# HICKSIAN VISIONS AND VIGNETTES ON (Non-Linear) TRADE CYCLE THEORIES

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# Hicksian Visions and Vignettes[*] on (Non-Linear) Trade Cycle Theories[†]

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**Abstract**

Hicks theorized about the trade cycle, from many perspectives, over the whole span of his long, fertile and distinguished professional life. Beginning with thoughts on an equilibrium approach to the problem of the trade cycle in the early 30s, traversing[1] those disequilibrium workhorses of macrodynamics, IS-LM and Multiplier-Accelerator models, he returned, in hunted hare fashion, to a Robertsonian starting point. In this paper I attempt to construct a mathematical tapestry of some of these Hicksian visions and vignettes, concentrating on (non-linear) trade cycle theories. I suggest that there are still pearls of analytical wisdom, on the non-linear dynamics of trade cycle theory, to be extracted from *A Contribution to the Theory of the Trade Cycle* (*CTTC*). The unlikely link between the way an economic conundrum, inherent in *CTTC,* was resolved and the resolution of (Part B of) *Hilbert's 16th Problem* for Liénard's equation is briefly mentioned.

*Keywords*: Non-Linear Trade Cycle Theories, Mathematical Business Cycle Theories, Hicksian Models of the Trade Cycle
   *JEL Classification Codes*: B31, B41, C65, E32

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[1] To be very 'Hicksian' about it!

# 1 Preamble

"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 application was itself both the cause and the consequence of new developments in the field of pure theory; for one of the chief things that had to be done was to bring monetary theory into a closer relation with general (non-monetary) economics. The development in our knowledge of the Cycle was thus, from one point of view, a purely *theoretical* development."

[29], pp.28-9; italics added.

I first met Sir John Hicks in May, 1981. He had been one of the three external members[2] of the Professorial appointments committee at the European University Institute in Fiesole. After the formal meetings of the appointments committee he indicated an interest to meet me. I suspect it was because he was curious to talk to me about a particular paper on capital theory that had been listed in my CV[3]; but Richard Goodwin's effusive letter of recommendation may also have played a part in that interest, in view of events that transpired some years later. When that interest was conveyed to me I was most pleased and excited, but also, naturally, somewhat apprehensive. Here was the supreme economic theorist of the 20th century expressing interest to meet a supreme non-entity like me. In any case, the result was that we - my wife and I - had the great pleasure and privilege to host Sir John and Lady Hicks for lunch at the wonderful Ristorante Omero, in Arcetri, near where Galileo had spent his years of house detention. The lunch was, as it always used to be at Omero, sumptuous. I can still recall, with crystal clarity, that we drank some vintage reds from *Badia a Coltibuono* (to whose even lovelier restaurant in Gaiole in Chianti I was able to take Sir John and Lady Hicks, a few days later). Much good food and several bottles of wines had been consumed, interspersed with gentle, civilised, intellectual conversation about many things - ranging from capital theory and Swedish economic thought and Swedish economists all the way to events that were making a once salubrious Ceylon into a powder keg of ethnic violence - when a flushed and wholly red-faced Sir John suddenly asked us whether we knew that Galileo had lived nearby. That much we knew as we had often, after a meal at *Omero*, walked the few steps down the street to view that historic house. Then Sir John went on, equally suddenly (after, if my memory serves me well, at least four bottles of wine had been consumed at the

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[2] The other two were Herbert Giersch and Edmond Malinvaud.

[3] That particular paper was eventually published in 1995 (cf. [77]).

table, most of it by Sir John himself!), to recite, in the most mellifluous voice and tone I could then recall, from *Paradise Lost*:

> He scarce had ceased, when the superior fiend
>
> Was moving towards the shore, his ponderous shield,
>
> Ethereal temper, massy, large and round,
>
> Behind him cast. The broad circumference
>
> Hung on his shoulders like the moon, *whose orb*
>
> *Through optic glass the Tuscan artist views*
>
> At evening from the top of Fesole
>
> Or in Valdarno, to descry new lands,
>
> Rivers, or mountains, in her spotty globe.
>
> ....."

That sublime meeting set the tone for my relationship with that great and good man. In the years following that serendipitous meeting in May, 1981, till 1985,I had the pleasure to host Sir John and Lady Ursula Hicks at least once a year - sometimes in conjunction with his visits to Bologna as Stefano Zamagni's guest; at other times in association with his visits to Siena or Pisa. They were, all of them, memorable occasions. Later, after I had left Fiesole and moved to a Chair at Aalborg in Denmark, one of the first things I did was to organize a conference to celebrate '50 years after IS-LM', synchronizing it with a conference my friend Lars Jonung was organizing, at my suggestion, to reminisce on the 'Stockholm School', fifty years after Ohlin's famous papers of 1937. Sadly, Lady Hicks had, by then, passed away and Sir John did not feel able to travel alone with confidence (he had started using the wheelchair more regularly after the death of Lady Hicks). Therefore, I travelled to Porch House, was hosted by Sir John and stayed with him at his home, and accompanied him back, first to Stockholm and, after the conference in Saltsjöbaden, on to Aalborg. Soon after the IS-LM conference I went off to China and, thus, that happy occasion in Aalborg turned out to be the last time I saw him personally.

