Origins and Early Development of the Nonlinear Endogenous Mathematical Theory of the Business Cycle

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1. Prologue

> He [Oppenheimer] studied me with his remarkable blue eyes and asked, «What is new and firm in Physics?» The «and firm» impressed me.
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> Bernstein J. (2005), *Oppenheimer: Portrait of an Enigma*, Chicago, Ivy Publishers

We begin with a puzzle: Wicksell observes a 20-year deflation and constructs an *unstable model of inflation for stabilization purposes*. Why? The same fact, observed and recorded in their writings, led Fisher and Schumpeter to emphasize other aspects of the behaviour of economic institutions, agents and the economic system's *evolutionary dynamics*. Fisher developed the link between appreciation and interest via *expectations*; Schumpeter, on the other hand, that between *deflation* and *innovation* to justify the tendency for a capitalist system to undergo benign fluctuations.

A young macroeconomist facing, say an ageing Walras, at the turn of the century that took the 19th into the 20th, and confronted with the kind of question Bernstein was posed by Oppenheimer, may have had difficulties identifying the unstable cumulative process, the Fisher equation and Schumpeterian evolutionary dynamics as being part of the «and firm» description of the subject; although she may have recognized them as «new». After all, *even the subject did not exist at that time*.

In March 1952, during a lecture in Stockholm, Eli Heckscher (Hecksher, 1952) recalled, on *14 April 1898*, Wicksell «somewhat unexpectedly revealed

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We are both deeply indebted to Professor Stefano Zambelli for invaluable help over many years in making it possible for us to tell the kind of story narrated here. We are also indebted to our colleague Selda Kao for valuable intellectual and logistical assistance. We are responsible for the remaining infelicities.

before the [Stockholm Economic] Society what was perhaps his greatest theoretical achievement, his theory of the connection between interest rate and money value» (*ibidem*, p. 119). *Thus was born modern macroeconomics*.

Macroeconomics is a word coined in 1939 by the Swedish economist Erik Lindahl[1], himself Wicksell's distinguished pupil in the theory of public finance and taxation. The word had been in use, in academic circles in Sweden and Norway, from the early 1930s after Ragnar Frisch and Michał Kalecki had popularised the term macrodynamics in discussions about the problems of the trade cycle. But it was Lindahl who explicitly contrasted the word macroeconomics with microeconomics, in the senses in which we use them in modern economic theoretical discourse; and he did so in his famous book *Studies in the Theory of Money and Capital* (Lindahl, 1939).

It is, proverbially, a new name for an old subject. However, it was Wicksell – and, to a lesser extent, Fisher – not Keynes nor Hayek, who first stamped it with modernism in an unmistakable way – the modernism we associate with providing microfoundations for aggregate variables and behaviour. This he provided for the twin horns of macroeconomics – the real and the monetary sides; for the former on the basis of Austrian capital theory, which he almost single-handedly and rigorously re-wrote and re-did for Menger, Böhm-Bawerk and von Wieser; for the latter, on the basis of a wholly new approach to monetary theory by devising an innovative thought-experiment – *gedankenexperiment* – which obviated the need for a reliance on the quantity theory of money to explain inflation. This thought-experiment constructed a pure credit economy in which monetary transactions were conducted in an imaginary *giro* system.

The crucial event that spurred him to these conceptual innovations was the 20-year deflation – not recession – experienced, without exception, by all the advanced industrial nations, from the mid-1870s to the mid-1890s. He was – as Fisher was – deeply concerned that this deflation meant an unwarranted redistribution of wealth and income between lenders and borrowers. The theoretical discussion on bimetallism, and its policy ramification, had reached its summit.

The only conceptual tool that was available for *policy purposes* was the quantity theory of money. A reliance on this would have meant a further deepening of the deflationary process and an exacerbation of the unjust income and wealth distributions. He had to devise an alternative vision of the monetary mechanism in such a way that it would yield policy perspectives and tools that would stabilize the price level, whilst preserving consistency with the microeconomics of relative prices in a situation of deflationary dy-

[1] See, however, Velupillai (Velupillai, 2009), for reasonably complete details on the issue of the origins of the word Macroeconomics. In passing it should be stressed that the origins of the word attributed to Jacob Marschak in *The Economist*'s article on *The Other Worldy Philosophers*, on 16 July 2009, is incorrect; and so are the claims in the ensuing published letter to the editor of *The Economist*, by Kevin Hoover.

namics. Thus was born the Wicksellian analogue of the Malthusian mechanism: the discrepancy between the money rate of interest, determined by banking policy, and the natural rate of profit resulting from the capital structure of the production system.

Independently, and motivated by the same events and concerns, Irving Fisher had suggested an alternative mechanism for the interpretation and resolution of the same problem. In a sense, modern macroeconomics is an uncoordinated amalgam of Fisher's expectational mechanism and Wicksell's capital theoretic underpinnings on *Clower's monetary macroeconomic thought-experiments*.

In this paper our implicit working hypothesis is that the dynamics of Keynesian macroeconomics in Harrod (1936), the sequence analysis of the Swedes, most explicitly formulated in Lundberg (1937), that which has come to be called the «time-to-build» approach to business cycle theory, but originally in mathematical form encapsulated in the early work by Tinbergen (1931) and Kalecki, and the «cobweb» tradition, most elegantly broached, in a mathematical mode, by Leontief (1934), were the first successes in the drive *to integrate cycle theory, intrinsically, to macroeconomic theory*, as this subject itself emerged in a definable form in the 1930s. That these theories and their mathematical formulations have been subverted at the frontiers does not mean they have disappeared from the active research agenda of many scholars, working in a variety of traditions that cannot be encapsulated within any kind of equilibrium orthodoxy. Although we do not address these latter issues in this paper; it will form part of the subject matter of one of the sequels to this narrative. Moreover, we would like to assert, quite categorically, that we adhere to the *methodology* of nonlinear, endogenous mathematical modelling of macroeconomic fluctuations. It is our definite belief – going beyond «opinion» – that *epistemologically*, too, this approach is superior to the dominant linear stochastic approach to modelling macroeconomic fluctuation. In parallel work we have demonstrated, formally, this claim, from the point of view of the epistemology of computation.

In the next section we outline, in a very concise form, the early – essentially confined to the early years of the 1930s – attempts and discussions on the need to integrate cyclical phenomena with economic theory, especially, though not exclusively, equilibrium economic theory. In section 3 we attempt to describe the kinds of ways intrinsically nonlinear macroeconomic theories were mathematised nonlinearly. The concluding section suggests a way to proceed with this narrative, to a second stage, when consolidation of both the macroeconomic theory of the business cycle, and its mathematical formalisation, outlined in the paper came to maturity in the Golden quarter century of Keynesian Macroeconomics, i.e., 1947-72 – then declined, rose – and, in recent years, seems to have fallen again.

2. Integrating Cyclical Phenomena with Economic Theory

> Eine Krisentheorie kann nie die Untersuchung eines abgesonderten Theiles der socialwirtschaftliches Phänomene sein, sondern sie ist, wenn sie nicht ein diletantisches Unding sein soll, immer das letzte oder vorletzte Capitel eines geschriebenen oder ungeschriebenen socialwirtschaftlichen Systems, die reife Frucht der Erkenntnis sämmtlicher socialwirtschaftlichen Vorgänge und ihres wechselwirkenden Zusammenhanges. Daraus geht ein Doppeltes hervor. Erstens, dass jedem wissenschaftlichen System eine andere Krisentheorie entspricht; und zweitens, dass je weniger reif und vollendet das zugehörige wissenschaftliche System ist, desto hypothetischer, gewagter, sogar abenteuerlicher die darauf gebaute Krisentheorie geraten kann. Es is wie mit den volksthümlichen Auffassungen und Erklärungen vom Wesen der Krankheiten, die nicht auf eine solide Anatomie und Physiologie des menschliehen Organismus aufgebaut sind.
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> von Böhm-Bawerk E.,
> Zeitschrift für Volkswirtschaft Sozialpolitik und Verwaltung,
> *Die Wirtschaftskrisen. Geshichte der nationalökonomischen Krisentheorien*, Vol. VII, p. 132[2]

What began as an exercise in attempting a reconciliation between «theoretical economics» and the phenomenon displayed as «business cycles», in 1898, became, by the 1930s, the attempt to graft business cycle phenomena to equilibrium theory. Three interrelated, simultaneous, phenomena emerged from the attempt to synthesise traditional static, equilibrium, economic theory with *dynamic method*: business cycle theory, monetary macroeconomic theory (as outlined in an ultra-brief mode in the previous section) and the theory of economic policy (for long also referred to as stabilization policy). Two diametrically opposing *visions* – in the strict Schumpeterian sense[3] – of this attempted synthesis were enunciated by two of the giants of 20th century economics: Simon Kuznets and Friedrich von Hayek, both early Nobel Laureates (in 1971 and 1974, respectively). Kuznets, in a fundamental paper[4],

[2] A free translation by Velupillai would be as follows (where *socialwirtschaftliches* is rendered *economic*, although, perhaps, a direct translation of the word may suggest *social economy*, which is a more a 19th century word/phrase): «A theory of crisis can never be based on the analysis of one separate aspect of the economy alone. Unless it is to be an amateurish absurdity, it is always the last or last but one chapter of a written or unwritten system of economics, the ripe fruit of the insight obtained from the totality of the economic processes and their interaction. Two implications follow from this. First, that each scientific system requires its own crisis theory, and second, that the less mature and complete the corresponding scientific system is, the more hypothetical, daring, even preposterous the crisis theory built on it will be. This is similar to the popular understanding and explanation of illnesses, which are not based on a solid anatomy and physiology of the human organism».

[3] See Schumpeter (1954, pp. 41 ff.).

[4] In which he also pointed out that Böhm-Bawerk (see also the opening quote in this

outlining the nature of the synthesis that was being attempted so as to incorporate, in particular, business cycle phenomena that were considered naturally «dynamic», within the fold of the then orthodox equilibrium economic theory, came out with the radical conclusion:

What [should] be discarded is the notion of a stable or slowly varying equilibrium and the equational system of solving economic problems. What is substituted for it is a general recognition of the importance of the time element – a recognition which permits the utilization of the generalized experience of various special investigations in a more complex and a more realistic general theory of economic change. The equilibrium theory, in the limited meaning in which it is retained, will also be enriched, since the general theory of economic change will point out many more important economic factors than have heretofore been included in the equational systems of the mathematical school. *If we are to develop any effective general theory of economic change and any complete theory of economic behaviour, the practice of treating change as a deviation from an imaginary picture of a rigid equilibrium system must be abandoned* (Kuznets, 1930, p. 415; italics added).

Hayek, on the other hand, suggested that[5]:

[T]he thesis of Löwe (which remains... the basis of my own work) that the incorporation of cyclical phenomena into the system of economic equilibrium theory, with which they are in apparent contradiction, remains the crucial *problem of Trade Cycle theory*.

[...]

By «equilibrium theory» we here primarily understand the modern theory of the general interdependence of all economic quantities, which has been most perfectly expressed by the Lausanne School of theoretical economics (Hayek, 1933, p. 33 and 42; italics added).

It should be noted that for Kuznets[6] it was equilibrium theory that faced the problem of incorporating business cycle phenomena into its framework; the opposite is the case for Hayek. Somewhere in between there was Johan

section), as early as 1898, had taken up this topic (Kuznets, 1930, p. 384): «The organic relation between business-cycle theory and theoretical economics was stated by Böhm-Bawerk as early as 1898 (in a book review in the *Zeitschrift für Volkwirtschaft Sozialpolitik und Verwaltung*, Vol. VII, p. 132)». It is interesting to recall, as pointed out in section 1, that it was in 1898 that Wicksell's similar concern for the «organic relation» between *Monetary Theory and Theoretical Economics* – which was, at that time, not specifically identified with «equilibrium economics» – was first expressed in the international literature (Wicksell K. (1898, [1936]), *Interest and Prices*, translated by Richard F. Kahn, London, Macmillan).

