Algorithmic Social Sciences Research Unit

ASSRU

Department of Economics
University of Trento
Via Inama 5
381 22 Trento Italy

DISCUSSION PAPER SERIES

11 – 07

POSTKEYNESIAN[♣] PRECEPTS FOR NONLINEAR, ENDOGENOUS, NONSTOCHASTIC BUSINESS CYCLE THEORIES[♥]

K. Vela Velupillai

MARCH 2011

[♣] I shall use the unhyphenated term Postkeynesian to refer to the tradition of endogenous macroeconomic, non-orthodox, dynamics that emerged in the 1930s. I believe it is common practice in macroeconomic theory, to refer to Newclassical, Neoclassical, etc., theories of business cycles, growth, and so on. I cannot see any reason for a Postkeynesian underpinning of a theory of macroeconomic dynamics to be referred to in any different way. On the other hand, it is also common practice to use terms -- unhyphenated and hyphenated -- like New Keynesian ([55],[56]), Non-Monetary, Supply-side ([71]) in current discussions of varieties of macroeconomic theories. I prefer the unhyphenated terms for reasons of aesthetics. However, I shall, occasionally, also use post Keynesian, when referring to secondary references. Prepared for the **Handbook of Post Keynesian Economics**, edited by Geoff Harcout & Peter Kriesler, Oxford University Press, Oxford, 2011/2012.

[♥] This paper is dedicated to the memory of one of the most original and courageous Post Keynesian economist I have known, **Wynne Godley**. I cannot think of anyone more naturally and intrinsically encapsulating the honest and fierce spirit of Maynard Keynes at his intellectual best than Wynne Godley. I first met Wynne Godley at a wonderful lunch at Mario Nuti's home, in Cambridge, in 1977. Subsequently, I have had the pleasure and privilege of knowing him, professionally and personally, for over thirty years -- hosting him in Denmark, Italy, Ireland, and even in Cambridge. The high point of my tenure as a Fellow of Girton College, Cambridge, was when I had the opportunity to invite Wynne Godley to give the annual lecture at the College's Joan Robinson Society, in Spring, 2007. His last book, co-authored with Marc Lavoie ([13]), is, in my opinion, one of the best, most pedagogical, books on Monetary Macrodynamics in the grand tradition of Wicksell, Lindahl, Keynes and Myrdal. I hope the precepts I am trying to fashion in this paper are, in some way, a reflection of some of what I have learned from Wynne Godley.

1 By Way of a Preamble[1]

> "All these pretty, polite techniques, made for a well-panelled Board Room and a nicely regulated market, are liable to collapse. ....
>
> Perhaps the reader feels that this general, philosophical disquisition on the behaviour of mankind is somewhat remote from the economic theory under discussion. But I think not. .... I accuse the classical economic theory of being itself one of these pretty, polite techniques which tries to deal with the present by abstracting from the fact that we know very little about the future."
>
> John Maynard Keynes ([39], p. 215)

A Dynamical system capable of computation universality is subject to the *Halting Problem for Turing Machines.* Hence no future steady state – even if formally provable to 'exist' – is calculable, even given all past and present information *exactly.* Unknowability, undecidability and uncomputability of steady states, transients or any other dynamic trajectory are the dominant epistemic, epistemological and methodological issues of what may be called the algorithmic universe that seems to dominate many kinds of mathematical formalisms trying to encapsulate theories of the social sciences and humanities in the form of implementable models. The age of formal mathematics, proving the existence of non-constructible, non-computable, undecidable entities in economics, may, in the fullness of time, come to be seen as having occupied an insignificant, sorry, period in the grand development of economic theory that was initiated by the classical economists, nobly preserved and enhanced by the development of macroeconomics, and revived and rejuvenated – after the unfortunate interregnum of the neoclassical synthesis, its replacement by varieties of monetarisms, the brief interlude of neo keynesianism and the current, although fading, dominance of newclassical economics – by post Keynesian economics[2].

My main aim in this essay is to extract possible analytical precepts, from the continuing evolution of Postkeynesian economic theory, to suggest the way I think the rich Keynesian tradition – mostly, though not exclusively, of the

[1] On the whole, I shall not enter into the continuing debates on who or what characterises Postkeynesian economics, except for one issue: the place that should be occupied by the Neo Wicksellians, by whom I mean the second generation Swedish Wicksellians, Lindahl, Myrdal, Hammarskjöld and Lundberg (but *not* Ohlin). I leave these debates – perhaps they ought to be referred to as 'controversies' in the warm glow left behind by [31] and [23] – to the specialist scholars. Suffice it to acknowledge that I have been pleasurably enlightened by [22], [40], [**?**], [48] and some of the ensuing reviews (particularly [10] and [86]). I would be dishonest if I did not also confess that I was both mystified and angered – from a purely intellectual point of view – by the preposterous review of King (op.cit) by Davidson ([4]).

[2] As always, Hicks with characteristic prescience, acknowledged the obvious ([29], p.13):

> "Marshall and Pigou are called 'neo-classics'; but they were anti-classics. It is the post-Keynesian who would better be called neo-classics; for it is they who, to their honour, have wrought a Classical Revival."

multiplier-accelerator variety – of nonlinear, endogenous, non-stochastic theories of the business cycle, should be 'fertilised', so that the current dominance of the linear, exogenous, stochastic-shock theory[3], i.e., *Real Business Cycle* (henceforth, referred to as *RBC*) theory, could be challenged and supplanted, eventually. My stance here follows the idea of a 'constructive engagement with mainstream economics' suggested in persuasive ways by Fontana in several of his writings (op.cit, p. 414 & [11]). In another sense, I aim to suggest this 'constructive' - although critical – 'engagement with mainstream economics' in analogy with the way Postkeynesian economics has contributed to enriching mainstream growth theory[4].

A 'constructive engagement' with the protagonists of *RBC*, who are often dismissive of any approach to business cycle theory that is not intrinsically mathematical *in their particular sense*, is not easy, unless any alternative is also explicitly mathematical in its formulation. The leading exponent of *RBC*, Prescott, has gone even as far as enunciating that ([72], p.2; second set of italics added)[5]:

> "[T]he meaning of the word *macroeconomics* has changed to refer to *the tools being used* rather than to the study of business cycle fluctuations."

The implicit 'tenor and tone' of this essay is, therefore, also mathematical, but hopefully without too many compromises with 'responsibility to the canons of scholarship' ([82], p.605).

With these aims in mind the essay is organised as follows. In the next section I attempt to extract precepts from the rich traditions of the many strands of post Keynesian economics for the modelling of a Postkeynesian theory of aggregate fluctuations. In section 3 I try to summarise the way I think the classics of nonlinear, non-stochastic, endogenous theories of the business cycle – incorporating, naturally, also growth – satisfy many of the precepts extracted in section 2. In section 4, a summary of Minsky's approach to modelling 'crisis' is outlined, mostly critically. As far as I am concerned, Minsky is a distinguished contributor to the richness of the post Keynesian vision of economic crisis. The concluding section is a brief summary of the broad lessons to be gleaned from the approach taken in trying to understand how to make the precepts of post

[3] Or, the theory of *ad-hoc shokeries*, Richard Day's felicitous phrase to describe the modelling methodology of the Newclassicals in general and the Real Business Cycle theorists, in particular (cf. [5], p. 180).

[4] See, for example, Harcourt's excellent discussion in chapter 7 of [24].

[5] Obviously, Prescott did not pay attention to Stigler's ([82], p.605; italics added) admonishment of Samuelson's methodology in the **Foundations of Economic Analysis** ([73]):

> "Some of the infinities of mathematical possibilities are discussed, but only in the most formal terms; there is no instance of the enlargement of our knowledge of economic processes in our society. Samuelson may reply that he is only providing tools, but who can know what tools we need unless he knows the material on which they will be used?"

Keynesian economics become intrinsic to a mathematical modelling of economic dynamics with intrinsic undecidabilities, incompleteness and uncomputabilities.

There is one non-standard stance I take in this essay, following the tradition I leanred from my own sometimes-Postkeynesian maestro, Richard Goodwin: there is no incongruency or inconsistency in harnessing Schumpeter for Postkeynesian purposes!

2 Postkeynesian Precepts: Beyond 'only another box of tricks'

> "What we have tried to show is that within the various strands that we have discerned and described, there *are* coherent frameworks and approaches to be found, though obviously there remain within each unfinished business and unresolved puzzles. The real difficulty arises when attempts are made to synthesize the strands in order to see whether a coherent whole emerges. Our own view is that *this is a misplaced exercise*, that to attempt to do so is mainly to search for ... 'only another box of tricks' to replace the 'complete theory' of mainstream economics which all strands reject. *The important perspective to take away is*, we believe, that *there is no uniform way of tracking all issues in economics* and that the various strands in post Keynesian economics differ from one another, not least because they are concerned with different issues and often different levels of abstraction of analysis."
>
> [22], pp.24-5; first italic in the original.