My main topic on this occasion is, unsurprisingly for someone who was a pupil of Richard Goodwin, Björn Thalberg and Nicholas Kaldor, *Non-linear Trade Cycle Theory*. Thus, the major portion of the paper will be against the backdrop provided by the contents of *A Contribution to the Theory of the Trade Cycle [31];* henceforth referred to as *CTTC*). I don't think Hicks ever really, wholly, repudiated any of his early works, even when he had some misgivings about them with the advantage of hindsight; but, then, don't we all gain in wisdom with hindsight! In his later years it seemed as if his greatest reservations were directed at the analytical and conceptual limitations of *Mr Keynes and the Classics* and *CTTC*[4]. In a letter to me, dated 14 February 1984, he wrote:

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[4]Dr Andrew Schuller, Economics Editor at Oxford University Press, during the presentation of his contribution to the 'Hicks Centennial' reminded us that OUP wanted to let *CTTC* go

"The part of my own work which comes nearest to Dick Goodwin's is of course my **Trade Cycle** (1950); but of all my books that is the one from which I nowadays feel most remote."
[38]

His reservations about IS-LM are recorded, perhaps most cogently, in [37], but the misgivings about both [30] and *CTTC* are stated in a broader context in his contribution to the Georgescu-Roegen *Festschrift* ([34]). It is, therefore, with some trepidation that I seek to pay homage to this economic theorist of impeccable achievements via a reflection on the impact *CTTC* and trade cycle theories based on IS-LM have had on the profession. I am quite unsure whether this form of homage would have appealed to Sir John Hicks. But, as Patinkin pointed out at the 1987 IS-LM celebrations in Aalborg, Sir John may well have misgivings or even disown his own creations; they have, however, after their creations by him, become part of the folklore of the subject and live lives that he may not have intended them to live - somewhat like children who carve out a life of their own, without necessarily denying their origins. *CTTC* and Trade Cycle Theories based on IS-LM continue to be fertile sources of inspiration for work in macrodynamics and celebrating them is, surely, also a way of paying homage to their imaginative creator. His intentions when he conceived them may have been wholly different from the lives they lead now. That, alas, is the way of events in *historical time*, a way Sir John celebrated with increasing vigour in his later works.

John Hicks contributed to the *theory* of aggregate economic fluctuations in many ways; he interpreted the *history* of fluctuations in almost equally many ways; moreover, his macroeconomic theoretical constructions were used by legions of economists to theorize and interpret the stylized facts of fluctuations in a number of fertile ways. His earliest contribution to the theory of business cycles was produced when he was squarely in the Robbins-Hayek stables, in 1933; his last published work on the theory of cycles, in 1981, suggests that he had found a Robertsonian theoretical corner most comfortable - having traversed, for long periods, a Keynesian path. This is not a survey or an evaluation of Hicksian theories of the trade cycle. It is, instead, an attempt, to understand the *non-linear* contexts in which his main theoretical contribution to aggregate fluctuations, the model(s) in *CTTC* and those inspired by IS-LM, arose, survivied and flourished, and how to place them in that setting and context. With this in mind, §2 provides a fairly extensive story of the context in which non-linear Keynesian theories of the trade cycle arose and became consolidated.

Like all *classics* CTTC merits reading and re-reading, if not for the ostensible subject matter, then for its supremely elegant prose. One reviewer is reputed to have referred to it as a *lyrical poem*. In my re-reading of it I was pleasantly surprised to discover insights and suggestions that seem to have escaped formalizations by earlier students of the book, many of whom have formalized one or

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out of print in the late 60s. However, Sir John had voiced strong objections against such a decision and, as a result, the book continued to be available in its Clarendon imprint from OUP.

another deep aspect in the book. In §4 this 'discovery' is explained, formalized and a solution is suggested.

Hicks knew, and others pointed out almost *ad nauseam*, that the two main economic and technical infelicities in the mathematical model of *CTTC* were his handling of the definition of *autonomous investment* and the economic underpinnings of the lower turning point, the so-called *floor*; they are, of course tightly connected. I have my constructive say about remedying an aspect of the infelicity regarding *autonomous investment* in §3. One of the most imaginative suggestions to circumvent the technical problems of having to rely on a rigid *floor* was given by Richard Goodwin in his illuminating review of *CTTC*. This technical suggestion led to the formulation of a new kind of dynamical system, one of those rare occasions when a purely economic hypothesis suggested, to a supremely innovative economic theorist, a particular formalism that resulted in the discovery of a new kind of non-linear differential equation. Even more remarkably, this particular formulation eventually resulted in a partial resolution of one of the most obdurate of Hilbert's celebrated '*Mathematical Problems*' (it was part B of the 16th in a collection of 23). A brief recapitulation and description of the essential story is given in §5. I conclude with reflections on the experience of reading and interpreting Hicksian writings.