[5] Quoted in a fractured way, out of context, and inaccurately by Lucas (1981, p. 215).

[6] Incidentally, the almost «universal» reference to Frisch (1933) as the macrodynamic origins of what is now referred to as the Frisch-Slutsky methodology is seriously unfair to Kuznets (1929), who also pointed out that the classic Slutsky work was even referred to by Mitchell (1927, p. 478).

Åkerman, perhaps best characterised as the lone *Schumpeterian*[7] voice, in an otherwise *Wicksellian Sweden*, whose methodological views were refreshingly original in that he also brought into consideration issues of the roles played by deductive and inductive processes of reasoning in equilibrium theory and cycle theory[8]. A representative view of his stance on the problem of integrating the phenomenon of the business cycle with equilibrium theory, on which he wrote systematically during the decade late 1920s and the whole of the 1930s, may be gleaned from his superbly pedagogical article in the *Ekonomisk Tidskrift* of 1932 (Åkerman, 1932), where also copious references to his previous writings on the subject is made available. It is clear, even with only rudimentary mathematical mastery of nonlinear dynamics, he was advocating an endogenous, nonlinear, deterministic approach to the modelling of business cycle phenomena, although he did not neglect seasonal factors and, to some extent, both exogenous shocks and psychological factors (although critical of Pigou's stance on this factor in the latter's *Industrial Fluctuations* (Pigou, 1927)) also played a role in his desiderata for a formal theory of the cycle within economic theory.

Hicks, in 1933, as, indeed, Kaldor at that time[9], was «minimising [his] differences from Hayek» (Hicks, 1933, p. 28) and went so far as to claim (*ibidem*, p. 29; italics added):

> The great advances that have been made in recent years in our understanding of the Trade Cycle have consisted chiefly of the successful application of economic theory (and especially monetary theory) to the problem of fluctuations [...] The development in our knowledge of the Cycle was thus, from one point of view, a purely

[7] Long before Schumpeterian evolutionary economics, where the cycle was an intrinsic manifestation of the dynamic growth process, was a codified chapter in macroeconomic theory.

[8] It should be recalled that business cycle theory was referred to as *konjunkturtheorie*, as in German, and was differentiated from crisis theory by the use of the word *krisen* for the latter phenomenon. Johan Åkerman's doctoral dissertation (Åkerman, 1928), is an important document in the history of mathematical business cycle theories, not least because Ragnar Frisch was the official examiner. It is the only document, to the best of our knowledge, by any Swedish economist in the interwar period, where there is an explicit acknowledgement to S.D. Wicksell, the statistician son of the great Knut Wicksell: «Under min studietid vid universitetet i Lund har professor Emil Sommarin och professor S.D. Wicksell visat ett livligt intresse för min undersökning och givit mig många värdefulla råd och anvisningar, för vilket jag härmed får uttrycka min stora tacksamhet» [I express my immense gratitude to Professor Emil Sommarin and to Professor S.D. Wicksell for the lively interest they have shown for my investigations and for having given me valuable advice and instruction during my stuies at Lund (KVV's translation of the original Swedish)]. During Velupillai's early years as a doctoral student at the University of Lund, Johan Åkerman was an occasional auditor at special advanced seminars in the department of economics. He was, by then, almost totally deaf and was always accompanied by his wife, who helped him interpret any talk.

[9] Kaldor was the joint translator (together with H.M. Croome) of Hayek's classic *Monetary Theory and the Trade Cycle* (Hayek, 1933).

theoretical development. It took the form of the construction of a theory of Money that finds a place inside general economic theory rather than outside it.

The object of the present paper is to make a small contribution to this theoretical development by enquiring into the place that is to be occupied in the new theory of Money and of the Cycle by the central notion of pure economics: *the concept of equilibrium*.

That this «new theory of Money» was untenable, both from the point of view of a seamless integration with economic theory and as *a foundation for a cycle theory within equilibrium economic theory*, was the message of the two classics by Myrdal (1931) and Sraffa (1932), but it took Hicks more than a quarter of a century to acknowledge the twin messages of the great Swede and the Cambridge Italian maestro!

With the benefit of melancholy – at least from our point of view – hindsight, we now know that the Hayekian vision, in the form of old wine in new bottles, prevailed and is the dominant current approach; the enlightened and challenging vision of a dynamic theory free of viewing change as simply «a deviation from an imaginary picture of a rigid equilibrium system», now survives only in the underworlds of modern day reincarnations of Karl Marx, Silvio Gesell or Major Douglas[10]. Our adherence to this underworld is uncompromisingly complete. It is based on exactly the reasons for which Kuznets advocated the abandonment of equilibrium economics and its formalisations.

Formalisation of *dynamic method*[11] that could encapsulate proper disequilibria, the existence of multiple equilibria and even lack of any equilibria to which the system may or may not tend, or around which fluctuations may or may not recur – whether as small deviations or large and sustained departures, was the sought after criterion such that it was possible to incorporate it coherently with the formal systems of general equilibrium equations of the real economy of orthodox theory. Hence, *dynamic method*, formalised as ordinary differential, difference or mixed difference-differential equations, and, very occasionally, also as differential *inequalities* were to be made an adjunct of, or an integral part of, the systems of equilibrium equations, for which, then, solutions would be sought in a similar manner to traditional methods (*whatever they may have been*). The first, tentative, steps – methodologically – were simple additions of time subscripts to standard variables and a claim that the consistent equilibrium formulation and solutions to this new

[10] Paraphrasing Keynes (1936, p. 32; italics added): «The great puzzle of Effective Demand [...] could only live on furtively, below the surface, *in the underworlds of Karl Marx, Silvio Gesell or Major Douglas*».

[11] We have in mind the idea of formalising an intuitive concept in a precisely defined scientific context. This is similar to the way Alan Turing, and others, formalised the intuitive notion of *calculability* with the precise notion of *computability*. The intuitive notion of continuity is still to find a definitive formalisation, despite claims to the contrary by Bourbaki, and others.

system of equations was an answer to the puzzle of synthesising «change» or «dynamics» and «static equilibrium».

In this paper, and its sequels, we concentrate on those macroeconomic business cycle theories that tried to encapsulate *dynamic method* in terms of *nonlinear* differential, difference and mixed difference-differential equations such that the solutions – *the attractors* in the language of dynamical systems theory – had the potential to display *multiple*, *unstable*, *endogenously generated*, *equilibria*, where the trajectories in any relevant *basin of attraction* would be consistent with well defined economic disequilibria. This is the standard approach of the nonlinear, endogenous, business cycle theories, when appropriately formalised. However, we shall also suggest that theories of the business cycle, for example that associated with *Swedish Sequence Analysis*, may not be consistent with generalised nonlinear dynamical systems modelling. This is because a literal, purist, interpretation of *Swedish Sequence Analysis* suggests that they were seeking to model economic dynamics of a kind that was not associated with any equilibrium. We suggest that this interpretation is not consistent with modelling in terms of any kind of dynamical system[12] and one has to seek, at least in the first instance, a formalism for dynamics that cannot be associated with any kind of differential, difference or mixed difference-differential equation system.

This observation is, in our opinion, dual to Samuelson's important remark on the existence of dynamical systems that cannot be associated with any (useful) maximum principle (Samuelson, 1971) and he gave the homely example of the (nonlinear) multiplier-accelerator model of the business cycle to illustrate the point.

These two principles of modelling – nonmaximum and nonequilibrium economic dynamics – will form the touchstone for the structure and content of the entire work, and their crucial roles will emerge only as the whole tapestry is completed. This part of the story is but one aspect of the final tapestry envisaged.

Finally, the problem setting itself *should* be provided by a background narrative, at the outset, of *two parallel stories*: one, an outline of the business cycle theories that provided the foundations for nonlinear, endogenous, dynamic modelling; two, a concise outline of the parallel development of nonlinear dynamics, but extending backwards to Poincaré, and coming down the years till the dawn of the era of dynamical systems theory – i.e., from Poincaré and the elder Birkhoff, via van der Pol and the Andronov school, and ending with Cartwright-Littlewood, Levinson and the Lefschetz school. This is an outline of a 70-year history[13], as the back-

[12] An utter trivialisation of *Swedish Sequence Analysis* was central to a thoroughly confused study of the *dynamic method* of the «Stockholm School» by Hansson (1982). Suffice it to say, one aspect of the thorough confusion perpetrated in this work is due to a lack of a consistent, formal, circumscribing of the notion of *dynamic method*.

[13] Our «model» here is the excellent expository historical narrative by Aubin - Dalmedico

drop for the kind of mathematical formalisms used in the dynamic method of the theories of nonlinear, endogenous, business cycle theories. The interaction between the formal dynamics invoked by the macroeconomist and that being developed by the mathematician did have some felicitous outcomes, and we will highlight some of them. But this paper will, inevitably, be crippled by leaving out the parallel development in the mathematics of nonlinear dynamics, as itself emerged from nonlinear oscillations theory to become dynamical systems theory. This latter story will also form a part of the completed tapestry.

From the strictly macroeconomic point of view the following fourteen classics will provide the textual foundations on which we will outline the emergence of nonlinear, endogenous, business cycle theories (all of them produced during the 1930s): Tinbergen (1931), Kalecki (1939), Fisher (1933), Hayek (1931), Hawtrey (1931), Myrdal (1931), Frisch (1933), Hicks (1933) Leontief (1934), Keynes (1936), Harrod (1936), Lundberg (1937), Lindahl (1939) and Schumpeter (1939). It is not without significance that eleven of these classics emanated on «this side» of the Atlantic and three were by the members of the «Stockholm School».

Connoiseurs of the history of business cycle theories may wonder at the absence of many classics – in particular the three *League of Nations* commissioned studies by Haberler (1937) and Tinbergen (1939; 1939a). To them our answer is that this is *not* a study of the origins and development of *business cycle theories*; it is, instead, a study of the way a mathematical mode was introduced to study the nonlinear, endogenous, vision of business cycle theory.

From the point of view of the differential, difference and mixed difference-differential equations that were canonical in the formalisation of the dynamics of the emerging nonlinear, endogenous, business cycle theories, the following played crucial roles[14]:

The van der Pol equation:

$$\ddot{x} - k(1 - x^2)\dot{x} + x = 0 \tag{1}$$

Equations of the Liénard type:

$$\ddot{x} + f(x)\dot{x} + g(x) = 0 \tag{2}$$

(2002). The essays by Anosov, Arnold, Il'yashenko, Shil'nikov and Sinai in Boilbruch *et al.* (2005) were also important for the way we structure our own story. Finally, we are also deeply influenced by the «insider's accounts» given in a series of papers by Mary Cartwright, spanning almost forty years of the history of how nonlinear oscillations theory became, first, topological dynamics and, eventually, dynamical systems theory (Cartwright, 1952; 1964; 1974).