'The various strands in post Keynesian economics' not only 'differ from one another ... because they are concerned with different issues and often different levels of abstraction of analysis', but also because the analytical, conceptual and, above all, methodological and epistemic foundations are wholly underpinned by what that arch non-Keynesian, Schumpeter, would have referred to as a *vision* of dynamic economic development, whether of advanced, emerging or developing economies. I believe, and agree wholeheartedly with Hamouda and Harcourt (and, thereby, happily also with Joan Robinson), that there is no - cannot ever be, and has never been - a '*complete theory*', or, more pertinently, a *unified theory* based on acknowledged *closures*[6], of every aspect of dynamic economic

[6] The official *neoclassical closure* - which is what unifies newclassical, New Keynesian and even core aspects of Austrian economics - are the triptych of *preferences*, *endowments* and *technology,* on which the maximization–equilibrium edifice stands. Variations on the triptych, like the much-hyped notion of information as a supplement to them, adds no further insight to the sterile basis, nor its infertile superstructure. I have spent most of the last quarter of a century showing, with 'officially' sanctioned mathematical rigour, that neither the neoclassical closure, nor the edifice that stands on it, are capable of handling anything remotely interesting from any formally interesting dynamic point of view (cf., for example: [92] & [93]). By 'formally interesting dynamic point of view', I mean computation, formal dynamical systems theory (particularly nonlinear dynamics), rationality, reproducibility, evolution, and so on

development, as is explicitly and implicitly argued, or at least tacitly accepted, by every kind of orthodox economics.

In the recent comprehensive, almost exhaustive (if such is conceivable), *Elgar Companion to Post Keynesian Economics* ([41]), the Editor's opening characterisation of the subject is most instructive for anyone attempting to suggest precepts for constructing a Postkeynesian Theory of the Business Cycle, especially on the groundings of Cambridge or Minskyan Theories of Aggregate Economic Fluctuations (p. xiv):

> "Stripped down to the bare essentials, Post Keynesian economics rests on the principle of effective demand: in capitalist economies, output and employment are normally constrained by aggregate demand, not by individual supply behaviour. ..... Moreover, there exists no automatic or even minimally reliable mechanism that will eliminate excess capacity and involuntary unemployment."

At least from the point of view of these 'bare essentials', both Cambridge Theories of the Business Cycle and (any) Mynskian Crisis Theory are, surely, squarely Postkeynesian in their conceptual underpinnings and analytical frameworks. In the above admirably concise characterisation, King emphasises, implicitly and explicitly and correctly in my view, *seven* essential Postkeynesian precepts for any dynamic theory of *aggregate* fluctuations[7]: *aggregate demand*, *the fallacy of composition*, *involuntary unemployment*, the (endemic nature of the persistence of) *excess capacity*, *instability*, the absence of any conceivable *self-adjusting mechanism* towards (any kind of, mythical or not, unique or not) *equilibrium* and thereby, *disequilibria* and the existence of *multiple equilibria*.

In addition to these seven Postkeynesian precepts for a macroeconomic (aggregate) theory of fluctuations, I would like to suggest that the following are also among the characterising features of modelling aggregate fluctuations by one or another strand of post Keynesian economics: *non-maximum dynamics*,

- issues that lie at the heart of ostensibly fractured strands of Postkeynesian economics. I take this opportunity to acknowledge Lance Taylor's priority in using the notion of 'closure', although I did not know, till a few months ago, that he had been using it since at least 1979 (in joint work with Frank Lysy). I began using the word in the above sense from around 1983.

[7] King does not suggest that these are Postkeynesian precepts for an *aggregate* – i.e., *macroeconomic* – theory of fluctuations, i.e., business cycle theory. This is my extraction from King's perceptive summary. Regrettably, however, King's own various remarks, thoughts and comments on business cycle theory in [40] including the entry on business cycles in [41] leave much to be desired. For example, in the latter (p. 39), we are informed that:

> "Michał Kalecki (along with Ragnar Frisch and Eugene Slutsky) was a pioneer of the external shock approach."

In the former we are assured, referring to Kalecki's classic, [35], p. 38, that,

> "The mathematical foundations of Kalecki's model were ... assessed by Ragnar Frisch and Jan Tinbergen ... . Frisch confirmed the integrity of the analysis [in [35]]."

Unfortuantely in the latter case, and fortunately in the former case, these claims are not even remotely correct.

'*time-to-build*' production and implementation, *non-ergodicty*, *complexity* and *systemic uncertainty* of the dynamical system encapsulating a Postkeynesian theory of the business cycle. One or another of the post Keynesian strands referring to the latter five concepts do not invoke them, or encapsulate them, in any theory of the business cycle, whether Postkeynesian or not. Moreover, even the 'senior' post Keynesian or Keynesian authors (and some of their 'derivative' followers) who pioneered their consideration (in particular non-ergodicity[8] and complexity[9]), refer to and invoke them, for criticising mainstream economics,

[8] I have in mind here, primarily, [3], among the 'senior' authors and, for example, [8], among the 'derivative', younger followers. Paul Davidson's paper is littered with a plethora of technical infelicities and, even in 1983, it was known that there were mean ergodic theorems for many realistic classes of non-stationary stochastic processes. Moreover, Davidson's paper has some serious conceptual and philosophical infelicities, even worse than his technical mistakes. His typos border on the hilarious (LaPlace, Wald instead of Wold, etc.). The silliest assertion in Davidson's paper has to do with a serious 'accusation' against Keynes: "Unfortunately, in his day, Keynes did not have access to the meticulous work of the Moscow School of Probability which developed in exacting detail the now standard theory of stochastic processes. In retrospect, therefore, we can only seek to reinterpret Keynes' fine intuition of the distinction between uncertain and probable events in terms of such processes." (p. 188). This is utter and complete nonsense, for many reasons. First of all, the 'Moscow School of Probability' underpinned their theory of stochastic processes on precisely the kind of theories of probability that Keynes rejected. Secondly, it is simply not true that 'we can *only* seek to reinterpret Keynes' fine intuition.. in terms of such processes'. (italics added). Neither Davidson, nor indeed Pasinetti, seem to be aware of the massive developments in algorithmic probability theory, at the hands of Kolmogorov, Solomonoff and Chaitin and that Solomonoff's starting point was the Keynesian theory of probability. Dunn's entry on Non-ergodicity in [41] is equally replete with technical and conceptual infelicities. For example, what is one to make of the thoroughly muddled and technically senseless following claim ([41], p.281):

> "However, as some stationary stochastic processes are non-ergodic, that is, limit cycles, non-stationarity is not a necessary condition for the existence for [sic!] non-ergodic processes. But all non-stationary processes are non-ergodic. Non-stationarity is thus a sufficient condition for non-ergodicity and provides an empirical foundation for Post Keynesian claims about the relevance of history and uncertainty."

What is the status of, for example, of the following limit cycle (expressed in polar coordinates): $\dot{r} = r\left(r^2 - 1\right)$; $\dot{\theta}$ at that value of r which makes it *unstable*?

[9] Pasinetti, in referring to the 'Walrasian behavioural model' ([70], p.229) claims:

> "It could very simply be rejected even on the basis of the well-known principle of the possibility of emerging characteristics in the analysis of any complex system."

This sentence is simply false. Firstly, it is not true that so-called 'emerging characteristics' – frankly, I am not sure what the author actually means by 'emerging characteristics', but I assume, provisionally, he is referring to what has become fashionable in much of the Santa Fe inspired agent based economic literature – can be generated by 'any complex system'. Secondly, there is no rigorous definition of 'emerging characteristic' – i.e., 'emergent phenomena' – such that it is possible to impute the phenomenon to the laws of dynamics under which 'any complex system' generate such phenomena. I can generate any number of 'complex systems' which are provably incapable of generating so-called 'emergent phenomena'; conversely, I can show how to generate so-called 'emergent phenomena' by extremely simple systems. The evolution of the concept of 'emergents', first at the hands of George Henry Lewes, inspired by John Stuart Mill and, then, intensively developed by the *British Emergentists* ([58]), to resurface via von Neumann and Ulam, in the modern versions is itself a complex phenomenon that deserves more serious thought than such flippant allusions.

do so on the basis of thoroughly faulty mathematical underpinnings.

Now, by *non-maximum dynamics* I mean what was first referred to by Paul Samuelson in his *Nobel Memorial Prize Lecture* as follows:

> "I must not be too imperialistic in making claims for the applicability of maximum principles in theoretical economics. There are plenty of areas in which they simply do not apply. Take for example my early paper dealing with the interaction of the accelerator and the multiplier. This is an important topic in macroeconomic analysis. .....
>
> My point in bringing up the accelerator-multiplier here is that it provides a typical example of *a dynamic system that can in no useful sense be related to maximum problem*.
>
> [74], pp. 12-13; italics added.