There are serious omissions in my discussion of works that were inspired by *CTTC*. Two, in particular, need to be especially mentioned right at the outset: the remarkable Yale doctoral dissertation by Ana Maria Martinera Mantel ([54]); and the comprehensive guide to difference equations, disguised as an elaboration of *CTTC*, by Ragnar Frisch in the *Mahalanobis Festschrift* ([15]). These two works were genuine extensions of the *CTTC* model, *within its own method of linear difference equations*. Having chosen to concentrate on the non-linear visions and vignettes on trade cycle theory, I have deprived myself of the chance to make the story more comprehensive and more in tune with the way Hicks himself chose to tackle the topic. Perhaps a revised and expanded version of this attempt would find a way to encapsulate the Mantel and Frisch contributions in a seamless way into the story.

In re-reading Hicks, whilst preparing this paper, there were occasions when I was puzzled by some of his more critical mathematical reflections, particularly on stability, existence, non-linearity and dynamics. Coming from the kind of background in which I was brought up, I am familiar and comfortable with fallible gods. Thus, I am, at first, perplexed when I read a statement like the following:

> "My equilibrium path is the same as Harrod's warranted path, and (for the same reason as his) it is unstable. But (as I say) 'mathematical instability does not itself elucidate fluctuations. *A mathematically unstable system does not fluctuate; it just breaks down*'. A fluctuating model, even if it is unstable in the large, must be stable in the small, in order that the path which it actually follows should be determinate. In order to get that *local* stability, I had to introduce lags."

[36], p.171; first set of italics, added.

A part of the above 1982 'Prefatory Note' to his 1949 review of Harrod's *Towards a Dynamic Economics* ([28]) reads suspiciously like a mathematically incorrect statement. But, whenever I suspect that a gentle intellectual giant of the profession has made a mistake I remind myself of fallible gods and the Buddha's wise response to the hair-splitting Subuhti's perplexed question on whether the 'Venerable Solow' might have 'made a mistake':

> "Forsake fear, Subhuti. Venerable Solow may make peculiar assumptions, but *he never makes a mistake*"
> [67], p.40; italics added.

On the whole, this precept has been a helpful guiding hand in re-reading the *Maestro* on *Trade Cycle Theory*, and other things.

## 2  Non-Linear Trade Cycle Theory:The Setting

> "van der Pol believes[5] 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."
> [80], p.624.

Goodwin remarked, in 1951, ([20], p.1; italics added) that:

> "[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 is one of the earliest issues of this journal, has gone largely unheeded"

In a sense, he was only summarizing a research activity which, by then, had gathered enough momentum to lead to textbook encapsulations of nonlinear models of the trade cycle by the late 50s. 'The Setting' I refer to was provided by the period 1928-1953. The quarter century in consideration was a period of

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[5]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."
> [79], p.1081.

flourishing and fertile research in the mathematical modelling of business cycles. My choice of precisely these initial and terminal years are motivated by *ex-post* considerations. To the best of my 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."
>
> [25], p.112[6]

The terminal year of 1953 was when Takuma Yasui's influential work was published, formalizing Kaldor's 1940 model of the trade cycle in terms of van der Pol's celebrated equation.[7]

The same quarter century saw cataclysmic economic events impinge not only upon the advanced industrial economies but the world economy, in general. In a period of such turmoil, it is hard to rely on current data to reflect the workings of stable institutions, consistent individual behaviour or exploitable technical possibilities. The sobriety of time's tempering hand, the sifting and winnowing of history's critical eye and the sheer drudgery of collecting, collating and systematising relevant data, mitigates against immediate analysis and hasty conclusions on these (and many other) fronts. Even granting this particular caveat against the discipline of the empirical record, the paralysis with which the great depression was received by the orthodox economic theories of the time did have repercussions in the theoretical corpus and applied economic frameworks of academic economics. The subject of Macroeconomics was born, for a second[8] time, and a lasting framework for the statistical underpinning of the new subject was developed with single-minded purposefulness and clear directions by Keynes, his collaborators and the Swedes. The result was the almost simultaneous birth of formal national income accounting, tailored to fit the aggregate categories of Macroeconomics and conceptually well defined from

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[6] My own discovery of Hamburger's work is as follows. Concisely summarised, it was the late Professor Sukhamoy Chakravarty who, during a personal conversation in Cambridge in 1982, referred me to Hamburger's claims to priority in this area. Some of this information was summarised, after I passed it on to her, in the doctoral dissertation of my brilliant student, Serena Sordi.

[7] I am using, for purposes of reference in this paper, the 'somewhat condensed version' of [83] that was mimeographed and circulated in 1961. I am grateful to Professor Masanao Aoki for making this available to me. In a sense Yasui's paper brought to the attention of 'western' researchers the influential work on non-linear macrodynamics that was being done in Japan by Ichimura, Morishima and others.