[14] In all of the cases, when used in macrodynamic models of the business cycle, x and y signified either aggregate output, income or sectoral (for example agricultural in cobweb models) output values. The nonlinearities encapsulated in the functions $f(x)$, $g(x)$ and $F(x)$ represented the nonlinear accelerator or delayed adjustment of an independent variable.

studied in the Liénard Plane:

(3) $$\dot{x} = y - F(x), \dot{y} = -g(x)$$

The generalized, forced, van der Pol equation:

(4) $$\ddot{x} + f(x, \dot{x})\dot{x} + g(x) = p(t)$$

The Rayleigh equation:

(5) $$\ddot{x} + \eta\left(-\dot{x} + \frac{\dot{x}^3}{3}\right) + x = 0, \{0 < \eta < \infty\}$$

The Logistic Map:

(6) $$x_{n+1} = \lambda x_n (1 - x_n)$$

The difference-differential equation:

(7) $$\sum_{\mu=0}^{m}\sum_{\nu=0}^{n} a_{\mu\nu} y^{(\nu)}(x + \mu) = 0$$

The second-order difference equation:

(8) $$y_{n+1} = F(y_n, y_{n-1}) \quad \forall n = 0, 1, 2, \ldots$$
where $F : \mathbb{R}^2 \to \mathbb{R}$ and given initial conditions $y_0, y_{-1} \in \mathbb{R}$

The first five encapsulated the business cycle theories of Fisher, Keynes, Harrod, Schumpeter and Hawtrey; the sixth, models of the «cobweb» type, as in Leontief; the seventh, in various specialised forms, the business cycle theories of Tinbergen, Kalecki and Frisch; the last one, with variously specified functional forms for *F*, encapsulated variations on the dynamics of *Swedish Sequence Analysis* (although we do not fully subscribe to this interpretation of their «dynamic method»), on the one hand, and the Hicks version of a Keynes-Harrod model of the trade cycle.

> van der Pol believes[15] that even periodic business cycles show a certain analogy to the relaxation oscillation of a physical system. The essential condition for such oscillations is negative damping for small deviations and a rather rapidly increasing positive damping for large deviations from the equilibrium position. The psychological response of certain groups of people to changing business conditions shows doubtless some analogy to the behaviour of mechanical systems capable of relaxations oscillations.
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> von Karman (1940, p. 624)

How reliable are «analogies» in devising fruitful models in economics in general and in economic dynamics in particular? Is it sufficient to rely on analogies at a phenomenological level to justify mathematical modelling of a particular variety and then to seek behavioural and other basic hypotheses to justify that particular kind of formalization? Arguably, no field of formal economic analysis has been subject to serious and systematic «analogical thinking» that has led to mathematical formalizations of one sort or another in more fruitful ways than business cycle theory.

From time to time, distinguished mathematicians, physicists, biologists and other natural scientists make important forays into economics, make fundamental contributions that changes the face of the subject in profound ways, and they themselves return to their own, original disciplines, whilst the economists and economics continue to reap the results of such beneficial influences for years on end. von Neumann, Wald, Mandelbrot, Smale, Gale and a few others come immediately to mind as outstanding examples of such remarkable individuals. There are, of course, less obvious successes and, equally, also less edifying examples of such attempts. The early 30s was a fertile time for this kind of activity and economic theory was at the dawn of becoming almost swamped by a wave of mathematizations that was to change its character beyond recognition forever. Two outstanding natural scientists – one an applied mathematician, in the sense in which the phrase was commonly used a few decades ago, another a classic polymath – suggested a particular formalization for the modelling of the macroeconomic phenomenon of business cycles: P. Le Corbeiller and J.B.S. Haldane. The former advocated the formalization of business cycles as relaxation phenomena in a non-linear dynamical system; the latter advocated the use of integral equations to formalize similar phenomena. Their individual advocacies reflected the particular concerns that had, at that point in time, occupied their fertile minds: maintained oscillations in electrical

[15] For example: «Returning to a general consideration of relaxation oscillations many more instances of these oscillations can be cited... Even the periodic reoccurrence of economical crises and epidemics may possibly follow similar laws» (van der Pol, 1934, p. 1081).

and mechanical units in the case of Le Corbeiller and evolutionary biological phenomena in the case of Haldane. We try to tell the circumstances that led to Le Corbeiller's innovative suggestion being taken up by an economist who, subsequently, pioneered the non-linear approach to business cycle modelling. However, we do not mention Haldane's name in these contexts frivolously! The same economist, in a later «incarnation», was directly and personally influenced by Haldane to further the non-linear cause in macrodynamic modelling in even more dramatic ways. That, too, forms a lining in this story – but only as a kind of icing on the cake. Perhaps the implicit message in the way the story will be constructed and narrated is that fertile cross-disciplinary harvests require timely seedings in receptive soil to be nurtured by men and women of imaginative, tenacious and audacious temperament. This is because harvests take time to mature and blossom.

One important theme here is to tell the story of mathematical business cycle theories as adventures in non-linear dynamics. Thus, it will not be a complete story – of the past, the present or possible future – of mathematical business cycle theories; only the part that embraced and was fertilised and enriched by being modelled as non-linear dynamical systems.

In this section, a succinct description of the way nonlinear dynamics was introduced into formal business cycle theory is given. There is a discussion of the way a purely economically motivated hypothesis was fruitfully formalised as a *characteristic* underpinning a special case of Liénard's equation. The serendipitous way Goodwin and Le Corbeiller came to meet and collaborate is also described.

> [E]conomists will be led, as natural scientists have been led, to seek in nonlinearities as explanation of the maintenance of oscillation. Advice to this effect, given by Professor Le Corbeiller in one of the earliest issues of this journal, has gone largely unheeded (Goodwin, 1951, p. 1).

The thirty years in consideration was a period of flourishing and fertile research in the mathematical modelling of business cycles. Our choice of precisely these initial and terminal years are motivated by *ex-post* considerations. To the best of our knowledge, it was in 1928 that the idea of interpreting economic cycles as being generated by a *non-linear dynamical system* capable of *relaxation oscillations* was first hypothesized:

> The present writer would like to point out that the applicability of the principle of relaxation-oscillations to economic cycles was first emphasized by him in 1928 [at the May 7, 1928, Meeting of the Batavian Society of Logic Empirical Philosophy] in a discussion following a paper read by Messers. Van der Pol and J. van der Mark on «The Heartbeat Considered as a Relaxation-Oscillation, and an Electrical Model of the Heart» (Hamburger, 1934, p. 112)[16].

[16] Velupillai's discovery of Hamburger's work is as follows: Concisely summarised, it was

The terminal year is defined as the dawn after the twilight characterised by the classic by Hugh Hudson (1957) which summarised, in elegant prose and classic diagrammatic exegesis, the nonlinear, endogenous, business cycle theories that had became, for that time, the standard approach.

We will outline the idea that invoking *non-linear* models capable of *relaxation oscillations* to encapsulate economic data had to rely on reasonably reliable empirical evidence of a particular kind, historically and theoretically substantiated:

– evidence of the *persistence of fluctuations*;

– of asymmetric cycles (in the sense of time series of aggregate variable displaying *significantly non «sinusoidal» behaviour*);

– of *multiple equilibria*;

– of, at least, *local instability of equilibria*;

– of significant intrinsic non-linearities in economic relationships or behaviour in variables defining macroeconomic fluctuations.

The five desiderata, *persistence*, *asymmetry*, *multiple equilibria*, *instability* and *non-linearity* as criteria for a model of macroeconomic fluctuations implied, in turn, an endogenous cycle. The key *economic hypotheses* underpinning these ideas (multiple equilibria, instability and non-linear behavioural relations) and the *stylized facts* (persistent and asymmetrical fluctuations) underlined departures from orthodox visions of the workings of the economic system in advanced industrial economies. Thus the instability hypothesis meant that deviations from equilibria did not call forth automatic self-adjusting mechanisms of the metaphorical world of the *invisible hands*. The hypothesis of multiple equilibria implied, in conjunction with the loss of self-adjustment capabilities, that economies could, for endogenous or exogenous reasons, end up in undesirable basins of attraction, out of which the system could not, of its own accord, extricate itself and, hence, signalled an active role for policy. That, in turn, called forth a theory of macroeconomic policy to be developed within the same context[17]. Instability, multiple equilibria and a theory of policy within a framework of growth and business cycles in an advanced industrial monetary economy were themes broached by, and models for them were crafted by, four pioneering economists: Wicksell, Lindahl, Keynes and Harrod. None of them, however, fashioned an explicit mathematical model. We conjecture that none had the theoretical technology to construct meaningful unstable, multiple equilibria, models mathematically. Their deep economic insights, expressed in every one of their cases in

the late Professor Sukhamoy Chakravarty who, during a personal conversation in Cambridge in 1982, referred Velupillai to Hamburger's claims to priority in this area. Some of this information was summarised, after he passed it on to her, in the doctoral dissertation of his brilliant student, Serena Sordi.

[17] The choice between a van der Pol formalism and a Rayleigh formalism for non-linear business cycle theory had, as its economic backdrop, a precise stance on policy.

exceptionally elegant prose[18], left no doubt as to the necessity of non-linear tools to encapsulate their fertile ideas. It was left to their students and near contemporaries – in the chronological order in which their works came to be published, Erik Lundberg, Nicholas Kaldor, Richard Goodwin and John Hicks – to realise that aim.

Several authors, in the period considered, appealed to one or more of the above desiderata. However, to the best of our knowledge, only these four invoked the whole set as defining criteria for a model of macroeconomic fluctuations. Of these four, the first and the last, Erik Lundberg and John Hicks, framed their models in terms of piecewise linear relations; the second, Nicholas Kaldor, described his economic model graphically and set out the defining economic relationships algebraically in non-linear functional forms without, however, deriving the final, crucial, non-linear equation which would encapsulate the dynamics and show the nature of its underlying relaxation oscillation behaviour. This significant task, for the Kaldor economic model, was first accomplished by Takuma Yasui only in 1952-3 and it was shown, in a masterly pedagogical piece of analysis, that the Kaldor non-linear *Model of The Trade Cycle* implied a formalism in terms of the *van der Pol equation*. Only Richard Goodwin developed a formal mathematical macrodynamic model, explicitly satisfying every one of the criteria listed above, and derived the final, formal, equation – as it happened it was the *Rayleigh form for maintained oscillations* – in one fell swoop, so to speak.

These four supreme macroeconomic theorists did not invoke these desiderata arbitrarily or in an *atheoretical* vacuum. The intrinsic structure of the theoretical foundations on which each, in their own distinctive way, erected their respective business cycle models implied non-linear mathematical equations encapsulating, naturally, the five desiderata. It was not as if a non-linear equation was chosen, *a priori*, and, then, economic assumptions were tailored to fit the chosen equation; it was, instead, quite the other way about and according to the noblest Ockhamian traditions of model building and theorising. Indeed, it was precisely because these outstanding theorists went about the construction of their theoretical model of the business cycle in this traditional, noble, way that non-linear macroeconomic modelling of business cycles had many false starts, several still-born episodes and even unfortunate and unfounded dismissals, at least in the period under consideration. None of them, except Goodwin, ever managed to master the mathematical sophistication required for the understanding of the full formalism of non-linear dynamics. That Goodwin became a master – at least of some aspects of this fascinating area – was almost wholly due to the personal tutoring he received from Philippe Le Corbeiller.

[18] In Swedish of impeccable clarity and admirable directness, in the case of Wicksell and Lindahl.

Lundberg, Kaldor, Goodwin and Hicks had, each of them independently, constructed non-linear business cycle models of innovative and imaginative structure and each had their own sources of theoretical inspiration. Lundberg built on Wicksell and the contemporary work of his Swedish macroeconomic colleagues, particularly Erik Lindahl, Gunnar Myrdal and Dag Hammarskjöld; Kaldor subtly synthesised the works of Keynes, Harrod and Kalecki; Goodwin combined, with outstanding innovative imagination, elements of Schumpeter, Keynes and Harrod; Hicks, in his own, characteristic, low-key way, seemed to have relied on modified aspects of Keynesian and Harrodian elements to construct his piecewise linear model of the trade cycle[19]. In passing, it must be noted that modern studies on non-linear macrodynamics, particularly when it relates to business cycle theory, have had a tendency to pay justifiable *homage* to these pioneers – with the exception of Lundberg.

