge finance regime. Next, the pure Speculative finance regime will be equivalent to finite automata or Turing Machines facing recursively enumerable sets of sequences of economic data to process that are also recursive. Finally, the case of an agent or a system of coupled agents in a mixed Hedge-Speculative finance regime would have to solve decision problems satisfactorily by processing sequences of economic data that are recursively enumerable but not recursive.

Concluding Reflections

> **"We do not know why a great speculative orgy occurred in 1928 and 1929. *The long accepted explanation that credit was easy and so people were impelled to borrow money to buy common stocks on margin is obviously nonsense*. On numerous occasions before and since credit has been easy, and there has been no speculation whatever. "**
> **Galbraith**, 1954, p. 169; italics added.

What has been suggested, in the constructive part of this paper, which means, essentially, the previous section, is that the agents who populate an economy, or the institutions in which they 'reside' or even the whole macroeconomy, be viewed algorithmically. This was a natural implication of the Shackle-Simon vision I tried to suggest in the previous section, hopefully providing the missing link of 'Keynesian uncertainty' for Minsky-type modelling of credit-based capitalist dynamics. In the implied formalization of the Shackle-Simon agent, institution or economy, there is the possibility of interpreting their algorithmic implementation as a dynamical system capable of what is called computation universality.

However, the natural algorithmic domain and range for the variables, parameters and constants are the constructive or computable numbers. This fact meshes comfortably with the other important fact that economic variables, parameters and constants are, at best, algebraic numbers, although in practice they are simply rational numbers. This means the dynamical

system equivalent of a Turing Machine has to be a discrete dynamical system acting on rational numbers (or the natural numbers).

Even if such is possible – i.e., constructing a discrete dynamical system acting on rational numbers – the further requirement, for the kind of crisis theory Minsky seems to have had in mind, such a dynamical system must be capable of encapsulating three additional properties:

i. The dynamical system should possess a relatively simple global attractor;
ii. It should be capable meaningfully and measurably long – and extremely long – transients;
iii. It should possess not just ordinary sensitivity dependence on initial conditions (SDIC) that characterise 'complex' dynamical systems that generate strange attractors. It should, in fact, possess Super Sensitive Dependence on Initial Conditions (SSDIC). This means that the dynamical system *appears* to possess the property that distances between neighbouring trajectories diverge too fast to be encapsulated by even partial recursive functions.

Is it possible to construct such rational valued dynamical systems or, equivalently, algorithms that imply such dynamical systems?

The answer, mercifully, is yes. In Velupillai (2010a), I have discussed how, for a Clower-Howitt 'Monetary Economy' (cf. Clower-Howitt, 1978), with rational valued, say-tooth like monetary variables, it is possible to use the '*Takagi function*' to model its dynamics, while preserving its algorithmic nature. But in this case, it is necessary to work with computable – or recursive – analysis. It would be more desirable to remain within classical algorithmic formalizations and, hence, working with rational- or integer-valued dynamical systems that have a clear algorithmic underpinning.

It is a pleasure to end this paper of many speculations (sic!) with a positive conjecture: I believe Goodstein's algorithm (cf, Goodstein, 1944) could be the paradigmatic example for modelling rational - or integer - valued dynamics (Paris-Tavakol, 1993) of a credit-based capitalist economy.

Even more satisfactorily, if we are to take the Minsky vision of the dynamics of credit-based capitalist economic dynamics seriously, and try to solve its policy dilemmas, then it seems to

me that the best analogy – as pointed out above, in footnote 20 – is the policy maker as poor *Hercules* and the *Hedge-Speculative-Ponzi* being as *Hydra*[25]. In other words, every time Hercules slays one of the heads of the Hydra, two more sprout from where the source of the slain one! Is this to be a *Sisyphean task* for the poor policy maker – or can she emulate Hercules and find the equivalent of *Iolaus* to conquer, once and for all, the seemingly eternal repetition of 'manias and panics' in credit-based capitalist economic dynamics?

Formally at least – and actually, of course, in Greek mythology – there is a solution to the problem of *Hercules vs. Hydra*, meaning by this there may well be a policy resolution to the eternal dilemma of recurrent manias and panics (Kirby-Paris, 1982).

But I shall end with a more down-to-earth tone, invoking the ever-wise and sobering reflections of John Kenneth Galbraith, from his masterly study of *The Great Crash – 1929* (ibid, p. 169; italics added):

> "[T]he collapse in the stock market in the autumn of 1929 was implicit in the speculation that went before. The only question concerning that speculation was how long it would last. Sometime, sooner or later, *confidence* in the short-run reality of increasing common stock values would weaken. When this happened, *some people* would sell, and *this would destroy the reality of increasing values*. Holding for an increase would now become meaningless; the new reality would be falling prices. There would be a rush, pell-mell, to unload. This was the way past speculative orgies had ended. It was the way the end came in 1929. It is the way speculation will end in the future."

Amen!

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[25] It must, of course, be remembered that the Hydra had 'only' *one immortal head*. Somewhere in the recesses of the core of the true characteristics of a credit-based capitalist economy there must be 'an immortal head'. Many prophets, not least Marx and Schumpeter, have in the past been confident they had discovered it, only to be proved false prophets within a generation, or two. Minsky is only one in this long list of great prophets.

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