[8] Perhaps, some would say, a third time. The first time at the hands of the Classical Economists; the second time in the imaginative works of Wicksell and the third time inspired by Keynes and the Swedish followers of Wicksell.

a measurement point of view, as well. The third element in making the subject indisputably empirical was the codification of the mathematical statistics to discipline the unruly data which were tamed by the constraints imposed by national income accounting and underpinned by macroeconomic theory. The most significant and cementing fourth component was, of course, the mathematical formalization of the macroeconomic theories, with the new aggregate categories as the building blocks, constrained by imaginative and evolving accounting rules so that relevant statistics could be collected, collated and systematised for use by the discipline of econometrics.

Against the backdrop provided by the above potted history of the codification of macroeconomics, it is clear 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, and theoretical desiderata buttressed by such empirical evidence. A study of the relevant work of the period suggests the following desiderata were explicitly or implicitly invoked:

- 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 models of macroeconomic fluctuations were tied to an additional modelling precept: endogeneity of economic fluctuations. 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. 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[9]. Instability, multiple equilibria and a theory of policy within a framework of growth and business cycles in an advanced industrial monetary economy

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[9]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. The former emphasises proportional and the latter derivative policy controls.

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. I 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 exceptionally elegant prose[10], 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 ([52]), Nicholas Kaldor ([42], Richard Goodwin([18]) and John Hicks ([31]) - to realize that aim.

Several other authors, in the period considered, appealed to one or more of the above desiderata. However, to the best of my 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 (*op.cit*) 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 theorizing. 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

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

due to the personal tutoring he received from Philippe Le Corbeiller.

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 synthesized 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[11]. 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 - except to Lundberg.

However, before concluding this section, it might be useful to face the following issues and try tentative answers for them:

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, why were Harrod's implicit non-linear hypotheses for the accelerator not recognized?

4. Fourthly, what of Kalecki's place in this particular non-linear setting?

5. Fifthly, 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?

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[11] 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 Goodwiin." [46], 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 briefly discussed above. The only surprise in the lists above is the absence of Harrod's name.

6. 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 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 (*ibid*) that his 'suggestion .. was ..corroborated by results indicated in [his] paper[s]' in Dutch and French, published, respectively, in 1930 and 1931 ([23], [24])[12] However, the 'corroboration' is simply by way of appeal to descriptive similarities of crude statistical plots of time series pertaining to arbitrary economic variables[13]. 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.

The full details of Lundberg's model of the inventory cycle cannot be discussed here[14]. All I 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

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[12]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:

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

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 $\alpha$ (0.1, 1.0, 10), presumably for a given value of $\omega$ (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.

[13]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 theorizing 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* (cf. [72], footnote 71, p.288)

[14]Readers wishing to get a partial idea of what is meant here could profitably read [3] and [4].

would act as bounds on unlimited expansion and catastrophic contractions. In effect, the formal model was in terms of a piecewise linear 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."

> [62], 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. Formally, from a mathematical point of view, there is no difference between Lundberg's model of 1937 and the Hicks model of 1950 presented in *CTTC*. But the latter work inspired a vast and fascinating line of research on non-linear modelling of macroeconomic fluctuations and the former was buried as a noble and pioneering work on (linear) inventory cycles[15].

Harrod's *Trade Cycle* ([26]) contained a clear economic presentation of the *non-linear accelerator*. Unfortunately, the same fate that befell Lundberg's piecewise-linear model at the well-meaning hands of Lloyd Metzler was experienced by Harrod's non-linear model through a review by Tinbergen . In the brief review, Tinbergen's devastating point was made with characteristic simplicity and directness:

> "Die kombination also der >>relation<< mit dem Satz über den >>multiplier<< id der oben beshriebenen Weise gibt essentiell keine Theorie des Zyklus ...."
> [73]

By the time it was made clear to the macrodynamic community that Harrod's *Trade Cycle* contained an economic formulation of the *non-linear accel-*

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[15] In addition to bequeathing to macrodynamics the legacy of the 'Lundberg lag', fruitfully exploited in some of Richard Day's work, using a (non-linear) IS-LM framework in modelling 'Keynesian Chaos'.

*erator*[16], the works by Hicks and Goodwin had superceded that early non-mathematical attempt at formulating a model of endogenous maintained oscillations underpinned by multiplier-accelerator interactions[17].

In a non entirely unrelated context Solow observed, in his Presidential Address to the *Eastern Economic Association*:

> "I half expect to be struck by one bolt of lightening labelled Wicksell and another labelled Kalecki."
> [68]

Having saved myself from the Wicksellian bolt of lightening via Lundberg, let me now try to cover the potential Kaleckian bolt. Happily I will only have to refer the interested reader, that proverbially mythical character, to Kaldor's perceptive discussion of Kalecki[18] ([45]) in the appendix to [42]. Kalecki, somehow, was not able to transcend the linear paradigm, even although he had all the necessary economic underpinnings to take the crucial step to make his model generate non-linear relaxation oscillations. That step was left for Kaldor to take, by subtle, but essential, modifications of crucial economic dynamic assumptions that implied a locally unstable equilibrium constrained to lie in a closed and bounded region of the income-capital phase plane. That brief discussion in the appendix to Kaldor's classic paper suggests, also, that the origins of the *S*-shaped nature of Kaldor's ingenious non-linear investment and savings curves lie in Kalecki's prior insight on the geometry of the investment decision curve $\phi_e$:

> "There are good reasons to believe that the curve representing the function $\phi$ is *S*-shaped."
> [45], §3, p.310.