Thus, before concluding this section, four issues must be faced and resolved.

1) First of all, why did Hamburger's pioneering conjectures fail to elicit any response at all?

2) Secondly, why is Lundberg's impressive and highly original work *not* bracketed together with Kaldor, Goodwin and Hicks as one of the pioneers of non-linear business cycle modelling?

3) Thirdly, what of many other significant calls for the «non-linearization» of macrodynamics in general and business cycle theory in particular, of this period, and why didn't any of them – some by outstanding theorists of the profession such as Paul Samuelson and Nicholas Georgescu-Roegen – lead to serious modelling exercises, satisfying the five desiderata enumerated above?

4) Only one such «clarion call», that by Ph. Le Corbeiller, elicited any response at all, by economic theorists – why?

Hamburger's imaginative and original line of economic research was sadly terminated by the tragedy of the holocaust. Despite the valiant empirical case he tried to make to substantiate his claims that economic fluctuations should be modelled as the relaxation oscillations of a nonlinear differential equation, his work did not attract much – or, indeed, any – attention in the vibrant efforts that were being made, throughout the 30s, to model the business cycle. «Emphasizing the applicability of the principle of relaxation-oscillations to

[19] It is interesting to recall the reflections of one of the pioneers of macroeconometric model building on the theoretical sources that inspired them: «The econometric models that I have constructed as practical tools for analyzing or predicting the economies of the United States, Canada, United Kingdom, and Japan have been based on combinations from the theoretical models of Marx, Kalecki, Keynes, Lange, Hicks, Kaldor, Metzler, Goodwin, and others [...] Actually most models in existence today could be decomposed into ideas first found in the models of Kalecki, Kaldor, Metzler, and Goodwin» (Klein, 1964, p. 189). It is interesting that Metzler's name appears in both lists. The precise role of the particular contribution by Metzler to which Klein refers, in the «subverting» of the piecewise linear Lundberg model, is discussed above. The only surprise in the lists above is the absence of Harrod's name.

model economic cycles», is one thing; to actually build a formal mathematical model of aggregate fluctuations, *ab initio* from economic principles, encapsulated in the dynamics of a nonlinear (or even a linear) system of equations capable of relaxation oscillations, is quite another thing. Hamburger pointed out (*ibidem*) that his «suggestion [...] was [...] corroborated by results indicated in [his] paper[s]» in Dutch and French, published, respectively, in 1930 and 1931[20]. However, the «corroboration» is simply by way of appeal to descriptive similarities of crude statistical plots of time series pertaining to arbitrary economic variables[21]. Although it is surprising that his innovative suggestions were not taken up in serious research circles, the reasons for the failure of the modelling effort he wished to promote to take-off are equally unsurprising. Except for what may be called a tendentious preoccupation with the importance of relaxation oscillations, Hamburger provided no unifying economic theoretic modelling principle within which a theory of the business cycle could be embedded and at least a few of the desired criteria satisfied[22].

The full details of Lundberg's model of the inventory cycle cannot be discussed here[23]. All we shall do here is to report the main conclusion. Lundberg's construction was of a linear, unstable model of inventory cycles, made to generate bounded fluctuations by building in natural, economic, constraints that would act as bounds on unlimited expansion and catastrophic contractions. In effect, the formal model was in terms of a piecewise linear

[20] In Hamburger (1930; 1931). The van der Pol equation does appear in both of these papers (as equation 7, on p. 5, in the former and in footnote 7, p. 6 in the latter) in the form:

$$\frac{d^2y}{dt^2} - \alpha(1 - y^2)\frac{dy}{dt} + \omega^2 y = 0$$

Figures 1 to 3 (in both papers) show the increasing loss of (nearly) sinusoidal behaviour of the time variation of y for increasing values of α (0.1, 1.0, 10), presumably for a given value of ω (unspecified in the papers). The equation and the simulations are supplemented by a couple of pages of a discursive discussion on the meaning of relaxation oscillations in the abstract.

[21] For example, figure 4 plotting the monthly variation in sales in so-called «Five- and ten-cent chain stores» in the US, for the five years from 1921 to 1925, does show a remarkable consistency with a possible underlying relaxation mechanism. The hard work is to go from suggestive statistics to the underlying model and that does not seem to have exercised Hamburger's considerably fertile mind. I have devoted more space than warranted on the marginalised work of Hamburger simply because I feel his untimely demise may have deprived the economic profession of an unusual talent that may have helped speed up the introduction of nonlinear mathematical modelling to the art of business cycle theorising much sooner than happened in his absence. The only reference in the mainstream economic literature to anything by Hamburger is the one by Tinbergen in his famous *Survey* (Tinbergen, 1935, p. 288, footnote 71).

[22] For the same reason we have not gone into details of the contributions by Marrama and Palomba to the nonlinear macrodynamic tradition. Our friend, Professor Giancarlo Gandolfo's sterling effort on this front may be referred to, for the interested reader (for example Gandolfo, 2010).

[23] Readers wishing to get a partial idea of what is meant here could profitably read Berg (1991) and Baumol (1991).

difference equations. Lloyd Metzler endogenised the bounds and converted the model into a completely linear system. Why did he do it? We had to wait thirty years to get a straight, candid, answer – as always with characteristic directness from Paul Samuelson:

In leaving Frisch's work of the 1930's on stochastic difference, differential and other functional equations, let me point out that a great man's work can, in its impact on lesser men, have bad as well as good effects. Thus, by 1940, Metzler and I as graduate students at Harvard fell into the dogma – I use the word «dogma» in the non-perjorative sense of Crick's dogma on DNA and RNA, as a leading hypothesis – that **all economic business-cycle models** should have *damped* roots [...][W]hat was so bad about the dogma? Well, **it slowed down our recognition of the importance of non-linear autorelaxation models of the van der Pol-Rayleigh type**, with their characteristic amplitude features lacked by linear systems (Samuelson, 1974, p. 10; bold emphasis added).

Lundberg's non-linear, unstable, model of the inventory cycle was, after its unfortunate transmogrification by Metzler, forever cast into the linear mould, until recent, sporadic, revivalist attempts, with hardly a ripple in mainstream thought or practice.

In 1933, in the very first volume of *Econometrica*, Philippe Le Corbeiller had written, suggestively and challengingly:

Le problème des crises, et plus généralement des oscillations des prix, est assurément l'un des plus difficiles de l'Économie Politique; il ne sera sans doute pas de trop, pour approcher de sa solution, de la mise en commun de *toutes les ressources de la théorie des oscillations* et de la théorie économique. C'est pourquoi j'ai pensé pouvoir vous présenter un compte-rendu succinct d'un avance récente, que je crois importante, de la théorie des oscillations: celle apportée au problème des *systèmes autoentretenus par la découverte des oscillations de relaxation*, due à un savant hollandais, le Dr. Balth. van der Pol (Le Corbeiller, 1933, pp. 328-9; italics added).

The suggestion was not one of those famed «bolts from the blue». First of all, by the time it came to be published, it had been in the hands of, Ragnar Frisch, the Editor of *Econometrica*, for over an year[24]. Secondly, there is

[24] Unfortunately, the University of Oslo library where, at present most of the Frisch Archives are deposited, do not allow copying of personal letters without the written permission from descendents on both sides of a correspondence! Many of the letters between Le Corbeiller and Frisch, particularly from the former, are in handwriting that is indecipherable without expert help. On 12 July 1932 Frisch wrote as follows to Le Corbeiller (typewritten): «My dear Professor Le Corbeiller, Your manuscript "Les systèmes autoentretenus..." has been referred to me as Editor of the newly established journal *Econometrica*, the journal of the Econometric Society. If this paper has not been published elsewhere and if you do not plan to have it published elsewhere, I shall be glad to accept it for publication in an early issue of *Econometrica*. Please drop me a line about this at your earliest opportunity. Sincerely yours, Ragnar Frisch». Le Corbeiller replied, with a handwritten note, from Paris, three days later,

ample evidence, even at those very early stages in the development of the analytic apparatus of (non-linear)[25] *relaxation oscillations*, that Le Corbeiller was deeply interested in, and committed to, an investigation of diverse phenomena in the natural and physical world that were amenable to an interpretation in terms of a non-linear formalization emphasising this aspect in its dynamics[26]. Thirdly, here we are conjecturing without hard evidence, it is more than likely that his lifelong intimacy and friendship with van der Pol had already begun in the late 20s. He may, therefore, have been aware of Hamburger's remarks on the van der Pol - van der Mark paper, via personal discussions or communications from van der Pol himself. We believe a little more research effort may close this minor gap and help present a complete picture of the background to Le Corbeiller's fascinating and suggestive paper. There is no mention of possible interpretations of economic fluctuations as relaxation oscillations in his 1931 monograph, the contents of which were given as seminars in May, 1931. Frisch had received[27] a copy of the first draft by July, 1932. Sometime in the 14-month period between these two dates, Le Corbeiller had conceived and written this pioneering paper. The source of the inspiration remains to be discovered.

To the best of our knowledge, there are only three explicit references to Le Corbeiller's call for a non-linear, relaxation oscillation, approach to the modelling of economic fluctuations: Paul Samuelson in his path-breaking monograph, *Foundations of Economic Analysis* (1947); Georgescu-Roegen in one of his contributions in the *Cowles Foundation Monograph* on *Activity Analysis of Production & Allocation* (1951) and, finally, Richard Goodwin (1951). It was only this latter work that directly took up the challenge posed by Le Corbeiller and codified into a usable formalization, within standard macroeconomic theory, a model of the business cycle in a theoretically sound and empirically implementable way.

Paul Samuelson simply catalogued some possibilities for mathematically modelling endogenous business cycles using non-linear differential and dif-

expressing his gratitude for the honour Frisch was bestowing upon him with the proposal to publish his piece.

[25] Lest the unwary reader think we are being facetious with the qualifying «non-linear», we must point out that, in economics, an early attempt at applying the ideas underlying relaxation methods emphasised linearity. We shall deal with this later, in this paper.

[26] This is eminently clear in his elegant booklet of 1931, based on Seminars given at the *Conservatoire National des Arts et Métiers* on 6-7, May, 1931. In particular, the concluding section, sub-titled *Aperçu historique et conclusion* (Le Corbeiller, 1931, pp. 43-5), although the whole work reflects the mind of a scientist with an admirably broad vision of natural and physical phenomena. It will not come as a surprise to anyone familiar with this beautiful little exposition that this fertile mind saw the possibility of a fruitful interpretation of fluctuating economic phenomena in terms of non-linear relaxation oscillation mechanisms as the underlying cause. The significant step of identifying these mechanisms in terms of meaningful and incontrovertible economic factors had to wait another decade and a half, much due to the personal efforts of Le Corbeiller himself, albeit indirectly.

[27] Although through which channels is still a mystery.

ference equations, in a brief section of two and a half pages, in his monumentally influential book of 447 pages. Perhaps the very fact that a voice as mathematically competent as Samuelson's, expressing that a non-linear, relaxation oscillation, approach to mathematical modelling of business cycles entails «formal difficulties of solution [...] so great that very much remains to be done» (Samuelson, 1947, p. 340), immediately after a reference to Le Corbeiller's paper, may have diverted the profession's attention away from the potential gains that may have been available with a little effort. Apart from this brief and wholly discouraging reference to Le Corbeiller, there are discursive remarks on general properties of non-linear dynamical systems, with explicit references to van der Pol's equation, without, however, any indication or attempt at encapsulating meaningful economic hypotheses in a mathematical formalism that may have resulted in such an equation.