In including 'time-to-build' as one of the Postkeynesian precepts for (mathematical) modelling of aggregate fluctuations I am only making explicit what is, ostensibly, acknowledged as one of the key building blocks of orthodoxy's core assumptions in developing real business cycle theories, and which was fundamental in Kalecki's classic of 1935, and thereafter remained central to his evolving versions of that classic; it was, as well, a key assumption in the canonical nonlinear equation that summarised the *Cambridge Theories of the Business Cycle*. In the latter two cases the resulting, final form, equation for aggregate fluctuations were linear[10] and nonlinear deterministic difference-differential equations.

As for '*systemic uncertainty*', which I consider to be of crucial conceptual importance in the construction of any aggregate, Postkeynesian, theory of the business cycle, I shall not follow convention and refer to chapter 12 of the *GT* and the tiresome cliche of an underpinning in the (in)famous 'animal spirits' for substantiation[11]. With the notable exception of Robin Matthews ([57]), very few appear to have tried to link the origins of the use of the phrase 'animal

[10] Kalecki, alas, always linearised, even when it was highly dubious to do so from any economic point of view. I have discussed, in some detail, the 'time-to-build' tradition in business cycle theory, in a recent essay (cf. [94]). This is a tradition that goes back to Volume II of *Das Kapital* and comes down through Bohm-Bawerk, to the 'modern' era of mathematical modelling of business cycles, initiated by Tinbergen in 1931 ([85]). Bohm-Bawerk's assumption, and the Austrian and, later, the Neo-Austrian traditions, as well, were not linked explicitly to aggregate fluctuations. Some purists may be able to refer to the Neo-Austrian notion of *traverse* as a manifestation of growth cycles. Thoughts along such lines are discussed in [94].

[11] Nor do I wish to refer to [43] and his much 'maligned' use of the distinction between risk and uncertainty. Most scholars are, of course, aware that both [37] and [43] were published the same year (1921). But few post Keynesian scholars are aware that between Lindahl's first lectures on monetary macroeconomics in 1921 (published in 1924 as [50]) and his subsequent pioneering Neo Wicksellian contributions (beginning with [51]) to what came to be called the economics of the Stockholm School, there was Myrdal's doctoral dissertation ([66]), deeply influenced by both [37] and [43]. It was this that was instrumental in the way that Wicksell's immediate Swedish macroeconomic followers, Lindahl, Myrdal, Hammarskjöld and Lundberg, incorporated expectations and anticipations – now fashionably referred to as non-probabilistic uncertainty – into their monetary macroeconomics, and thereby, via Brinley Thomas's LSE lectures, influenced the pioneering contributions of George Shackle. But this is a stroy that is

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 [27], 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; italics added), that:

> "Chapter 12 was apparently written less carefully and in a more light-hearted *spirit* [sic!] 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 of the *GT*, by many post Keynesians, are for all the right reasons; indeed, they may well be for misleading reasons. But in this they are not more culpable than *hoi polloi*.

Instead, to encapsulate the notion of *systemic uncertainty* in a Postkeynesian theory of aggregate fluctuations, I shall keep in mind, but not develop the formalism here, the *bounded rationality/bounded uncertainty* nexus, introduced by Herbert Simon ([79]) and George Shackle ([78]), respectively, within the framework of *decision problems* in the precise sense of metamathematics (cf., chapters 10 & 11) [92]), which leads to the characteristically simple, yet deep, observation by the latter (*ibid*, 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';. . . "

This idea is precisely formalisable in terms of the famous theorem of the *Halting Problem for Turing Machines,* and if the dynamical system modelling a Postkeynesian theory of aggregate fluctuations can be shown to be equivalent to a Turing Machine, then systemic uncertainty in the sense of Shackle above will be exhibited by that system. My strong conjecture is that the nonlinear dynamics of Postkeynesian endogenous, nonstochastic, models of aggregate fluctuations can be shown to be capable of computation universality and, thus, formally equivalent to the computing behaviour of a Turing Machine. The full development of this conjecture must await a different exercise.

---

not central to the main theme considered in this paper and must be left for another occasion. To complete this potted, capsule, history, there are those, like Frydman and Goldberg, [12], who would also include Hayek to the Keynes-Knight nexus, at least on this issue of the risk-uncertainty divide.

[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* ([37]), 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.

In summary, then, I have tried to identify the following twelve Postkeynesian precepts, *some combination of which* should form the basis for a Postkeynesian theory of aggregate fluctuations:

(1). aggregate demand; (2). the fallacy of composition; (3). involuntary unemployment; (4). the persistence of excess capacity; (5). functional distribution (of income and wealth); (6).instability; (7). absence of *self-adjusting mechanisms* (i.e., intrinsic or natural negative feedback mechanisms) towards unique (or one or another of a multiple) equilibrium; (8). disequilibrium; (9). *non-maximum* dynamics; (10). 'time-to-build'[13]; (11). non-ergodicity; (12). systemic uncertainty; (13). complexity; (14) historical time.

Surely, (3) and (4) should be subsumed into one precept[14]. It can be shown that non-maximum dynamics (8), instability (6), disequilibria (7) and multiple equilibria (7) form one unified quadruple. Finally, any serious, rigorous, formal dynamics must consider non-ergodicity (10), complexity (13) and historical time (14) together in a nonlinear framework, if it is to be seriously Postkeynesian in theorising about aggregate fluctuation endogenously and nonstochastically[15]. This leaves seven Postkeynesian precepts that a mathematical theory of aggregate fluctuations should be constrained by, in its construction. The immediate question would be: how many of these are satisfied by mainstream economics? The answer is: exactly one: 'time-to-build' - but this is encapsulated within the standard production function apparatus, which is subject to the strictures of one respectable strand of post Keynesian economics, within a theory constrained by uniquely stable equilibrium configurations generated by ergodic, non-complex, maximum dynamical systems, without involuntary unemployment or excess capacity, generally insensitive to the fallacy of composition. Furthermore, in the era of a methodology dominated by *RBC* (cf., [72]), *calibration* of the mythical aggregate production function, particularly in its much-maligned Cobb-Douglas

[13] My friend, sometime colleague and former mentor, Mario Nuti, when he looked at [94], where I emphasised the crucial role of 'time-to-build' in Kalecki's theories of the business cycle, wrote back as follows (e-mail, 21 January 2011):

> "I could only browse, it would take ages for me to get through it and do it full justice, but thanks for keeping me posted.
>
> The "gestation period" always had a great importance both in Kalecki's theory of capitalist macroeconomics and for his work on investment planning under socialism. All due to his own experience as the son of a manufacturer who went bankrupt, I am not sure in what cycle."

[14] Involuntary unemployment is a concept defined at the individual level and, therefore, mainstream economics has concentrated on 'debunking' it. This is because the notion of a decision variable which is not underpinned by a 'voluntary' act makes it impossible to implement it within an optimization framework, driven by 'Olympian rationality' ([80], p.12). For the reason that it is a concept defined at the individual level, I subsume it within the general notion of the persistence of excess capacity, as in standard Cambridge theories of the business cycle.

[15] For example, in the canonical equation encapsulating Cambridge theories of aggregate fluctuations in nonlinear, endogenous, nonstochastic modes, this means, at the minimum, that the 'initial conditions' must play a significant part in the determination of the dynamics – whether of the short-run or long-rum variety, whether leading to one or another kind of attractor or remains unclassifiable and in transition.

version, functional income distribution[16], (5), is effectively ignored.

And neither money – at least via l*iquidity preference*[17] - nor market structure[18] have even been mentioned!

It is in this sense that I think Hamouda & Harcourt were absolutely on the mark with their wise injunction to refrain from trying to find a *uniform way of tracking all issues in economics so that a complete theory to replace orthodoxy can be constructed*. The search for 'complete theories', like the doomed pursuit for a 'unified theory of knowledge', and other such paranoidal obsessions has been the bane of intellectual integrity for too long. The mainstream misadventures with 'complete theories' are themselves 'complete' red herrings: it is possible to construct thoroughly trivial complete theories without any correspondence with the elements of ontology or epistemology. Any attempt at constructing a post Keynesian 'complete' theory, satisfying all of the above seven precepts (plus liquidity preference) should be resisted in the wise sense in which it is gently discouraged by Hamouda and Harcourt

Yet, the main positive contribution of this paper is the claim that Cambridge theories of the business cycle, suitably modified and interpreted, encapsulate all of the above core Postkeynesian precepts, but the incorporation of systemic uncertainty in the sense of Shackle, above, requires the dynamical system encapsulating the theory of fluctuations to be interpreted in terms of the (computing) behaviour of a Turing Machine. This interpretation is achieved via the demonstration of an equivalence between the computing behaviour of a (Universal) Turing Machine and a nonlinear dynamical system capable of what I have come to call computation universality[19].

[16] This is one important issue that dominates at least one strand of post Keynesian economics with links to the noble tradition of classical economics.

[17] I have quite deliberately refrained from introducing liquidity preference as a precept in the above context for a very special reason: money, especially in the form of finance or (bank) credit, enters the Cambridge theory of aggregate fluctuations via balance sheet, national accounting and social accounting rules, that are themselves dynamic. This is particularly clear in Minsky's work, as made especially explicit in Lance Taylor's elegant formalisations ([83] & [84]).