There are interesting parallels between the way Goodwin extracted the essential non-linearities from Harrod's *Trade Cycle* to, then, go onto formulate his famous and lasting non-linear model of the trade cycle and the way in which

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[16] In Ichimura's famous paper on non-linear trade cycle theories, in the Kurihara volume on *Post-Keynesian Economics* of 1955, ([41], footnote 28, p.217).

[17] In a personal letter to me, dated 17 June 1985, Goodwin wrote as follows ([21]):

> "And that was what I was interested in - trying to formulate a model which would show that Harrod was right and Tinbergen wrong in that rather brutal review he wrote of Harrod's book. ... For Harrod's model was a first order equation, not second order, as was the great breakthrough of v.d.P [van der Pol]. So, in my view, Tinbergen goofed, by squashing Harrod, but goofed in one of those profoundly important ways ..."

This letter was in response to one of my own to him in which I had reported that Sukhamoy Chakravarty, in personal conversations, had indicated to me that Tinbergen was well aware of van der Pol's work, if not directly, then at least through Hamburger's references. Tinbergen had, in fact, cited Hamburger's work in his celbrated Business Cycle Survey papers of the 30s (cf. footnote, 11, above).

[18] Meade's review in the June, 1939 *Economic Journal* has an elegant and concise description of the essence of the theory of the cycle in [45] (cf. in particular pp. 304-5 of [56]).

Kaldor made explicit the rich non-linear potentials in Kalecki's *Theory of Economic Fluctuations* in *his* justly famous 1940 *Model of the Trade Cycle*, although Kaldor did confess, in his 'Hicks Lecture', that he:

> [N]ever had the patience to learn mathematics."
> [44], p.188

Thus, almost immediately after Kalecki's 1939 version of his *Theory of Economic Fluctuations* was published[19], Kaldor's 'non-linear Keynesian version' of it appeared and, in a precise mathematical sense transformed mathematical business cycle theory - a transformation that continues to have repercussions to this day, even as I write these lines.

Goodwin's re-writing of a Harrodian mediated non-linear Keynesian business cycle theory needed a longer period of gestation before it appeared, clothed in the full paraphernalia of the mathematics of non-linear differential equations, harnessing results from the frontiers of that subject. As he explained to me, in 1985:

> "In my case, the proper way to phrase it ... is to say that I was concerned for ten years with trying to formulate a model which would show that Harrod was right in his intuition, however preposterous his inept formulation - and it was this almost single-minded pursuit, which finally led me to the formulation of the one-sided nonlinear oscillator"
> [21]

That 'single-minded pursuit' was facilitated by an unlikely partnership, brought about in the most serendipitous of ways. In 1933, in the very first volume of *Econometrica*, Philippe Le Corbeiller had written, suggestively and challengingly:

> "Le problème des crises, et plus généralment 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 pouquoi j'ai pensé pouvoir

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[19]Interestingly, the 1936-7 *RES* version of the model does not contain as explict a statement as the 1939 version on the $S$-shaped nature of the $\phi$ curve. Is this why Hicks wrote, in his 1942 *Economica* review of Robertson's *Essays in Monetary Theory*:

> "Mr Kaldor's 'Model of the Trade Cycle' is perhaps the only strikingly original contribution to the theory of fluctuations which has seen the light since September, 1939."

Was Kalecki's 1939 version of *Theory of Economic Fluctuations* the last 'strikingly original theory of fluctuations' before Kaldor's appeared in March, 1940? Many 'strikingly original theories of fluctuations appeared in those crowded 30s - years of adventures for businss cycle theory by some of the great pioneers of the subject: Haberler, Schumpeter, Tinbergen, to mention just the more famous few (in addition to Lundberg, Harrod and Kalecki, the protagonists in this story).

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 oscilla-tions de relaxation*, due à un savant hollandais, le Dr Balth. van der Pol."

[48]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.[20] Secondly, there is ample evidence, even at those very early stages in the development of the analytic apparatus of (non-linear[21]) *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 emphasizing this aspect in its dynamics.[22] Thirdly, here I am 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. I believe a little more research effort may close this minor

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[20]Unfortunately, the University of Oslo library where, at present most of the Frisch Archives are depsoited, 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 systremes 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, expressing his gratitude for the honour Frisch was bestowing upon him with the proposal to publish his piece.

[21]Lest the unwary reader think I am being facetious with the qualifying 'non-linear', I must point out that, in economics, an early attempt at applying the ideas underlying relaxation methods emphasised linearity. I shall deal with this later, in this section.