Georgescu-Roegen opens his illuminating and interesting paper with an explicit reference to Le Corbeiller's pioneering role in emphasising the relevance of «relaxation phenomena as a model for business cycles» (Georgescu-Roegen, 1951, p. 116). He, then, goes on:

> However, Le Corbeiller's suggestion has found little echo among economists, and the literature shows only sporadic references to his paper. Paul A. Samuelson [...], speaking of this possible approach, admits that practically nothing has been done along this line. The only economic problem which could be regarded as having something to do with relaxation is the famous cobweb problem, but this has been developed independently of any relation to the concept of relaxation (*ibidem*, p. 116).

Georgescu-Roegen's attempt at introducing relaxation phenomena in economic dynamics took the unusual form of emphasising the discontinuity residing in them by highlighting the fact there were two time-phased regimes encapsulated in the system. He, then, interpreted all attempts at encapsulating the discontinuity within one functional equation, such as van der Pol's, as «veiling the real meaning of relaxation, which is the discontinuity of the regime». He went on, therefore, to consider the two regimes formalised as two separate systems of linear differential equations. There was, therefore, no scope for taking seriously the full message of Le Corbeiller's challenge and, indeed, like Samuelson's reference to it, had the unfortunate consequence of diverting the attention of the business cycle theorist away from it.

The first formal attempt at a fully developed *non-linear relaxation oscillation* mathematical model of the *The Business Cycle as a Self-Sustained Oscillation*[28] was presented by Richard Goodwin at the Cleveland Meetings of the

[28] In view of the fact that Goodwin, in his own celebrated non-linear model of the business cycle, emphasised the Rayleigh rather than the van der Pol equation, it may be of interest to recall the title of the pioneering paper by Lord Rayleigh in which that system was developed: *On Maintained Vibrations* (Strutt, 1883). It was, perhaps, not a coincidence that, forty

Econometric Society, on 30 December, 1948 (Goodwin, 1949). The full paper was published subsequently in the same Journal as the lead article in the first issue of 1951 (Goodwin, 1951). The mathematical model of the business cycle presented in this paper was the first fully-fledged formalization of the phenomenon that satisfied all the five criteria discussed above: *persistence*, *asymmetry*, *multiple equilibria*, *instability* and *non-linearity*. Le Corbeiller's role in the development of the work that enabled Goodwin to produce this pioneering paper is evident in the footnote to the lead quote of this section (above):

My debt to Professor Le Corbeiller is very great, not only for the original stimulation to search for the essential nonlinearities, but also for his patient insistence, in the face of the many difficulties which turned up, that this type of analysis *must* somehow be worked out (*ibidem*, p. 2; italics in original).

4. Concluding Notes: Towards Consolidation, Decline and Renewal

> Certainly we do not want a theory of the cycle which clamps the facts into a vice; but this theory [which Frisch has called the theory of *erratic shocks*[29]] [...] does not explain *enough.*
>
> (Hicks, 1950, pp. 90-1; italics in the original)

In no uncertain terms, based on lucid economic and mathematical reasoning, Hicks pointed out (1950, p. 91) that:

[T]he theory of damped fluctuations and erratic shocks proves unacceptable; but if we reject it, what is the alternative? There is an alternative...

The «alternative» is, of course, the subject matter of this paper: *non-linear theory*.

The first «Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel»[30] was shared by Tinbergen and Frisch in 1969. The citation for

three years later, van der Pol's classic paper, *On Relaxation Oscillations*, was also published in the same Journal (van der Pol, 1926). Incidentally, Marshall was Second Wrangler to Lord Rayleigh in 1865 and, for those numerologists interested in coincidences, 1883 was, of course the year Keynes and Schumpeter were born and Marx died! The non-linear business cycle theories in discussion in this paper rely, to some extent, on the economic theories of Marx, Keynes and Schumpeter. Some substantiation for this statement can be found in the first footnote in Goodwin (1951) and the last sentence of the second footnote of the same paper.

[29] Or, in Richard Day's more felicitous, if slightly less complementary, phrasing: the theory of *ad-hoc shockeries* (cf. Day, 1992, p. 180).

[30] Sometimes, misleadingly, referred to as the *Nobel Prize in Economics* and placed, incorrectly, on a par with the those awarded for Peace, Literature, Physics, Chemistry and Medicine and Physiology. Surely, it would have been more appropriate for the Bank of Sweden to follow the practice of the Mathematicians and award the equivalent of a Fields Medal – say, calling it a *Wicksell Medal* – to honour and celebrate excellence in economics!

Frisch stated that he was awarded the Prize «for having developed and applied *dynamic models for the analysis of economic processes*». Thirty five and forty two years later, we read that the 2004 Prize was to be shared by Prescott with another Norwegian, Finn Kydland, and the 2011 Prize was to be shared by Sargent and Sims. The former were awarded it «for their contributions to *dynamic macroeconomics*: the time consistency of economic policy and *the driving forces behind business cycles*». The latter award was for ostensibly different contributions – although by simply changing «for their contributions to *recursive* macroeconomics», rather than «*dynamic* macroeconomics», nothing else would have changed. The metaphor of the *rocking horse* was the cementing concept that unified the mathematical methodologies underpinning Frisch's «dynamic models for the analysis of economic processes» and the Kydland-Prescott real business cycle models of «dynamic macroeconomics» (and the underpinning for Sargent's so-called «recursive macroeconomics»). That much maligned metaphor was incorrectly attributed, by Frisch (cf. Frisch, 1933, p. 178, footnote 5) to Wicksell's famous lecture in Oslo, to the Statsøkonomisk Førening, on May 6, 1907 (cf. Wicksell, 1953). No amount of fine-toothed combing of that fine lecture will unearth any reference to a rocking (or, more appropriately, an unrocking[31]) horse. Wicksell invoked the metaphor of the rocking horse in a review of an obscure and best-forgotten book titled *Goda och Dåliga Tider*[32] by a long-forgotten minor Swedish economist by the name of Karl Petander (cf. Wicksell, 1918, p. 71, footnote 1)[33].

Hicks was, of course, not alone in finding the «the theory of damped fluctuations and erratic shocks unacceptable»; the names we have invoked in the pages of this paper are a testimony to that fact.

But is it necessary to choose between such starkly different alternatives – between a linear stochastic theory and a non-linear deterministic theory? It was not in Hicks' nature, nor in the nature of Schumpeter, Keynes,

[31] Zambelli (1992) has shown, unambiguously and convincingly, that Frisch's «rocking horse» does not «rock». It is a pity that Zambelli's exceptionally careful and detailed analysis of the untenability of the numerical underpinnings of Frisch's economic assumptions, such as implausible initial conditions and unsustainable historical trajectories, have received hardly any attention in the macrodynamic profession. It is nothing less than a minor scandal that a prestigious Prize is awarded to work that is, to put it mildly, less than careful in its historical foundations, cavalier in its numerical methodology and vacuous in its logical foundations – and I am not referring to Frisch in this case.

[32] «Good and Bad Times».

[33] Frisch translated only the first of the two sentences in this footnote which referred to the now famous rocking-horse metaphor. Just for the record, the full Swedish statement in this Wicksellian footnote is as follows (Wicksell, 1918; italics in the original): «Om man slår på en gunghäst med en klubba, så bli gunghästens rörelser mycket olika klubbans. Stöten är *orsaken* till rörelsen, men föremålets egna jämiktsbetingelser äro *förutsättningarna* för rörelsens *form*». [The impulse is the *reason* for the movement, but the object's own equilibrium tendencies {structure} are the *prerequisites* for the form of the movement (KVV's translation from the original Swedish, of the second sentence)].

Lindahl, Lundberg, Tinbergen, Leontief, Kalecki, Kaldor, Goodwin, Yasui, Morishima or Day, to depict possible worlds in starkly contrasting colours; their's was a world of shades and many colours and this was so even in their theories of the trade cycle. Even though Hicks opted for the alternative of theorising without reliance on *ad-hoc shockeries*, he did add the characteristic caveat (Hicks, 1950, p. 90):

> It [the theory of *erratic shocks*] certainly is an interesting theory; it is quite likely that a «stochastic» hypothesis of this sort has some part to play in the explanation of what happens. But this particular hypothesis will not do.

There was a time when the theoretical technology of computing mitigated against the use of non-linear dynamical systems to model macroeconomic fluctuations in excess of two or three dimensions. However, advances in the technology of feasible, large-scale computations and simulations of high-dimensional non-linear dynamical systems suggests new approaches to the modelling of macroeconomic fluctuations. Moreover, it is also possible, with the new developments in theory and technology at hand, to use modelling techniques and strategies that *go beyond* the traditional reliance on difference, differential and mixed difference-differential systems, whether deterministic or stochastic, whether linear or nonlinear. Indeed, even the traditional and worn dichotomy between deterministic and stochastic systems can be questioned from the point of view of newer mathematical modelling possibilities brought to the fore by concepts of incompleteness, uncomputability and undecidability[34].

The rest of this concluding section, apart from summarising very briefly the way nonlinear, endogenous, mathematical theorising of the phenomenon of aggregate fluctuations proceeded, is also a mini-manifesto of hope.

4.1. *The Interregnum: 1958-1970*

We have called this period an *Interregnum*. This is an era that seemed to have reached a nadir in the nonlinear, endogenous, mathematical theory of the business cycle, with the provocative and perennially falsified thought that the business cycle was «obsolete». A conference convened by the *Social Science Research Committee on Economic Stability*, with distinguished business cycle theorists in attendance – E. Lundberg, R. Matthews, L. Klein, B. Hickman, R.A. Gordon, P.J. Verdoorn and many others – with the main theme being: *Is the Business Cycle Obsolete?* (Bronfenbrenner, 1969). The closing

[34] Note that we carefully avoid mentioning the fashionable – although «fashion» has a way of making obsolete even current «buzz» words faster than adherents to them can imagine – notion of deterministic randomness, deterministic chaos, and so on – at least here.

year of the period is significant in that it was also approximately midway between the year of Friedman's famous AEA address that ushered in the natural rate of unemployment as an essential ingredient in macroeconomic thinking and modelling and the birth of newclassical macroeconomics at the hands of Lucas (1972)[35]. Apart from sporadic contributions to business cycle theory – mostly in the linear mode – the significance of the period for the story being told here is that 1967 marked the year that Goodwin's remarkable *A Growth Cycle* was published, in the *Dobb Festschrift* (Goodwin, 1967) and a new impetus that was given to the worn out mantle of *IS-LM* by Hugh Rose in an influential and inspired series of contributions that integrated the non-linear Phillips curve within the fold of the dying embers of the Neoclassical Synthesis and helped revive it, at least for a few years[36]. The former introduced, into mainstream macrodynamic modelling, the famous *Lotka-Volterra equations* and with it a wholly different set of issues from non-linear dynamical systems theory – even while that theory was itself undergoing, literally, cataclysmic changes with the publication of Steve Smale's famous survey paper on *Differential Dynamical Systems* (Smale, 1967). The latter – i.e., the contributions of Hugh Rose – introduced into the toolbox of the macrodynamic student, once and forever, the powerful *Poincaré-Bendixson theorem*. In the early years of the *Interregnum*, crossing over and overlapping with the period of *Excitement at Birth*, there was a sudden burst of activity, probably inspired by the powerful contributions by Yasui (1953), in the late 1940s and early 1950s, by Japanese economists. Kurihara (1955), Ichimura (1955) and Morishima (1958), surveyed and pushed the frontiers of non-linear Keynesian macroeconomics in interesting directions. Indeed, few realise that Morishima's doctoral dissertation was on *Non-Linear Macrodynamics*. There is also another important contribution to the main theme of this section: Hugh Hudson's little acknowledged but hugely important pedagogical effort at making non-linear trade cycle theory comprehensible to the general macroeconomic community (Hudson, 1957) – which, by and large, did an admirable job of completely bypassing it in the manner of Robertson's «Scottish Preacher» (Robertson, 1952, p. 70). Finally, the re-formalisation

[35] Indeed, the «closing year» – 1970 – was the year Lucas submitted his famous paper to the *Journal of Economic Theory*, received by the Journal on September 4, 1970. However, it had been subject to «a withering rejection from the journal to which it was first submitted» (Lucas, 1981, p. 10), which must date its completion in draft form a little earlier. It is clear from the Introduction to Lucas (1981) that the foundations of Newclassical economics lay in the concepts – natural rate, neutrality, rational expectations, etc. – and metaphors – the island paradigm, for example – conceived by Muth, Friedman and Phelps during the previous decade.