[18] Both Kalecki and Harrod ([25], [26]) emphasise imperfect market structures in their theories of aggregate fluctuations; the former in pricing, especially. However, it is not clear to me that imperfect market structures are among the fundamental precepts for a Postkeynesian theory of aggregate fluctuations.

[19] It may be apposite and necessary to point out that I am not referring at all to so called 'deterministic chaos' in the construction of such an equivalence.

3 Cambridge Theories of the Business Cycle[20]

> "Once progress is admitted on the ground floor of a theory the awkward question arises about the historical validity of the system. The relevance and usefulness of economic theory to economic history has been small. If anything, business cycle theory has done better than some other branches. The situation becomes more serious the moment we restrict ourselves to *a theory simple enough to be written down in a few equations*. To imagine any connection between such a model and economic history seems grotesque, and yet, if there is no relation, there seems little use in constructing it."
>
> Richard Goodwin ([18], pp.207-8).

The canonical 'few equations' of Cambridge theories of aggregate fluctuations may have been underpinned by one or another 'theory simple enough' to be encapsulated by them; but the 'few equations', (indeed the one canonical equation[21]), have defied complete analysis for the more than one hundred years during which they have been studied, analytically, experimentally, computationally and geometrically, by a galaxy of pure mathematicians, applied mathematicians, physicists, numerical analysts, computer scientists and metamathematicians. Surely, the enigma that is the canonical equation mirrors the riddle that it tries to encapsulate: capitalist economic development - a fact which was known to those who tried to fashion, less and more successfully, Cambridge theories of aggregate fluctuations.

3.1 Background

> "Keynes' *General Theory* was exclusively concerned with a monetary economy in which changing beliefs about the future influence the quantity of employment. Yet **money** plays no more than a perfunctory role in the Cambridge theories of **growth**, **capital**, and **distribution** developed after Keynes."
>
> [44]; bold emphasis, added.

[20] I consider [45] a repository of some of the true classics of post Keynesian economics, contrary to the obviously uninformed opinion expressed in [40], p.9. Thus, for example, [7], [30] and [46] have come to play important parts in the development of various strands of post Keynesian economics, both explicitly and implicitly. Contrariwise, I don't consider [1] of any relevance to either post Keynesian economics or as making any contribution to a Postkeynesian theory of endogenous, nonlinear, nonstochastic theory of aggregate fluctuations. Both books have 'Post-Keynesian' in their titles. Incidentally, Davidson's reference to an example in Blatt ([3], footnote 1, p.186, [1], pp. 204-216) is simply wrong. Blatt is not computing anything for a *limit cycle*, but for a '*centre*' type dynamics, which is, by the way, *structurally unstable*.

[21] I am, of course, referring to the forced Rayleigh-van der Pol equation (in the form given it in Goodwin's defining classic of Postkeynesian business cycle theory, the *Nonlinear Accelerator and the Persistence of the Business Cycle* [17], p.12):

$$\epsilon\theta\ddot{y} + [\epsilon + (1-a)\theta]\,\dot{y} - \varphi(\dot{y}) + (1-a)\,y = \Theta(t) \tag{1}$$

Does, then, *money* play any role in possible *Cambridge Theories of the Business Cycle*[22] 'developed after Keynes'? Indeed, are there *Cambridge Theories of the Business Cycle* in the same sense in which there are Cambridge theories of growth, capital and distribution? One of my aims, not necessarily the main aim, in this essay is to substantiate the claim that there were, in fact, clear, identifiable, *Cambridge Theories of the Business Cycle*, incorporating *growth*, (functional) income *distribution* and *money*.

However, although Kregel's perceptive observation was made a quarter of a century ago, I believe there is a central core of truth in it that may appear to be valid, even for *Cambridge Theories of the Business Cycle*, at least if one relies on the more comprehensive expository characterisations of the development of Postkeynesian economics since 1936[23]. In the ensuing quarter of a century, since Kregel's claim, despite the sometimes stuttering incorperation of Minsky's work on the interaction between financial system and *aggregate fluctuations*[24], theories of the business cycle – with and without money, whether of Cambridge origin or not – seem to occupy, at best, a shadowy existence in the characterisation of Postkeynesian economics, itself being enacted, thus far, almost as a Japanese *Noh* drama.

The acknowledged classics of the nonlinear, endogenous, non-stochastic theory of business cycles, in the mathematical mode, are [16][25], [17], [18] [19], [32] and [28]. Obviously, even if [35] predates *The General Theory* ([38], henceforth, *GT*) and Lundberg's remarkable '*Studies*' ([53]) appeared almost simultane-

[22] The older, more vintage, term used for this phenomenon was trade cycle theory – although in the classics by Goodwin and Kalecki the more modern phrase was used. I shall adhere to the 'more modern phrase' in this essay, purely for simplicity, but also with the hope of engaging some variants of orthodoxy in my case for taking seriously some, at least, of the Postkeynesian analytical precepts for modelling business cycles.

[23] In other words, since the publication of [38], to which I shall henceforth refer simply as *GT*. However, my own understanding of Postkeynesian economics is heavily and deeply informed and influenced by the pre-Keynesian writings of Lindahl ([51], [52]), Myrdal ([66], [67]), Hammarskjöld ([21] and Lundberg ([53]), all of whom emphasised the monetary basis of aggregate economic fluctuations. In this sense, then, I do not think of Postkeynesian economics as a vision that is confined strictly to the post-GT period. I cannot imagine any Postkeynesian – perhaps woth the possible exception of Paul Davidson ([4]), particularly those interested in business cycle theories – *not including* Kalecki's pioneering writings that *preceded* the GT (for example [35]) in any sensible characterisation of Postkeynesian economics. Although [53] was published in 1937, I subscribe to Schumpeter's view ([77], pp. 1173-4) on this classic,

> "But no work of this range and depth can, within a single year, be *formed* by an outside influence unless its author has arrived at somewhat similar conclusions by himself."

[24] See below, section 4.

[25] In this remarkable review of Hicks' classic, *A Contribution to the Theory of the Trade Cycle*, Goodwin unshackled the endogenous, nonlinear, non-stochastic theory from relying on the accelerator as the economy approaches the 'floor'. This meant the construction of a planar dynamical system generating a stable limit cycle, that was, for the first time, independent of the Rayleigh-van der Pol '*cubic characteristic*'. This construction also provided the impetus for the *uniqueness* proof of the existence of a limit cycle, the latter routinely proved by an appeal to the Poincaré-Bendixson theorem (see [89] & [90] for complete details and references). In the standard 'story' of nonlinear, nonstochastic, endogeous theories of the business cycle, the pioneering role of [16] & [18] are, to the best of my knowledge, never acknowledged.

ously with the *GT*, they, too, should be – and often, especially Kalecki, are considered to be – part of the set of 'acknowledged classics' in this *genre*. Of these latter two, Lundberg's classic, in what the Neo Wicksellians referred to as the *sequence analysis tradition* was squarely in the nonlinear, endogenous, non-stochastic tradition of business cycle theories, despite its fame in the standard business cycle literature in the linear form bestowed it by Metzler ([59]). However, *the Kalecki classic*, in spite of its intrinsic nonlinear structure, was *linearised* and studied as a *mixed linear difference-differential equation* in the standard literature, and even by the *master* himself[26].

I would like to add two personal remarks and one additional point to 'substantiate' the contents of the previous two paragraph. When I first attended Goodwin's lectures on Economic Dynamics, at Cambridge University in the Michaelmas term of 1973, he wrote up, on the blackboard, just three references: Schumpeter ([75]), Kalecki (op.cit) and Lundberg (op.cit). Secondly, when Goodwin reminisced about the development of mathematical theories of the business cycle, in the post-depression 1930s, at the Conference in honour of Björn Thalberg[27] in the following way:

> "The Great Depression of the 1930s appropriately gave rise to the first[28] precise, quantitative cycle models. First came a highly original piece by Michael Kalecki, but it was succeeded by the more famous and successful Hansen-Samuelson multiplier-accelerator model, to be followed by the related Lundberg-Metzler inventory cycle."
> [20], p.87.

The 'additional point' I wish to emphasise is the following: right from the outset, Goodwin's development of nonlinear, endogenous, non-stochastic theory of fluctuations was inspired by Keynes of the *GT* and Schumpeter's *theory of innovations* ([75]). This was reflected in every pioneering aggregate nonlinear, endogenous, non-stochastic, dynamic model developed by Goodwin, all the way from 1946 till his classic paper in the **Dobb Festschrift** ([19]). Ironically, much of the interpretative literature has tended to claim that the first series of contributions by Goodwin, (i.e., up to, but not including [19]), all of them representable by some variation of the canonical equation (1), concentrated on a theory of aggregate fluctuations in which either growth was an exogenous trend component or, worse, entirely absent. This is simply untrue. The trilogy that represented the core contributions by Goodwin to a Cambridge theory of aggregate fluctuations in a nonlinear, nonstochastic, endogenous model were produced in the intensive consecutive years of 1950 ([16]), 1951 ([17]) and 1952

[26] This aspect is highlighted in my essay in the *Harcourt Festschrift* ([87]).