[22]This eminently clear in his elegant booklet of 1931 (cf. [47]), based on Seminars given at the Conservatoire National des Arts et Métiers on 6-7, May, 1931. In particular, the con-cluding section, sub-titled Aperçu historique et conclusion (pp.43-5), although the whole work reflects the mind of a scientist with an admirably broad vision of natural and physical phe-nomena. 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 incontrovert-ible economic factors had to wait another decade and a half, much due to the personal efforts of Le Corbeiller himself, albeit indirectly.

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. However, the paper that was eventually published in 1933 had, in fact, been presented at the September, 1931 Lausanne meeting of the Econometric Society. Frisch had received a copy of the first draft by July, 1932. Sometime between May and September, 1931, Le Corbeiller had conceived and written this pioneering paper. The source of the inspiration remains to be discovered.

To the best of my 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: In Paul Samuelson's path-breaking monograph, *Foundations of Economic Analysis* ([61]); in Georgescu-Roegen's contributions to the **Cowles Foundation Monograph** on *Activity Analysis of Production & Allocation* ([17]) and, finally, in Richard Goodwin ([20]). It was only this latter work that directly took up the challenge posed by Le Corbeiller and codified it into a usable formalization, within standard macroeconomic theory, as 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 difference 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' (ibid, 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 emphasizing the relevance of 'relaxation phenomena as a model for business cycles', (ibid, 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'

[ibid, p.116]

Georgescu-Roegen's attempt at introducing relaxation phenomena in economic dynamics took the unusual form of emphasizing 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 formalized 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*[23] was presented by Richard Goodwin at the Cleveland Meetings of the *Econometric Society*, on 30 December, 1948 and reported in the *Econometrica* the following year, ([18], pp.184-5). The full paper was published subsequently in the same Journal as the lead article in the first issue of 1951 ([20]). 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, p.6):

> "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."
>
> ibid, p.2; italics in original

A detailed presentation of the full Goodwin model of 1951, though useful for the context of the discussion, will not be attempted. This is mainly due to the fact that there is an excellent, pedagogical, presentation for applied mathematicians, and others with a similar bent or training, in [2], §.5.2, pp. 184-190.

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[23] 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*' (**Philosophical Magazine**, Series 5, Vol. 15, April, 1883). It was, perhaps, not a coincidence that, forty three years later, van der Pol's classic paper, '*On Relaxation Oscillations*', was also published in the same Journal (**Philosophical Magazine**, Series 7, Vol. 2, November, 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 also, albeit implicitly, on the economic theories of Marx, Keynes and Schumpeter. Some substantiation for this statement can be found in the first footnote in [20] and the last sentence of the second footnote of the same paper.

However, the influence and impact Goodwin's paper had on the subsequent development of non-linear business cycle theory came about in a peculiar and precise way through the effect it had on the work of a trio of remarkable Japanese economists: Takuma Yasui, Shinichiro Ichimura and Michio Morishima.

The high point of Goodwin's paper was his demonstration that the interaction of the non-linear accelerator and the dynamic multiplier could be shown, under highly plausible economic assumptions, to be reducible to a *forced*[24] Rayleigh-like equation in output $y$ (cf. [20], equation 5e, p.12):

$$\ddot{y} + \vartheta\dot{y} - \frac{\varphi(\dot{y})}{\epsilon\theta} + \left[\frac{(1-\alpha)}{\epsilon\theta}\right]y = \frac{O^*(t)}{\epsilon\theta} \tag{2}$$

Yasui, using this as a starting point and observing that the general economic hypotheses underpinning the Hicks model in CTTC, particularly the reliance on a non-linear acceleration principle, even though it was formulated in discrete time, were substantially equivalent to those in the Goodwin model, began a tradition of referring to the Hicks-Goodwin Model being represented by the (*forced*) Rayleigh equation. In contrast, there was the non-linear Kaldor model, relying on an investment function depending on the level of income and the stock of capital. Yasui was the pioneer who reduced it to the (*unforced*) van der Pol-type equation in income, $y$(cf. [83], equation 2.17, p.232):

$$\ddot{y} + \frac{1}{\epsilon}\left[\epsilon\left(\mu + \delta\right) + s - f'(y)\right]\dot{y} + \frac{s}{\epsilon}\left(\mu + \delta\right)y - \delta f(y) = 0 \tag{3}$$

It will be immediate to any perceptive reader that the above equations are special cases of the generalized Liénard equation:

$$\ddot{y} + g(y,\dot{y})\dot{y} + h(y) = r(t) \tag{4}$$

This was still an era of deriving special equations and showing the existence of limit cycles. It was not yet the era of formal dynamical systems formulations and (non-constructive) proofs of existence of limit cycles in planar dynamical systems using the Poincaré-Bendixson theorem. It was an era that came to an end with its high point being a demonstration by construction of specific equations - that economic hypotheses could lead to equations of the van der Pol-type or Rayleigh-type[25] and then to harness results for such equations to show the existence of economic cycles with the required characteristics. An era that began with Hamburger's raw insights and naive analogies and ended with the dominant macroeconomic theory of the times being harnessed to form the basis for a theory of aggregate economic fluctuations, formalized in terms of systems that were being studied at the frontiers of non-linear dynamical systems theory. It had taken a full quarter of a century from the first, dim, insights of Hamburger to the final formalizations of non-linear Keynesian relaxation oscillations at the hands of Goodwin and Yasui. Everything that future generations

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[24]The forcing term comes about because Hicks and Goodwin introduced autonomous investment in a *linearly additive way* into their system. More on this below.