[36] The most illuminating and comprehensive of a series of three papers (Rose, 1966; 1967; 1969) by Hugh Rose was the 1967, *RES* contribution. Rose had been a pupil of Hicks; so it is entirely natural that his fundamental contributions arose from considering the neglected «monetary chapters», XI and XII (as did Hudson, a decade earlier (Hudson, 1957) – see below), in the Hicksian classic (Hicks, 1950).

of Kaldor's model, in formally more precise ways than in Yasui's early paper that was referred to above, was expertly attempted by Chang and Smyth (Chang - Smyth, 1971). This paper had a significant influence in inspiring some interesting work on non-linear Keynesian models of the business cycle and further helped in making the economist more familiar with the mathematics of *planar dynamical systems*. We may add that it also imprisoned the mathematically inclined business cycle theorist within the straitjacket of two-dimensional modelling. A large part of the story, both adventurous and monotonous, was due to the dominance of planar dynamic modelling. That it was a necessity in the early years cannot be denied; that it was a straitjacket in a later period is something to be established by argument.

But the story of this part, as befits the meaning of the word *Interregnum*[37], will be about an afterglow and a setting of the scene for a new thrust. An afterglow after the excitements of birth and early growth of a nascent discipline and the expectations of continued progress in understanding and taming the more virulent aspects of cyclical fluctuations. With hindsight, it will also be a story of the scene that was being set for the new developments in non-linear dynamical systems theory to be embraced by macroeconomic theories that were going beyond and away from Keynesian paradigms and freeing themselves from the somnambulance of the Neo-Classical Synthesis.

4.2. *Hopes Betrayed: 1970-1987*

The dawn of this period saw the challenge posed by Clower to the Neoclassical Synthesis, even while the capital, growth and distribution controversies were going on at another end of the macroeconomic spectrum. Meanwhile Friedman was mounting a sustained and increasingly plausible attempt at reviving Monetarism to place it as the centerpiece not just of macroeconomics traditionally conceived, but also as a basis for business cycle theories. Out of these challenging developments at the core of macroeconomic theory emerged, at first with great promise and much excitement, varieties of *Fix-Price Macroeconomics*[38]. There were two immediate fountainheads for these theories: the challenge to the Neoclassical Synthesis posed, on the one hand, by Clower from a Keynesian perspective; and, from another end, by Barro and Grossman. The former line of macroeconomics was further codified by Malinvaud's famous *Yrjö Jahnsson Lectures* (Malinvaud, 1977) and added a new impetus to non-linear modelling of economic fluctuations. New tools of non-linear dynamics, particularly René Thom's *Catastrophe Theory* and

[37] The *OED* definition, #4, is: «*A breach of continuity; an interval, pause, vacant space*». The other three definitions are almost equally applicable, for the sense we have in mind.

[38] However, the fix-flex price divide in macroeconomics had first been broached by Hicks much earlier, in his comparison of aggregate accounting by Lindahl and Keynes, in a severely neglected masterpiece in the *Lindahl Festschrift* (Hicks, 1956).

Christopher Zeeman's work at the University of Warwick in the same tradition, came to dominate that version of macroeconomic fluctuations emanating from the *French* version of *Fix-Price Macroeconomics.* Perhaps the most comprehensive study along these lines (Blad, 1969), summarised the economics and the non-linear mathematics of catastrophe theory and was used to formalize regime changes as phases in economic dynamics. They were, then, interpreted as macroeconomic fluctuations (Michael Blad's doctoral dissertation at Warwick University (Blad, *ibid.*), out of which he was to spawn some influential articles on «new» methodologies for modelling nonlinear, fluctuating, phenomena, Blad (1981) and Blad - Zeeman (1982), exemplify this work). At the level of graduate pedagogy, with a specific application of the mathematical methods introduced by Rose (for example in Rose (1967)) in the framework of the macroeconomics of the neoclassical synthesis, there were important contributions by, for example Benassy (1986).

The quintessential nonlinear, endogenous, business cycle contribution, which was to lead to a flurry of activity in the application of modern dynamical systems theory in a variety of Non-Newclassical macrodynamic models – New Keynesian Economics is what it transmogrified into, but that was to be in a future where the nonlinear mathematical underpinnings were diluted – was of course the classic by Grandmont (1985).

Almost all of these developments that emerged out of the ruins of the Neoclassical Synthesis were, initially, theories of *Disequilibrium Macrodynamics*[39]. The tide, however, was turning against this paradigm as the defining theme for macroeconomics and the early years of the period, particularly after the *Phelps Volume* (Phelps, 1970), saw a revival of the equilibrium approach to macroeconomics in general reasserting itself. The 1970s saw the codification of Lucasian Macroeconomics, re-named *Newclassical Macroeconomics*, built on fusing of eight fundamental concepts in a remarkable *tour de force* of model building by Lucas:

– the *natural rate of unemployment* (from Friedman and Phelps);

– the *rational expectations hypothesis* (from Muth);

– endogenising labour supply via the *search model* (from Stigler and McCall);

– exploiting the local-global divide to formalise misperceptions in a monetary economy subject to shocks by situating the rational agent in *Phelpsian Islands*;

– reintroducing *Human capital* as an additional factor of production in aggregate production functions;

– incorporating all these elements in an *overlapping generations model* (from Samuelson);

[39] To be distinguished from *current* work on *Keynesian Disequilibrium Macrodynamics*, most systematically and competently developed and pursued by Carl Chiarella, Peter Flaschel and their collaborators (cf., for example, Chiarella - Flaschel, 2000; Chiarella *et al.*, 2005; Flaschel *et al.*, 1997; Asada *et al.*, 2010).

– reinterpreting business cycles as *equilibrium phenomena* (claiming allegiance to Hayek's thesis of the early 30s);

– and utilising developments in linear filtering theory to reinterpret the rational agent as a *signal processor* (from Kalman and Wesley Clare Mitchell, as explicitly acknowledged by Lucas).

By the end of this era of *Hopes Betrayed*, Newclassical economics *was* Macroeconomics and at least so far as business cycle theory was concerned, non-linear, disequilibrium theories of macroeconomic fluctuations had been banished to the hinterlands. Kydland and Prescott published, in 1982, their celebrated paper that defined the dominant research paradigm for business cycle theory for the whole of the period after that, *Real Business Cycle Theory* (Kydland - Prescott, 1982), Lucas and Romer «endogenised» growth theory (Lucas, 1988; Romer, 1986), Lucas gave up on his original monetary misperception theory of the business cycle, Kydland and Precott nailed the coffin that bore the remains of the fix price macrodynamic visions with their own policy nihilistic codification via [Kydland - Prescott, 1977], the first of Sargent's hugely successful series of Newclassical textbooks appeared (Sargent, 1987), *DSGE* modelling became the paradigm and with it six decades of adventures with non-linear dynamics in business cycle modelling came to an end – *or so it seemed*.

Mercifully – or is it better to say, fortunately – not all was lost and not all was as it seemed or appeared. There had been *momentous* – the word is chosen carefully – developments in the theory of non-linear dynamics. *Chaos* and, more generally, *sensitive dependence on initial conditions* had been *rediscovered* and the Poincaré-Birkhoff tradition in non-linear dynamical systems theory was about to explode into a frenzy of research activity, much facilitated by the new power brought into that branch of work by the availability of cheap computing resources. Lorenz, Takens, Ruelle, May, Feigenbaum, Smale, Abraham, Arnold and others had taken non-linear dynamics into new frontiers, beyond where it had been left off by the giants of the first half of the 20th century: Poincaré, above all; but also van der Pol, the Russian school fostered and nurtured by the great Andronov; the Latin American schools inspired by Peixoto and Lefshetz, in Mexico, Brazil, Argentina and Uruguay; Littlewood and Cartwright; Levinson, Minorsky and Lefshetz (now, in his US roles) and, of course, many others in Continental Western Europe. While all this was going on, two significant papers were published in core economic journals that pointed the way towards the usefulness of these new developments in non-linear dynamical systems theory for the modelling of macroeconomic fluctuations. First of all, there was the remarkably elegant and almost deceptively simple paper by David Gale (1973); and, then, building on this, a series of papers by Richard Day, beginning with (a joint work with Jess Benhabib) Day - Benhabib (1982).

The period was dominated by the emergence of the Newclassical approach to business cycle modelling; but it ended with a hope for the revival

of non-linear business cycle modelling due, primarily, to external factors. The external factors were something entirely new in the adventures of non-linear mathematical business cycle modelling: the power, facility and feasibility of studying non-linear systems by simulation due to the cheap and easy availability of computers, literally at one's fingertips, and the increasingly well documented and competently prepared software for studying and simulating complex non-linear equations.

In the excitement that was brewing for the dawn of the next period all and sundry forgot that much had been written and claimed for *chaos* and its existence; but little had been done about developing a *theory of chaos*.

4.3. *Adventures in Poincaré's Paradise: 1988-2003*

In the early years of this period, a leading non-linear theorist remarked:

> The chaotic attractor of *mathematical theory* began with Birkhoff in 1916. The chaotic attractor of *simulation experiment* arrived with Lorenz in 1962 [...] The identification of these two objects has not yet succeeded, despite many attempts during the past twenty years. Of course, everyone (including myself) expects this to happen soon (Abraham, 1985, p. 117; italics added).

The ferment and the plethora of articles, books and manuscripts on non-linear economic dynamics describing complex behaviour paid little or no attention to the above dichotomy. This sense of careless excitement was compounded by a habitual disregard, in economic modelling, for the need to understand three interrelated issues:

– the digital computer, with floating-point precision, needs to be fed discrete dynamical systems; hence, if economic modelling has been done in continuous time, then such systems have to be discretized in a way that preserves the characteristics of its attractor (supposing there to be one for the system);

– the non-linear dynamical system, when implemented in a digital computer, takes on the characteristics of a recursive function that is iterated, or that of a Turing Machine that is initialised to implement a computation; hence, the theory of computation acts to constrain the feasible trajectories and the characteristics of the basin of attraction of the dynamical system;

– in view of the above two points, any study – theoretically or experimentally – of a non-linear dynamical system cannot be complete without a correspondence with a theory of numerical analysis and recursion theory (the theory of computation).

In describing the work on mathematical business cycle theory in the non-linear mode of this period, against the backdrop of the development in the mathematics of non-linear dynamical systems theory, the above three caveats and Ralph Abraham's cautionary note must be kept in mind.