[27] Who was my first teacher of economics and, indeed, who introduced and initiated me into the weird and wonderful world of nonlinear, endogenous, nonstochastic theories of the business cycle of the Kaldor-Goodwin-Hicks variety before I came under the magical spell of Cambridge theories of aggregate fluctuations at the feet of Goodwin himself. All this happened during an intensive, unforgettable, three-year period, 1971-1973.

[28] He had forgotten that his own references to the 'first precise, quantitative cycle model', in lectures, was to Tinbergen's famous *Ein Schiffbauzycklus* ([85]).

([18]), the latter first presented in 1952. All of them were models of 'growth cycles' in a clear macrodynamic sense, with the cyclical part built on Keynesian elements of aggregate demand, excess capacity, instability, disequilibrium, multiple equilibria, non-ergodic/complex historical time and encapsulating the fallacy of composition; the growth part owed its construction and incorporation in these classic and pioneering models on the basis of Schumpeter's theory of innovations. This is no where better characterised than in [18][29] (pp.204-6):

> "In order to fuse growth and cycle unalterably we may make the following two assumptions: (a) economic progress is not steady but comes in spurts, these spurts occurring primarily in booms; (b) the cycle is not a case of over– and under–shooting of a stationary level, but rather it is dominated by – and possibly would not exist without – economic growth. The source of these two assumptions is Schumpeter and, in my opinion, it is in his work that we shall find the most fruitful ideas for the problem of trend and cycle. .... Schumpeter's theory, as he often complained, is difficult to formulate in simple mathematical terms[30]. ... Schumpeter's original, pure theory can be put simply: 'The recurring period of prosperity of the cyclical movement are the form progress takes in capitalist[31] society'.[32] ...He thus fused into an organic whole the concepts of

[29] Remembering that it was first presented at the celebrated *Oxford IEA Conference on The Business-Cycle in the Post-War World*, in September, 1952, (where Kaldor was also present and presented his contribution to the same part of the book in which Goodwin's paper was published). The relevance of the observation within parenthesis will become clear shortly.

[30] Compare this measured reflection, on the 'difficulty' of formulating Schumpeter's theory of innovation 'in simple mathematical terms', to Kaldor's lofty dictum ([33], p. 53; italics added):

> "*[I]t is not possible* to make the [Schumpeter] story as a whole into a 'model' (meaning by a model the sum total of assumptions which are just sufficient – no more no less - together to provide the necessary and sufficient conditions for the generation of a recurrent cycle with a clear periodicity) without incorporating into it elements which would suffice by themselves to explain the cycle – without recourse to Schumpeter's own stage army of initiators and imitators, or even the very concept of techincal progress."

Two comments are in order: one, how does Kaldor know that 'it is *not possible* to make the [Schumpeter] story as a whole into a model'? Is this an 'impossibility theorem', within some mathematical formalism of theories and models? Secondly, it is precisely the construction of a 'model' to encapsulate the 'Schumpeter story' that was attempted and achieved in [14] – but, of course, not 'with a clear periodicity, which was not a criterion in the 'Schumpeter story'.

[31] Observe that Schumpeter uses the word 'capitalistic', not 'capitalist.

[32] The original statement by Schumpeter was followed by a characteristically honest caveat ([76], p.295; second set of italics added):

> "*The recurring periods of prosperity of the cyclical movement are the form progress takes in capitalistic society.* ...By saying this we mean to state a fact requiring both proof and explanation. Whilst we hope to be able to contribute, ..., something towards the latter, *it is impossible here* to satisfy the reader as to the former."

Perhaps it was this last italicised phrase that prompted Kaldor to make his characteristically growth and cycle, and implied that the one could not exist without the other. .... It has always seemed to me that the theory of effective demand and liquidity preference could be used to bring greatly enhanced usefulness to Schumpeter's theory, but it is plain that he would have none of it."

Thus, as far as my own interpretation of Cambridge theories of aggregate fluctuations is concerned, as a strand in the post Keynesian economics of growth cycles[33], the basis in the classics – Marx, in particular – and in Schumpeter have to be acknowledged and I am happy, privileged and proud to do so and consider myself a member of that lineage.

It is unfortunate that Kaldor[34], maverick post Keynesian though he was (at least in my opinion), made thoroughly unwarranted and mathematically unsubstantiable assertions about Cambridge theories of growth cycles[35], [33], p.54:

> "[T]he development of trade-cycle theories that followed Keynes' *General Theory* has proved to be positively inimical to the idea that cycle and dynamic growth are inherently connected analytically – to the idea that is that the cycle is a mere by-product of, and could not occur in the absence of, 'progress'. For it has been repeatedly (and in my view, conclusively[36]) shown that a few simple additions to Keynes' own model of a general equilibrium of production in the economy will produce the result that this 'equilibrium' will take the form, not of a simple steady rate of production in time, but a rhythmical movement of constant amplitude and period – in other words, a perpetual oscillation around a stationary equilibrium position."

Almost all the analytical assertions in this Kaldorian observation are incorrect. The damage this kind of technically groundless claims do, especially when invoked uncritically by natural post Keynesians like King ([40]), to any attempt by non-mathematical post Keynesians to develop a Postkeynesian theory of growth cycles is immeasurable, and appeals only to mathematically able

rash 'impossibility' assertion - without the careful Schumpeterian caveat: '*here*'!

[33] I shall, thus, from now on refer to *Cambridge theories of aggregate* **growth cycles**.

[34] I should state very explicitly that Kaldor was my first Cambridge PhD supervisor, and this 'privilege' of being one of his last formal doctoral students was entirely due to Geoff Harcourt's felicitous – at least as far as Geoff's intentions were concerned – intervention. Personally, the (mercifully) brief period I was Kaldor's formal pupil came to an end with the welcome return to power of the Labour government in February, 1974 and I was able to sit at the feet of Goodwin for the rest *his* life, in Cambridge and Siena.

[35] Claims and assertions unfortunately approvingly referred to by King in his very readable, but doctrine historically multiply flawed, particularly from the point of view of Postkeynesian theories of growth cycles, book ([40], pp. 63-4). Indeed, King is completely 'off base' in his thoroughly unscholarly remarks on Kaldor and his early allegiance to Austrian capital theory. Kaldor came to acknowledge his errors against Frank Knight's acute criticisms of Austrian capital theory only over twenty years after te famous Econometrica debates of 1937, in fact at the celebrated Corfu Conference on Capital Theory ([34], p. 294).

[36] At this point Kaldor adds a footnote and refers to the classics of Kalecki, Goodwin and Hicks, and to his own pioneering 1940 article ([32]) on Cambridge theories of growth cycles.

economists, often unsympathetic to Keynesian thought and enthusiastic about the *ad hoc shockeries* approach to business cycle modelling.

3.2 Encapsulating the Postkeynesian Precepts in Nonlinear, Endogenous, Nonstochastic Business Cycle Theories

> "[The] purpose [of *an Essay on the Importance of Being Nonlinear* ([95])] is to convince the reasonable skeptic that much of constitutes our body of *theoretical* knowledge in natural philosophy is based on linear mathematical concepts and to suggest how the more encompassing ideas of nonlinear mathematics would be better suited to the understanding of existing data sets."
>
> Bruce West ([95], p.3; italics in the original.

There are at least two kinds of Cambridge theories of nonlinear, endogenous, nonstochastic growth cycle theories, each satisfying one set of Postkeynesian precepts, but neither encapsulating all of them (in complete agreement with the caution by Hanouda and Harcourt, to which I referred in the opening lines of the previous section).

3.2.1 The Canonical Nonlinear Difference-Differential Equation of Cambridge Theories of Growth Cycles

The following nonlinear, endogenous, nonstochastic differential-difference equation subsumes every classic equation that characterises the models of the pioneers of nonlinear, endogenous, nonstochastic business cycle theories – all the way from Kalecki, via Lundberg and Kaldor, to Goodwin and Hicks:

$$\varepsilon y'(t+\theta) + (1-\alpha)\, y(t+\theta) = O_A(t+\theta) + \phi\left[y'(t)\right] \tag{2}$$

Where:

y : aggregate income; θ: one half the construction time of new equipment; $\phi\left(y^{'}\right)$: the flexible accelerator; O_A : the sum of autonomous outlays ($\beta(t)$ and $l(t)$);

The standard canonical equation (1), given earlier, is the basis for Yasui's famous formal 'equivalence' result, [96] between the classic nonlinear, endogenous, nonstochastic models of aggregate fluctuations developed by the pioneers: i.e., Kaldor, Goodwin and Hicks. That equation was obtained from (2) by an approximation that is hard to justify on economic grounds, and assuming technical progress to be an exogenously given constant. It entailed an approximation of the nonlinear differential-difference equation (2) by a nonlinear differential equation of unforced van der Pol-rayleigh type, such a (1). This was obtained simply by expanding the two leading terms of (2) in a Taylor series and retaining only the relevant first two terms.