[25]They are *formally* equivalent.

of non-linear macrodynamic theorists developed, with increasing technical sophistication, relied on the building blocks that were constructed by Kaldor, Hicks, Goodwin and Yasui; they, in turn, had built on the foundations that Schumpeter, Keynes, Kalecki and Harrod had provided. The only melancholy aspect of this story is the denial of Lundberg's role, built on the foundations that Wicksell, Lindahl, Myrdal and Hammarskjöld had provided for him, as one of the pioneers of non-linear macrodynamics.

# 3 Hicks-inspired Non-Linear Theories of the Trade Cycle

"Combining the difficulties of difference equations with those of non-linear theory, we get an animal of a ferocious character and it is wise not to place too much confidence in our conclusions as to behavior."
[19], p.319, footnote 6.

This wise warning seems to have been heeded by all macroeconomists who have worked on building models of aggregate fluctuations based upon one or another Hicksian construction. With the notable exception of Richard Day, almost all macroeconomists whose inspirations were based on the two workhorses that Hicks himself developed for generating non-linear maintained oscillations, IS-LM and M-NLA (Multiplier-Non-linear Accelerator), in aggregate variables, did so in terms of *differential equations*. The other group who belonged also to the exceptional class were the mathematicians whose interests were purely technical (cf., for example, [65] and [66]). I shall divide this section into three smaller sub-sections. The first, to discuss the classic formalization by Yasui (op.cit), Ichimura (op.cit) and Morishima (op.cit) of the M-NLA model in *CTTA* in terms of non-linear differential equations of the Rayleigh-type; the second sub-section is on the general approach by Hudson ([40]) and Schinasi ([64], separated by a gap of almost a quarter of a century, to generate non-linear maintained oscillations from a generalized IS-LM model; the third sub-section is a brief outline of the attempt by Day (op.cit) to retain the difference equation method of Hicks, but to construct an aggregate model of erratic dynamics on the basis of an IS-LM model.

## 3.1 The *Japanese School of Non-linear Macrodynamists*

"The present paper is intended as an attempt to explore further the possibility of post-Keynesian nonlinear theories of economic fluctuations, and also to re-examine the well-known classical theories of trade cycles in the light of recent theoretical developments along the lines of nonlinear macrodynamics. As will be shown, *most classical theories can be formulated in terms of nonlinear differential equations ...*"

[41], p.195; italics added.

The essential point here is that Yasui (op.cit), having identified the *economic underpinnings* of the models in Goodwin's '*Nonlinear Accelerator and the Persistence of Business Cycles*' ([20]) and *CTTA*, went on, also, to identify their totally different *mathematical formalisms.* Goodwin theorized and formalized in terms, largely, of continuous time and (non-linear) differential equations; Hicks, at least in *CTTA*, in terms of *linear* difference equations and *discrete* time. Indeed, Hicks was explicit about the reasons for eschewing continuous time and non-linearities, in *CTTA*. Economically, the discussion in the main text of *CTTA* had proceeded in terms of 'period analysis', a method he had probably absorbed from the Swedes; hence, it was natural, he claimed (*CTTA*, p.169), to mathematise in terms of discrete time, even though it may not have been mathematically necessary to do so. On the other hand, there was the economic question of *lags*; here he felt that the medium of continuous time would be able to handle only the very simplest kind and anything remotely realistic, in continuous time, would lead to *Integral Equation* formalisms which were 'easiest to deal with' as 'limiting cases of difference equations' (ibid). Anyone familiar with the elementary decision lag in Goodwin's model, and the approximations he had to resort to, so that the reduced form of the model could avoid being a non-linear difference-differential equation, should have no difficulty in appreciating Hicks's reasons for working with difference equations.

As for *linearity*, on this, too, Hicks was quite explicit, but less categorical (cf. also [33], pp. 212-3):

> "A much more serious limitation on the theory which follows is that all the basic relations ... are assumed to be linear. I am very conscious that this is an over-simplification; but I would plead some things in extenuation of it. ... [I]t may be questioned whether we derive any advantage from extensions into non-linearity, when we come to more complex cases.. The limitations imposed by my linearity assumption may ... be not so bad as it looks."
> *CTTC*, p. 170

Not long after *CTTC* and the Goodwin *classic* ([20]) were published, Roy Allen ([1]) codified them for textbook presentation, but did so in distinct chapters, maintaining their essential differences from both of the above points of view; similarly, Gandolfo's textbook presentations retained fidelity to the originals in these two respects ([16]), as did most textbooks of the 60s and 70s. Thus, it is not entirely clear that it is quite legitimate to interpret and categorize the *CTTA* model in the non-linear class.