Bifurcation theory played a crucial role in the non-linear economic models that were developed in this period. Examples are the Andronov-Hopf Bifurcations in classic Keynesian models of the business cycle and Turing bifurcations in Marxian models of distribution cycles. All kinds of macrodynamic models made this tool and concept, by the end of the period, as familiar to mathematically minded economists as the Perron-Frobenius theorem had been to linear economic model builders and economic theorists, and the Brouwer and other fix point theorems had become to general equilibrium theorists in earlier periods.

The economic workhorse, for the non-linear theorist of business cycles, turned out to be the overlapping generations model, owing a great deal to the pioneering two contributions by Gale and Day, mentioned above. This workhorse, encapsulating non-linearities in an ingenious way – exploiting, for example, the differences in attitude to risk by different generations populating the economy – served a dual purpose in what had become an intellectual battle between Newclassical visions of the economy as a self-sustaining, self-adjusting, equilibrium phenomenon and those on an obverse side challenging all or some of these characteristics. The first purpose was to demonstrate the existence of multiple equilibria and, hence, the possibility of selection via policy active measures. The second purpose was to show that even incorporating rational behaviour as the underlying disciplining criterion for a model, there was the possibility of persistency in disequilibrium configurations for long periods of time. In both of these ways, this signalled a return to the program that initiated the non-linear adventures in the mathematical modelling of business cycles, in 1928-57. It gives some substance to that famous Robertsonian wit and wisdom:

> Now, as I have often pointed out to my students, some of whom have been brought up in sporting circles, highbrow opinion is like the hunted hare; if you stand in the same place, or nearly the same place, it can be relied upon to come round to you in a circle (Robertson, 1956, p. 81).

In some sense this is the way the story of this period is told, keeping this Robertsonian precept in mind; but it applies only to a part of the story. The remarkable developments in the mathematics of non-linear dynamical systems is an undoubted advance in theory. Whether, and to what extent, there was progress in the economics of business cycle analysis, outside the Newclassical framework, to match the powerful non-linear dynamical system theories remains a moot point – or a «Robertsonian point».

4.4. *Beyond Dynamical Systems Theory – Towards Algorithmic Dynamics*

In completing the tapestry, some methodological and epistemological «reflections and ruminations» attempt to pull the diverse threads together to try to extract some precepts for future interdisciplinary research – a much maligned phrase, which deserves to be used with prudence.

We suggest a new line of approach to the mathematical modelling of business cycles with non-linear tools. The new approach pertains to the kind of stylized facts one should concentrate on, in the study of business cycles, and, concomitantly, the appropriate non-linear dynamical system that can encapsulate the data that underpins the suggested new sets of stylized facts. It will turn out that we will be suggesting that the mathematical business cycle theorist should, after 75 years of adventures with non-linear differential, difference and mixed differential-difference equations, move on to other formalisms and other adventures – but still remaining within the fold of the nonlinear theorist. Our suggestions, predominantly to facilitate modelling and simulation with *formal algorithms* so that *dynamic method* is divorced from exclusive reliance on dynamical systems theory, may forever silence critics and sceptics, such as George Stigler, who famously admonished Paul Samuelson when reviewing his majestic *Foundations of Economic Analysis*:

> Most discussions of economic dynamics, I feel, would better be entitled, «What I know about Differential and Difference Equations»; [...] 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? (Stigler, 1948, pp. 604-5).

The reason we might be able to hope, at the end of the narrative of the adventures of mathematical business cycle theorists with non-linear dynamics, is simple: one comes to respect data, its generating mechanisms, its processing tools and, therefore, the «material on which» non-linear tools will be used comes to take an equal importance with the tools themselves. But that, after all, where the story began and, as always, Robertson's wit and wisdom may have to have the last word.

But the risk is that Shelly's majestic – yet melancholy – reflection on Ozymandias may yet be the fate of the fascinating adventures in nonlinear, endogenous, macrodynamics, as it nears a century of tumultuous life:

> My name is Ozymandias, King of Kings:
> Behold all my works, ye mighty, and despair!
> Nothing beside remains. Round the decay
> Of that colossal wreck, boundless and bare
> The lone and level sands stretch far away.

References

Abraham R. (1985), Is There Chaos without Noise?, in Fischer P. - Smith W.R. (Eds.) (1985), *Chaos, Fractals and Dynamics*, Basel, Marcel Dekker, chapter 7, pp. 117-121.

Åkerman J. (1928), *Om Det Ekonomiska Livets Rytmik*, Stockholm, Nordiska Bokhandeln.

— (1932), En Konjunkturteoretisk Problemställning, *Ekonomisk Tidskrift*, vol. 34, nos. 5/6, pp. 141-156.

Asada T. - Chiarella C. - Flaschel P. - Reiner F. (2010), *Monetary Macrodynamics*, London, Routledge.

Aubin D. - Dalmedico A.D. (2002), Writing the History of Dynamical Systems and Chaos – *Longue Durée* and Revolution, Disciplines and Cultures, *Historia Mathematica*, vol. 29, no. 3, pp. 273-339.

Berg C. (1991), Lundberg, Keynes, and the Riddles of a General Theory, in Jonung L. (Ed.) (1991), *The Stockholm School of Economics Revisited*, Cambridge, Cambridge University Press, chapter 8, pp. 205-228.

Baumol W.J. (1991), On Formal Dynamics: From Lundberg to Chaos Analysis, in Jonung L. (Ed.) (1991), *The Stockholm School of Economics Revisited*, Cambridge, Cambridge University Press, chapter 7, pp. 185-198.

Benassy J.P. (1986), *Macroeconomics: An Introduction to the Non-Walrasian Approach*, London, Academic Press, Inc.

Blad M. (1969), Dynamical Models in Disequilibrium Theory, University of Warwick Doctoral Dissertation, April, *mimeo*.

— (1981), Exchange of Stability in a Disequilibrium Model, *Journal of Mathematical Economics*, vol. 8, no. 2, pp. 121-145.

Blad M. - Zeeman C. (1982), Oscillations Between Repressed Inflation and Keynesian Equilibria due to Inertia in Decision Making, *Journal of Economic Theory*, vol. 28, no. 1, pp. 165-182.

Bolibruch A.A. - Osipov Yu S. - Sinai Ya G. (Eds.) (2005), *Mathematical Events of the Twentieth Century*, Heidelberg, Springer/Phasis.

Bronfenbrenner M. (Ed.) (1969), *Is the Business Cycle Obsolete?*, Based on a Conference of the Social Science Research Council Committee on Economic Stability, New York, John Wiley & Sons.

Cartwright M. (1952), Non-Linear Vibrations: A Chapter in Mathematical History, *The Mathematical Gazette*, vol. XXXVI, no. 316, pp. 81-88.

— (1964), From Non-Linear Oscillations to Topological Dynamics, *Journal of the London Mathematical Society*, vol. 39, pp. 193-201.

— (1974), Some Points in the History of the Theory of Nonlinear Oscillations, *Bulletin of the Institute of Mathematics and its Applications*, vol. 10, pp. 329-333.

Chang W.W. - Smyth D.J. (1971), The Existence and Persistence of Cycles in a Non-Linear Model: Kaldor's 1940 Model Re-examined, *Review of Economic Studies*, vol. 38, no. 1, pp. 37-46.

Chiarella C. - Flaschel P. (2000), *The Dynamics of Keynesian Monetary Growth: Macrofoundations*, Cambridge, Cambridge University Press.

Chiarella C. - Flaschel P. - Reiner F. (2005), *Foundations for a Disequilibrium Theory of the Business Cycle: Qualitative Analysis and Quantitative Assessment*, Cambridge, Cambridge University Press.

Day R.H. (1992), Models of Business Cycles: A Review Article, *Structural Change and Economic Dynamics*, vol. 3, no. 1, pp. 177-182.

Day R.H. - Benhabib J. (1982), A Characterization of Erratic Dynamics in the Overlapping Generations Model, *Journal of Economic Dynamics and Control*, vol. 4, no. 1, pp. 37-55.
Fisher I. (1933), The Debt-Deflation Theory of Great Depressions, *Econometrica*, vol. 1, no. 3, pp. 337-357.
Flaschel P. - Reiner F. - Semmler W. (1997), *Dynamic Macroeconomics: Instability, Fluctuations and Growth in Monetary Economies*, Cambridge, Massachusetts, The MIT Press.
Frisch R. (1933), Propagation Problems and Impulse Problems in Dynamic Economics, in *Economic Essays in Honour of Gustav Cassel*, London, George Allen & Unwin Ltd., pp. 171-206; reprinted in Gordon R.A. - Klein L.R. (Eds.) (1965), *Readings in Business Cycles*, London, George Allen & Unwin, chapter 9, pp. 155-185.
Gale D. (1973), Pure Exchange Equilibrium of Dynamic Economic Models, *Journal of Economic Theory*, vol. 6, no. 1, pp. 12-36.
Gandolfo G. (2010), Quando il ciclo economico diventò nonlineare, *Lettera Matematica Pristem*, nos. 74-75, pp. 81-6.
Georgescu-Roegen, N. (1951), Relaxation Phenomena in Linear Dynamic Models, in Koopmans T.C. (Ed.) (1951), *Activity Analysis of Production and Allocation: Proceedings of a Conference*, New Haven, Yale University Press, chapter V, pp. 116-131.
Goodwin R.M. (1949), The Business Cycle as a Self-Sustaining Oscillation, *Econometrica*, vol. 17, no. 2, pp. 184-85.
— (1950), A Non-Linear Theory of the Cycle, *The Review of Economics and Statistics*, vol. XXXII, no. 4, pp. 316-20.
— (1951), The Nonlinear Accelerator and the Persistence of Business Cycles, *Econometrica*, vol. 19, no. 1, pp. 1-17.
— (1967), A Growth Cycle, in Feinstein C. (Ed.) (1967), *Socialism, Capitalism and Economic Growth: Essays Presented to Maurice Dobb*, Cambridge, Cambridge University Press, pp. 54-58.
Grandmont J.M. (1985), On Endogenous Competitive Business Cycles, *Econometrica*, vol. 53, no. 5, pp. 995-1045.
Haberler G. (1937), *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements*, League of Nations, Geneva, Economics Intelligence Service.
Hamburger L. (1930), Een Nieuwe Weg Voor Conjunctuur-Onderzoek, Een Nieuwe Richtlijn Voor Conjunctuur-Politiek', *De Economist*, vol. LXXIX, pp. 1-38.
— (1931), *Analogie des Fluctuations économiques et des Oscillations de relaxation*, Institut de Statistique de l'Universite de Paris, Supplément aux Indices du Mouvement des Affaires, Janvier, pp. 1-35.
— (1934), Note on Economic Cycles and Relaxation Oscillations, *Econometrica*, vol. 2, p. 112.
Hansson B. (1982), *The Stockholm School and the Development of Dynamic Method*, London, Croom Helm.
Harrod R.F. (1936), *The Trade Cycle: An Essay*, Oxford, Clarendon Press.
Hawtrey R. (1931), *Trade Depression and the Way Out*, London, Longmans, Green and Co.
von Hayek F.A. (1931), *Prices and Production*, London, George Routledge & Sons, Ltd.