The result was an appeal to standard results in planar dynamical systems and the genesis of limit cycles, existence of which was occasionally proved by an appeal to the celebrated Poincaré-Bendixson theorem or the Levinson-Smith theorem. This became an academic 'industry', first pioneered by the Japanese trio of Yasui, Ichimura and Morishima (cf. for an almost exhaustive story of this episode in [89]).

But, unfortunately, this approximation of the economically derived (2), by a mathematically convenient (1), implies that the Postkeynesian precepts on historical time and non-ergodicity/complexity are not satisfied. Apparently, also, functional income distribution and systemic uncertainty[37] are difficult to demonstrate in such a system. However, these properties can be shown in a more accurate approximation, retaining higher order terms of the Taylor series, of (2), say 3, 4 and 5 terms, as in the following three equations:

$$\varepsilon\frac{\theta^2}{2}y'''(t)+\left[\varepsilon\theta+(1-\alpha)\frac{\theta^2}{2}\right]y''(t)+[\varepsilon+(1-\alpha)\theta]\,y'(t)-\phi\,[y'(t)]+(1-\alpha)\,y(t)=0 \tag{3}$$

$$\varepsilon\frac{\theta^3}{6}y''''(t)+\left[\varepsilon\frac{\theta^2}{2}+(1-\alpha)\frac{\theta^3}{6}\right]y'''(t)+\left[\varepsilon\theta+(1-\alpha)\frac{\theta^2}{2}\right]y''(t)+[\varepsilon+(1-\alpha)\theta]\,y'(t)-\phi\,[y'(t)]+(1-\alpha)\,y(t)= \tag{4}$$

$$\varepsilon\frac{\theta^4}{24}y'''''(t)+\left[\varepsilon\frac{\theta^3}{6}+(1-\alpha)\frac{\theta^4}{24}\right]y''''(t)+\left[\varepsilon\frac{\theta^2}{2}+(1-\alpha)\frac{\theta^3}{6}\right]y'''(t)+\left[\varepsilon\theta+(1-\alpha)\frac{\theta^2}{2}\right]y''(t)+[\varepsilon+(1-\alpha)\theta]\,y' \tag{5}$$

A simulation of these more finessed approximations, with the same values for the parameters as in [17], 'restores' the two Postkeynesian precepts of historical time and non-ergodicity/complexity, and, in fact, strengthens its possibility of demonstrating the fallacy of composition. For example the much vaunted property of (1) to generate a (unique) limit cycle, independent of initial conditions – i.e., independent of 'history' – is not relevant for the more finessed approximation, where not only is apparent uniqueness is lost, but also the appearance of unstable limit cycles can be shown, in addition, of course, to multiple limit cycles, each dependent on initial conditions[38].

[37] To show systemic uncertainty in this kind of dynamics, as mentioned earlier, it is necessary to show that these more finessed approximate systems are capable of computation universality. For now, I am not able to offer a convincing proof, although I am fairly certain that it is possible with some concentrated work on the construction of the equivalence between the trajectory of a coupled, forced, Rayleigh-van der Pol system and the computing trajectory of a Turing Machine, initialised consistently with the dynamical system.

[38] As a matter of fact this kind of behaviour was shown by Chang and Smyth ([2]) for the original Kaldor model, making nonsense of Kaldor's unscholarly remarks on the pioneering works of Kalecki, Goodwin, Hicks and Kaldor himself (as quoted and pointed out above).

3.2.2 The Canonical Nonlinear System of Differential Equations of Cambridge Theories of Growth Cycles

Starting from Goodwin's celebrated **Dobb Festschrift** classic, *A Growth Cycle* ([19]), it is possible to divest it of all its non-Keynesian elements – assumption of Say's Law, a fixed-coefficient production function, and so on – and also its unattractive mathematical features – primarily structural instability – whilst preserving its crucial emphasis on functional income distribution as the accommodating, adjusting, variable in the disequilibrium path, and generate the following canonical three variable system of nonlinear differential equations, parametrised by *Tobin's q*, capable of satisfying many of the precepts for a Cambridge theory of growth cycles:

$$\frac{\dot{u}}{u} = F\left(u, v, y; q\right) \tag{6}$$

$$\frac{\dot{v}}{v} = G\left(u, v, y; q\right) \tag{7}$$

$$\frac{\dot{y}}{y} = H\left(u, v, y; q\right) \tag{8}$$

Where the notation is as in Goodwin's classic, except for the removal of the Say's Law assumption by defining:

$$y = \frac{Y_d}{Y_s} \tag{9}$$

where: Y_d : Demand for output (real); Ys : Supply of output (real);

Introducing differential savings propensities in classic Kaldorian fashion, we can get:

$$y = 1 + u\left[s_c\left(u\right) - s_w\left(u, v\right)\right] - s_c\left(u\right) \tag{10}$$

The assumption on production is via the technical progress function, along (once again) Kaldorian lines:

$$\frac{\dot{Y}_s}{Y_s} - \frac{\dot{L}}{L} = \Psi\left[\left((\frac{\dot{Y}_s}{Y_s} - \frac{\dot{L}}{L}) - (\frac{\dot{Y}_s}{Y_s} - \frac{\dot{K}}{K}\right), \left(\frac{\dot{w}}{w} - \frac{\dot{p}}{p}\right)\right] \tag{11}$$

Under very standard assumptions it can be shown that the dynamical system in the unemployment ratio (v), functional income distribution (u) and disequilibrium in the good market (y), i.e., equations (6) $\sim$ (8), parametrised by *Tobin's q*, exhibits a *Hopf bifurcation* from a limit point to a non-trivial periodic orbit ([88]). Every Postkeynesian precept, except systemic uncertainty and 'time-to-build' can be satisfied by this system, although historical time, non-ergodicity/complexity and the fallacy of composition requires that the above building blocks be considered for 'subsectors' of the economy (as, for example, in yet another of the Goodwin classics: *Dynamical Coupling with Especial Reference to Markets Having Production Lags*, [15]).

4 Minsky's Theory of Crisis

> "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."
>
> Hyman Minsky, [63], 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 , 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 ([36]) and Dudley Dillard ([7]), although there are also some stray Schumpeterian elements dotting the Minsky vistas.

Papadimitriou and Randall Wray ([69], 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' ([64], pp.105-6), Minsky postulates his famous 'three types of financial postures': *Hedge finance*, *Speculative finance* and *'Ponzi' finance*. The 'path-dependence' – i.e., historydependence – 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 ([51]) and Myrdal ([67]) – had devised a similar scheme, under the forces of 'non-probabilistic 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.

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.

Some 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.

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, Hicks and Goodwin (see the previous section). 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 [60] and [61] to [62], [9] and [6], there is a series of misrepresentations of the structure, mathematics and economics of the pioneering nonlinear macrodynamic models[39].

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 ([84]) or its more pedagogical and clearer version in [83], 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)[40] , 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.

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

[39] There is the preposterous assertion, in [62], 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 [47], 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.

[40] '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, [68], [54], and the many references therein).

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*[41]. 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 ([38], especially p. 242)[42] . If every configuration of the economy is equilibrium , 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 [83], 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 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.

Finally, to what extent does a Minsky crisis model satisfy the Postkeynesian precepts? I am in the unfortunate position of being completely baffled how to answer this question!

[41]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.

[42]Minsky's indebtedness to Dillard's reading of *Chapter 17* of the *GT* is most clearly expressed in [65], 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 ([81]). 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, [49]), 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 (op.cit), 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.

5 Concluding Notes

Postkeynesian economics is nothing if it is not endogenously dynamic and policy oriented. This was the natural domain in which the Cambridge theories of growth cycles was developed. It is, ostensibly, also the domain of analysis of Minsky, although here the nonlinear dimension is too slippery to locate.

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 . 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 ([42]).

If we are to go beyond conventional nonlinear dynamics and broach new analytic frontiers to formalize the Postkeynesian insights of the pioneers, my conjecture is that we must respect the natural domain of economic data: i.e., the natural or rational numbers, both positive and negative. This implies analytical, epistemological and methodological conventions and constraints that will entail less closed, less determined, mathematical models, encapsulating the richness of undecidable propositions in incomplete formal systems, facing uncomputable functions in the natural domain of economic data, economic institutions and history.

In other words, economic formalism, to be faithful to the rich Postkeynesian tradition, based on historical time and natural data and institutions, must embark on a Diophantine revolution in economics[43].

[43] As I have argued in a series of contributions, a synthesis of which can be found in [93].

References

[1] Blatt, John. M (1983), **Dynamic Economic Systems: A Post-Keynesian Approach**, M.E.Sharpe, Inc., Armonk, New York.

[2] Chang, W. W & Smyth, W. W (1971), *The Existence and Persistence of Cycles in a Non-Linear Model: Kaldor's 1940 Model Re-examined*, **Review of Economic Studies**, Vol. 38, # 1, January, pp.37-44.

[3] Davidson, Paul (1982/3), *Rational Expectations: A Fallacious Foundation for Studying Crucial decision-Making Processes*, **Journal of Post Keynesian Economics,** Vol. 5, # 2, Winter, pp.182-198.

[4] Davidson, Paul (2003/4), *Setting the Record Straight on "A History of Post Keynesian Economics"*, **Journal of Post Keynesian Economics**, Vol. 26, #2, Winter, pp. 245-272.

[5] Day, Richard (1992), *Models of Business Cycles: A Review Article*, **Structural Change and Economic Dynamics**, Vol. 3, # 1, June, pp.177-182.

[6] Delli Gatti, Domenico, Mauro Gallegati and Hyman MInsky (1994), *Financial Institutions, Economic Policy and the Dynamic Behavior of the Economy*, Paper prepared for the **International J. A. Schumpeter Society Fifth Conference** at Münster, Germany, August, 17-20, 1994.

[7] Dillard, Dudley (1955), *The Theory of a Monetary Economy*, chapter 1, pp. 3-30, in: [45].

[8] Dunn, Sephen. P (2004), *Non-ergodicity*, pp.280-4, in: **The Elgar Companion to Post Keynesian Economics**, edited by J. E. King, Edward Elgar Publishing Limited, Cheltenham, UK.

[9] Ferri, Piero and Hyman Minsky (1992), *Market processes and Thwarting Systems*, **Structural Change and Economic Dynamics**, Vol. 3, #1, pp. 79-91.

[10] Fontana, Giuseppe (2005), *A history of Post Keynesian economics since 1936: some hard (and not so hard) questions for the future*, **Journal of Post Keynesian Economics**, Vol. 27, #3, Spring, pp., 409-421.

[11] Fontana, Giuseppe, B. Gerrard & J. Hillard (1998), *The Death of Post Keynesian Economics*, University of Leeds, Leeds, UK.

[12] Frydman, Roman & Michael. D Goldberg (2007), **Imperfect Knowledge Economics: Exchange Rates and Risk**, Princeton University Press, Princeton, NJ.

[13] Godley, Wynne & Marc Lavoie (2007), **Monetary Economics: An Integrated Approach to Credit, Money, Income, Production and Wealth**, Palgrave Macmillan, Houndmills, Basingstoke, Hampshire.

[14] Goodwin, Richard. M (1946), *Innovations and the Irregularity of Economic Cycles,* **The Review of Economics and Statistics,** Vol. 28, No. 2, May, pp. 95-104.

[15] Goodwin, Richard. M, (1947), *Dynamical Coupling with Especial Reference to Markets Having Production Lags*, **Econometrica**, Vol. 15, # 3, July, pp. 181–204.

[16] Goodwin, Richard. M (1950), *A Non-Linear Theory of the Cycle*, **The Review of Economics and Statistics**, Vol. XXXII, No. 4, November, pp. 316-320.

[17] Goodwin, Richard. M (1951), *The Nonlinear Accelerator and the Persistence of Business Cycles*, **Econometrica**, Vol. 19, #1, January, pp. 1-17.

[18] Goodwin, Richard. M (1955), *A Model of Cyclical Growth*, pp.203-221, in: **The Business Cycle in the Post-War World,** *Proceedings of a Conference held by the International Economic Association, Oxford, September, 1952*, Edited by Erik Lundberg, Assisted by A.D. Knox, Macmillan & Co Ltd., St. Martin's Press, New York.

[19] Goodwin, Richard. M (1967), *A Growth Cycle*, in: **Socialism, Capitalism & Economic Growth: Essays Presented to Maurice Dobb**, edited by Charles H. Feinstein, pp. 54-58, Cambridge University Press, Cambridge.

[20] Goodwin, Richard. M (1992), *A Chaotic Reformulation of Multiplier-Accelerator Models*, chapter 7, pp. 87-101, in: **Nonlinearities, Disequilibria and Simulation: Quantitative Methods in the Stabilization of Macrodynamic Systems** - Essays in Honour of Björn Thalberg, edited by Kumaraswamy Velupillai, The Macmillan Press Ltd., Houndmills, Hampshire.

[21] Hammarskjöld, Dag (1933), **Konjunkturspridningen: En Teoretisk och Historisk Undersökning**, Kongl. Boktryckeriet, P. A. Norstedt & Söner, Stockholm.

[22] Hamouda, O. F & G.C. Harcourt (1988), *Post Keynesianism: From Criticism to Coherence*, **Bulletin of Economic Research**, Vol. 40, #1, pp.1-33.

[23] Harcourt, G. C (1972), **Some Cambridge Controversies in the Theory of Capital**, Cambridge University Press, Cambridge.

[24] Harcourt, G. C (2002), **The Structure of Post-Keynesian Economics**, Cambridge University Press, Cambridge.

[25] Harrod, Roy. F (1936), **The Trade Cycle**, Clarendon Press, Oxford.

[26] Harrod, Roy. F (1936), *Imperfect Competition and the Trade Cycle*, **Review of Economics and Statistics**, Vol. 17, #2, May, pp.84-88.

[27] Harrod, Roy. F (1951), **The Life of John Maynard Keynes**, Macmillan & Co. Ltd., London.

[28] Hicks, John. R (1950), **A Contribution to the Theory of the Trade Cycle**, Clarendon Press, Oxford.

[29] Hicks, John (1973), **Capital and Time: A Neo-Austrian Theory**, Clarendon Press, Oxford.

[30] Ichimura, Shinichi (1955), *Toward a General Nonlinear Macrodynamic Theory of Economic Fluctuations*, chapter 8, pp. 192-226, in: [45].

[31] Johnson, Harry. G (1951-1952), *Some Cambridge Controversies in Monetary Theory*, **The Review of Economic Studies**, Vol. 19, #2, pp. 90-104.

[32] Kaldor, Nicholas (1940), *A Model of the Trade Cycle*, **The Economic Journal**, Vol. 50, March, pp. 78-92.

[33] Kaldor, Nicholas (1954), *The Relation of Economic Growth and Cyclical Fluctuations*, **The Economic Journal**, Vol. 64, # 253, March, pp.53-71.

[34] Kaldor, Nicholas (1961), *Remarks* during *The Discussion of Professor Lutz's Paper*, in: **The Theory of Capital:** *Proceedings of a Conference held by the International Economic Association*, edited by F. A. Lutz & D. C. Hague, Macmillan & Co., Ltd., London.

[35] Kalecki, Michal (1935), *A Macrodynamic Theory of Business Cycles*, **Econometrica**, Vol. 3, #3, July, pp.327-344.

[36] Kalecki, Michael (1942), *A Theory of Profits*, **The Economic Journal,** Vol. 52, # 206/207, June-September, pp. 258-267.

[37] Keynes, John Maynard (1921), **A Treatise on Probability**, Macmillan and Co., Limited, London.

[38] Keynes, John Maynard (1936), **The General Theory of Employment, Interest and Money**, Macmillan and Co., Limited, London.

[39] Keynes, John Maynard (1937), *The General Theory of Employment*, **The Quarterly Journal of Economics**, Vol. 51, # 2, February, pp.209-223.

[40] King, J. E (2002), **A History of Post Keynesian Economics Since 1936**, Edward Elgar, Cheltenham, UK.

[41] King, J. E [Editor] (2003), **The Elgar Companion to Post Keynesian Economics**, Edward Elgar Publsihing Limited, Cheltenham., Glos., UK.

[42] Kirby, Laurie and Jeff Paris (1982), *Accessible Independence Results for Peano Arithmetic*, **Bulletin of the London Mathematical Society**, Vol. 14, pp. 285-293.

[43] Knight, Frank. H (1921), **Risk, Uncertainty and Profit**, Houghton Mifflin Company, Boston & New York.

[44] Kregal, J. A (1985), *Hamlet without the Prince: Cambridge Macroeconomics without Money*, **American Economic Review**, Papers and Proceedings, Vol. 75, # 2, May, pp.133-139.

[45] Kurihara, Kenneth. K (1955; editor), **Post-Keynesian Economics**, George Allen And Unwin Ltd., London.

[46] Kurihara, Kenneth. K (1955), *Distribution, Employment and Secular Growth*, chapter 10, pp. 251-273, in: [45].

[47] Kurihara, Kenneth. K (1960), *An Endogenous Model of Cyclical Growth*, **Oxford Economic Papers** - *New Series*, Vol. 12, #3, October, pp. 243-248.

[48] Lavoie, Marc (2006), **Introduction to Post-Keynesian Economics**, Palgrave Macmillan, Basingstoke, Hampshire, UK.

[49] Lerner, Abba. P (1952), *The Essential Properties of Interest and Money*, **The Quarterly Journal of Economics**, Vol. 66, #2, May, pp. 172-193.

[50] Lindahl, Erik (1924), **Penningpolitikens Mål och Medel**, Förlagsaktiebolagets I Malmö BOKTRYCKERI, Malmö.