— (1933), *Monetary Theory and the Trade Cycle*, New York, Harcourt Brace & Co., Inc.
Heckscher E.F. (1952), A Survey of Economic Thought in Sweden, 1875-1950, *The Scandinavian Economic History Review*, vol. 1, no. 1, pp. 105-125.
Hicks J.R. (1933), Equilibrium and the Cycle [original in the *Zeitschrift für Nationalökonomie*, no. 4, 1933; translated and published in *Economic Enquiry*, November, 1980], reprinted in Hicks J. (1982), *Money, Interest & Wages: Collected Essays on Economic Theory*, Volume II, Oxford Basil Blackwell, chapter 3, pp. 28-41.
— (1950), *A Contribution to the Theory of the Trade Cycle*, Oxford, Clarendon Press.
— (1956), Methods of Dynamic Analysis, in *25 Economic Essays in English, German and Scandinavian Languages in Honour of Erik Lindahl*, Stockholm, Ekonomisk Tidskrift.
Hudson H.R. (1957), A Model of the Trade Cycle, *Economic Record*, vol. XXXIII, no. 66, pp. 378-389.
Ichimura S. (1955), Toward a General Nonlinear Macrodynamic Theory of Economic Fluctuations, in Kurihara K.K. (Ed.) (1955), *Post-Keynesian Economics*, London, George Allen & Unwin Ltd., chapter 8, pp. 192-226.
Kaldor N. (1940), A Model of the Trade Cycle, *The Economic Journal*, vol. 50, no. 197, pp. 78-92.
Kalecki, M. (1939), *Essays in the Theory of Economic Fluctuations*, London, Allen & Unwin Ltd.
Keynes J.M. (1936), *The General Theory of Employment, Interest and Money*, London, Macmillan and Co., Limited.
Klein L.R. (1964), The Role of Econometrics in Socialist Economics, in *Problems of Economic Dynamics and Planning-Essays in Honour of Michal Kalecki*, Warszawa, PWN-Polish Scientific Publishers, pp. 181-191.
Kurihara K.K. (Ed.) (1955), *Post-Keynesian Economics*, London, George Allen & Unwin Ltd.
Kuznets S. (1929), Random Events and Cyclical Fluctuations, *Journal of the American Statistical Association*, vol. 24, no. 167, pp. 258-275.
— (1930), Equilibrium Economics and Business-Cycle Theory, *Quarterly Journal of Economics*, vol. 44, no. 3, pp. 381-415.
Kydland F.E. - Prescott E.C. (1977), Rules Rather Than Discretion: The Inconsistency of Optimal Plans, *Journal of Political Economy*, vol. 85, no. 3, pp. 473-491.
— (1982), Time to Build and Aggregate Fluctuations, *Econometrica*, vol. 50, no. 6, pp. 1345-1369.
Le Corbeiller P. (1931), *Les Systèmes Autoentretenus et les Oscillations de Relaxation*, Paris, Librairie Scientifique Hermann et Cie.
— (1933), Les Systèmes Autoentretenus et les Oscillations de Relaxation, *Econometrica*, vol. 1, pp. 328-332.
Leontief W. (1934),Verzögerte Angebotsanpassung und Partielles Gleichgewicht, *Zeitschrift für Nationalökonomie*, vol. V, no. 5, pp. 670-676.
Lindahl E. (1939), *Studies in the Theory of Money and Capital*, London, George Allen & Unwin Ltd.
Lucas R.E. Jr. (1972), Expectations and the Neutrality of Money, *Journal of Economic Theory*, vol. 4, no. 2, pp. 103-124.
— (1987), *Studies in Business Cycle Theory*, Oxford, Basil Blackwell.

— (1988), The Mechanics of Economic Development, *Journal of Monetary Economics*, vol. 22, no. 1, pp. 3-42.
Lundberg E. (1937), *Studies in the Theory of Economic Expansion*, London, P.S. King & Son.
Malinvaud E. (1977), *The Theory of Unemployment Reconsidered*, Oxford, Basil Blackwell.
Mitchell W.C. (1927), *Business Cycles: The Problem and its Setting*, Studies in Business Cycles no. 1, New York, National Bureau of Economic Research.
Morishima M. (1958), A Contribution to the Nonlinear Theory of the Trade Cycle, *Zeitschrift für Nationalökonomie*, vol. XVIII, nos. 1-2, pp. 165-173.
Myrdal G. (1931), Om Penningteoretisk Jämvikt: En Studie Över den «normala räntan» i Wicksells Penninglära, *Ekonomisk Tidskrift*, vol. 33, nos. 5/6, pp. 191-302.
Phelps E.S. - Archibald G.C. - Alchian A.A. (1970), *Microeconomic Foundations of Employment and Inflation Theory*, New York, W.W. Norton & Company, Ltd.
Pigou A.C. (1927), *Industrial Fluctuations*, London, Macmillan And Co., Limited.
Robertson D.H. (1952), *Utility and All That and Other Essays*, London, George Allen & Unwin Ltd.
— (1956), Thoughts of Meeting some Important Persons, in Robertson D.H. (1956), *Economic Commentaries*, London, Staples Press Ltd., chapter IV, pp. 71-81.
Romer P.M. (1986), Increasing Returns and Long-Run Growth, *Journal of Political Economy*, vol. 94, no. 5, pp. 1002-1037.
Rose H. (1966), Unemployment in a Theory of Growth, *International Economic Review*, vol. 7, no. 3, pp. 260-282.
— (1967), On the Non-Linear Theory of the Employment Cycle, *Review of Economic Studies*, vol. 34, pp. 153-173.
— (1969), Real and Monetary Factors in the Business Cycle, *Journal of Money, Credit and Banking*, vol. 1, no. 2, pp. 138-152.
Samuelson P.A. (1971), Maximum Principles in Analytical Economics, in *Les prix Nobel en 1970*, Stockholm, The Nobel Foundation, pp. 273-288.
— (1947), *Foundations of Economic Analysis*, Cambridge, Mass., Harvard University Press.
— (1974), Remembrances of Frisch, *European Economic Review*, vol. 5, no. 1, pp. 7-22.
Sargent T.J. (1987), *Dynamic Macroeconomic Theory*, Cambridge, Mass., Harvard University Press.
Schinasi G.J. (1981), A Nonlinear Dynamic Model of Short Run Fluctuations, *The Review of Economic Studies*, vol. 48, no. 4, pp. 649-656.
Schumpeter J.A. (1939), *Business Cycles: A Theoretical, Historical, and Statistical Analysis of the Capitalist Process*, Vol. I and Vol. II, New York, McGraw-Hill Books Co.
— (1954), *History of Economic Analysis. Edited from Manuscript by Elizabeth Boody Schumpeter*, London, George Allen & Unwin Ltd.
Smale S. (1967), Differential Dynamical Systems, *Bulletin of the American Mathematical Society*, vol. 73, pp. 747-817.
Sraffa P. (1932), Dr. Hayek on Money and Capital, *The Economic Journal*, vol. 42, no. 165, pp. 42-53.
Stigler G.J. (1948), Review (of Foundations of Economic Analysis by Paul Samuelson), *Journal of the American Statistical Association*, vol. 43, no. 244, pp. 603-605.

Strutt J.W. [Lord Rayleigh] (1883), On Maintained Vibrations, *Philosophical Magazine*, series 5, vol. 15, pp. 229-235.
Tinbergen J. (1931), Ein Schiffbauzyklus, *Weltwirtschaftliches Archiv*., vol. 34, no. 1, pp. 152-164.
— (1935), Annual Survey: Suggestions on a Quantitative Business Cycle Theory, *Econometrica*, vol. 3, no. 3, pp. 241-308.
— (1937), Review of The Trade Cycle by Roy Harrod, *Weltwirtscahtliches Archiv*., vol. 45, pp. 89-91.
— (1939), *Statistical Testing of Business-Cycle Theories, Volume I, A Method and its Application to Investment Activity*, Geneva, League of Nations, Economic Intelligence Service.
— (1939a), *Statistical Testing of Business-Cycle Theories, Volume II, Business Cycles in the United States of America, 1919-1932*, Geneva, League of Nations, Economic Intelligence Service.
van der Pol B. (1926), On Relaxation Oscillations, *Philosophical Magazine*, series 7, vol. 2.
— (1934), The Nonlinear Theory of Electric Oscillations, *Proceedings of the Institute of Radio Engineers*, vol. 22, no. 9, pp. 1051-1087.
Velupillai K.V. (2008), The Mathematization of Macroeconomics, *Economia Politica*, vol. XXV, no. 3, pp. 283-316.
— (2009), Macroeconomics. A Clarifying Note, *Economia Politica*, vol. XXVI, no. 1, pp. 135-137.
von Karman T. (1940), The Engineer Grapples with Nonlinear Problems, [The Fifteenth Josiah Willard Gibbs Lecture, delivered at Columbus, Ohio, December 27, 1939], *Bulletin of the American Mathematical Society*, vol. 46, pp. 615-683.
Wicksell K. (1907), Krisernas Gata, Stats-konomisk Tidskrift, 1907, pp. 255-284; traslated by Uhr C.G. (1953), The Enigma of Business Cycles, *International Economic Papers*, no. 3, pp. 58-74.
— (1918), Ett Bidrag till Krisernas Teori, *Ekonomisk Tidskrift*, vol. 19, pp. 66-75.
Yasui T. (1953), Self-Excited Oscillations and the Business Cycle, Cowles Commission Discussion Paper, no. 2065, *mimeo*.
Zambelli S. (1992), The Wooden Horse that Wouldn't Rock: Reconsidering Frisch, in Velupillai K.V. (Ed.) (1992), *Nonlinearities, Disequilibria and Simulation: Essays in Honour of Björn Thalberg*, Basingstoke and London, Macmillan, chapter 4, pp. 27-54.

Summary: Origins and Early Development of the Nonlinear Endogenous Mathematical Theory of the Business Cycle (J.E.L. B22, B23, C18, E32)

We study the origins of the nonlinear, endogenous, theory of the business cycle, in mathematical modes, within the framework of a macroeconomic theory, which was itself going through its own formal «birth pangs» at the same time, in the same years. The first part of the story begins in 1928 and ends, with the publication of Yasui's classic on Kaldor, Hicks and Goodwin, in 1953, and Hudson's classic of 1957. But there were other classics in the 1930s, even within some theories of the business cycles of the time – particularly the Austrian and that which may now be called the «time-to-build» tradition, which originates in Marx and Aftalion, independently, and reaches its *nonlinear* formalization origins in Tinbergen's work of 1931, followed by Kalecki's theories of the business cycle, substantially influenced also by Tinbergen's classic for mathematical method. There is also what may, for want of a better name, be called the «cobweb» tradition, on the one hand, and the tradition of *Swedish Sequence Analysis*, on the other (especially in the 1937 classic work of Lundberg, summarising the Swedish discussion on business cycle theory). The former having its origins, *partly*, in Austrian inspired search for an integration of dynamic method with equilibrium economic theory (especially represented by a series of classics by Rosenstein-

Rodan, from about 1929); and *partly* in the well known phenomenon of lagged responses in the supply-demand interactions in agricultural and commodity markets, particularly elegantly formalised by Leontief in 1934. From the point of view of economic theory, they were all part of the emerging consensus on the need to incorporate money and fluctuations in non-trivial ways as intrinsic components of orthodox equilibrium economic theory which was characterised as static theory. The implication was that the search was for a synthesis of *dynamic method* with traditional static equilibrium economic theory. The origins of macroeconomic theory, generally attributed to the post-depression development of monetary theory, business cycle theory and the theory of policy, could be traced to this particular search for a synthesis and was brilliantly summarised by Kuznets in a series of pioneering contributions in 1929-30. The story we try to tell is of mathematical business cycle theory in its non-linear modes, and how it emerged from one strand of macroeconomic theory, which, as just mentioned, was itself being forged, *ab initio*, dynamically.