[51] Lindahl, Erik (1929), **Penningpolitikens Mål**, C. W. K. Gleerup, Lund.

[52] Lindahl, Erik (1929), **Penningpolitikens Medel**, C. W. K. Gleerup, Lund.

[53] Lundberg, Erik (1937), **Studies in the Theory of Economic Expansion**, P.S.King & Son, London.

[54] Lundberg, Erik Filip (1996), **The Development of Swedish and Keynesian Macroeconomic Theory and its Impact on Economic Policy**, Cambridge University Press, Cambridge.

[55] Mankiw, N. Gregory & David Romer (1991), **New Keyneisan Economics,** *Volume 1: Imperfect Competition and Sticky Prices*, The MIT Press, Cambridge, Massachusetts.

[56] Mankiw, N. Gregory & David Romer (1991), **New Keyneisan Economics,** *Volume 2: Coordination Failures and Real Rigidities*, The MIT Press, Cambridge, Massachusetts.

[57] Matthews, Robin (1984, [1991]), *Animal Spirits*, Chapter 7, pp. 103-125, in: **Thoughtful Economic Man: Essays on Rationality, Moral Rules & Benevolence**, Cambridge University Press, Cambridge.

[58] McLaughlin, B.P (1992), *The Rise and Fall of British Emergentism* in: **Emergence or Reduction - Essays on the Prospects of Nonreductive Physicalism**. edited by Ansgar Beckermann, Hans Flohr & Jaegwon Kim (pp. 49-93). Berlin: Walter de Gruyter.

[59] Metzler, Lloyd. A (1941), *The Nature and Stability of Inventory Cycles*, **The Review of Economics and Statistics**, Vol. 23, # 3, August, pp. 113-129.

[60] Minsky, Hyman P (1957), *Monetary Systems and Accelerator Models*, **American Economic Review,** Vol. XLVII, # 6, December, pp. 860-883.

[61] Minsky, Hyman P (1959), *A Linear Model of Cyclical Growth*, **The Review of Economics and Statistics**, Vol. 41, #2, Part 1, May, pp. 133-145.

[62] Minsky, Hyman P (1965), *The Integration of Simple Growth and Cycle Models*, in: **Patterns of Market Behavior: Essays in Honour of Philip Taft** edited by Michael J. Brennan, reprinted as Chapter 12, pp. 258-277, in: [64].

[63] Minsky, Hyman. P, 1978 [1982], **The Financial Instability Hypothesis: A Restatement**, reprinted in: [64], Chapter 5, pp. 90-116.

[64] Minsky, Hyman. P (1982), **Can "It" Happen Again?: Essays on Instability and Finance**, M. E. Sharpe, Inc., Armonk, New York.

[65] Minsky, Hyman (1985), *The Legacy of Keynes*, **The Journal of Economic Education**, Vol. 16, No. 1, Winter, pp. 5-15.

[66] Myrdal, Gunnar (1927), **Prisbildningsproblemet och Föränderligheten: Akademisk Avhandling**, Almqvist & Wiksells Boktryckeri - A.-B., Uppsala.

[67] Myrdal, Gunnar (1931), *Om penningteoretisk jämvikt. En studie över den "normala räntan" i Wicksells penninglära*, **Ekonomisk Tidskrift**, Årg. 33, häft 5/6, pp. 191-302.

[68] Myrdal, Gunnar (1982), **Hur Styrs Landet – Första Delen**, Rabén & Sjögren, Borås.

[69] Papadimitriou, Dimitri. B & L. Randall Wray (2008), *Minsky's Stabilizing An Unstable Economy: Two Decades Later*, pp. xi-xxxv, in: **Stabilizing An Unstable Economy** by Hyman P. Minsky, McGraw-Hill, New York.

[70] Pasinetti, Luigi. L (2007), **Keynes and the Cambridge Keynesians: A 'Revolution in Economics' to be Accomplished**, Cambridge University Press, Cambridge.

[71] Phelps, Edmund S (1990), **Seven Schools of Macroeconomic Thought**, Clarendon Press, Oxford.

[72] Prescott, Edward. C (2004), *The Transformation of Macroeconomic Policy and Research*, **Nobel Memorial Prize in Economics,** *Lecture*, December 8, 2004.

[73] Samuelson, Paul. A (1947), **Foundations of Economic Analysis**, Harvard University Press, Cambridge, Massachusetts.

[74] Samuelson, Paul. A (1970 [1972]), *Maximum Principles in Analytical Economics*, **Nobel Memorial Lecture**, December 11, 1970.Reprinted in: **The Collected Scientific Papers of Paul A. Samuelson**, **Volume III,** edited by Robert C. Merton, Chapter 130, pp. 2-17; The MIT Press, Cambridge, Mass.

[75] Schumpeter, Joseph A (1934, [1912]), **The Theory of Economic Development: An Inquiry into Profits, Capital, Credit, Interest and the Business Cycle**, translated from the German by Redvers Opie, Oxford University Press, Oxford.

[76] Schumpeter, Joseph. A (1927), *The Explanaton of the Business Cycle*, **Economica**, # 21, December, pp.286-311.

[77] Schumpeter, Joseph. A (1954), **History of Economic Analysis**, George Allen & Unwin Ltd., London.

[78] Shackle, George. L. S (1966), **The Nature of Economic Thought: Selected Papers 1955-1964**, Cambridge University Press, Cambridge.

[79] Simon, Herbert A (1955), *A Behavioral Model of Rational Choice*, **The Quarterly Journal of Economics,** Vol. LXIX, February, pp. 99-118.

[80] Simon, Herbert A (1983), **Reason in Human Affairs**, Basil Blackwell, Oxford.

[81] Sraffa, Piero (1932), *Dr Hayek on Money and Capital*, **The Economic Journal**, Vol. 42, #165, March, pp. 42-53.

[82] Stigler, George. J (1948), *Review* of the *Foundations of Economic Analysis*, **Journal of the American Statistical Association,** Vol. 43, # 244, December, pp.603-5.

[83] Taylor, Lance (2004), **Reconstructing Macroeconomics: Structuralist Proposals and Critiques of the Mainstream**, Harvard University Press, Cambridge, Massachusetts.

[84] Taylor, Lance & Stephen A. O'Connell (1985), *A Minsky Crisis*, **Quarterly Journal of Economics**, Vol. 100, *Supplement*, Vol. 100, # 5, pp. 871-885.

[85] Tinbergen, Jan (1931), *Ein Schiffbauzyklus*, **Weltwirtschaftliches Archiv**, Vol. 34, Issue 1, pp.152-164.

[86] Tymoigne, E & F. Lee (2003-4), Post Keynesian Economics since 1936: *A History of a Promise That Bounced*, **Journal of Post Keynesian Economics**, Vol. 26, #2, Winter, pp. 273-287.

[87] Velupillai, K. Vela (1997), *Exploring '"The Original kalecki Model T"*, chapter 20, pp. 254-264, in: **Markets, Unemployment and Economic Policy: Essays in Honour of Geoff Harcourt**, Volume Two, edited by Philip Arestis, Galbriel Palma & Malcolm Sawyer, Routledge, London.

[88] Velupillai, K. Vela (2006), *A Disequilibrium Macrodynamic Model of Fluctuations*, **Journal of Macroeconomics**, Vol. 28, Issue 4, December, pp. 752-767, 2006.

[89] Velupillai, K. Vela (2008), *Japanese Contributions to Non-Linear Cycle Theory in the 1950s*, **Japanese Economic Review**, Vol. 59, No. 1, March, pp. 54-74.

[90] Velupillai, K. Vela (2009), *A Perspective on a Hicksian Non-Linear Theory of the Trade Cycle*, in: **Markets, Money and Capital: Hicksian Economics for the 21st Century**, Chapter 18, pp. 328-345, edited by Roberto Scazzieri, Amartya Sen & Stefano Zamagni, Cambridge University Press, Cambridge.

[91] Velupillai, K. Vela (2010), *Credit Crisis Theory* Redux*: A Critique and a Reconstruction***,** chapter 10, pp**.**233-248 in: **Understanding Ireland's Economic Crisis: Prospects for Recovery**, Edited by Stephen Kinsella and Anthony J. Leddin, Blackhall Publishing, Co. Dublin, Ireland.

[92] Velupillai, K. Vela (2010), **Computable Foundations for Economics**, Routledge, London.

[93] Velupillai, K. Vela (2011), *Towards an Algorithmic Revolution in Economic Theory*, forthcoming in: **The Journal of Economic Surveys**, Vol. XXV, #1.

[94] Velupillai, K. Vela & N. Dharmaraj (2011), *The Time-to-Build Tradition in Business Cycle Modelling*, ASSRU Discussion Paper 09-2011, March.

[95] West, Bruce. J (1985), **An Essay on the Importance of Being Nonlinear**, Springer-Verlag, berlin & Heidelberg.

[96] Yasui, Takuma (1953), '*Self-Excited Oscillations and the Business Cycle*', **Cowles Commission Discussion Paper**, No. 2065.