Whether it is legitimate to identify a 'Japanese School of Macrodynamists' on the evidence of the work of just three Japanese economists[26] on interpreting

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[26] I have chosen, quite deliberately, this sub-heading since I want, also, to pay homage to the memory of Michio Morishima on this occasion. I doubt there was any other economist more faithful to Hicksian economics than this remarkable mathematical economist whose eloquent

and identifying the economic model of $CTTA$ with a Ralyleigh-type equation is debatable. However, it is that identification that indelibly etched itself in the collective tradition of non-linear macrodynamics a place for $CTTA$ on the same footing as Kaldor and Goodwin. Once the die was cast, however, the consequences were inevitable: harnessing of standard theorems of non-linear differential equations to establish the existence of (at least one) limit cycle. In particular, the economics of the functional forms were subject to those hypotheses that were required for the validity of the relevant theorems to which Yasui, Ichimura and Morishima appealed - especially the famous theorem of Levinson and Smith ([50]). This meant that the geometry of the 'characteristic', $\chi(\dot{y})$, had to have a 'cubic' shape (cf. Figure 1), given that the $CTTC$ model was 'reduced' to an (unforced) Rayleigh-like equation for the dynamics of output:

$$\ddot{y} + \chi(\dot{y})\dot{y} + y = 0 \tag{5}$$

This meant, in turn, that the '*floor*' and the '*ceiling*' were indispensable assumptions. Thus it was that a particular mathematical formalism and method 'locked in' the economic underpinnings of the model for posterity; it was not only $QWERTY$ with which we were all 'locked in', but also a particular theoretical technology forced the $CTTA$ model to be saddled with its weak link, the appeal to the accelerator and the necessity of an additive, exogenously given, autonomous investment component so that the theorems of orthodox non-linear dynamical systems on the plane could be invoked. No one, except Goodwin, wondered whether it would be possible to conceive of a dynamical system capable of maintained oscillations but not require the *cubic characteristic*. That story belongs in this paper, but the context is not quite this sub-section.

## 3.2   Kalecki to Kaldor - Kaldor to Hicks!

> "[I]n Hicks' model of the cycle, the monetary factor is introduced as a *deus ex machina*, made necessary by the deficiencies of the real cycle."
> [40], p.378

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tribute to Hicks was memorably stated in his *Theory of Economic Growth* ([58], p. vi; italics in the original):

> "I started to write this book in a grand room in the historic tower of All Souls College, Oxford....... . At that time Sir John Hicks was writing '*Capital and Growth*' in his room of the same college. ... It is no wonder my 'Capital and Growth' is so similar, in topics and even in style, to his now highly celebrated book! It was indeed an exciting experience to run a race with the champion. It will never be forgotten - especially since I began studying economics with his *Value and Capital* and was solaced by reading it in the gun room of Ohmura air base of the Imperial Navy when I was called up for active service in the war."

No doubt, Morishima would have been a distinguished participant at this event had fate been a little kinder.
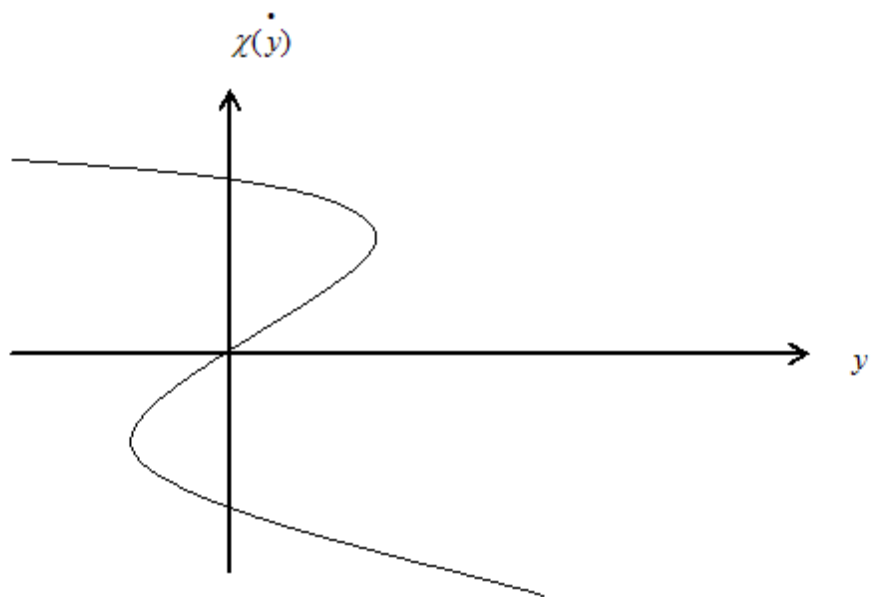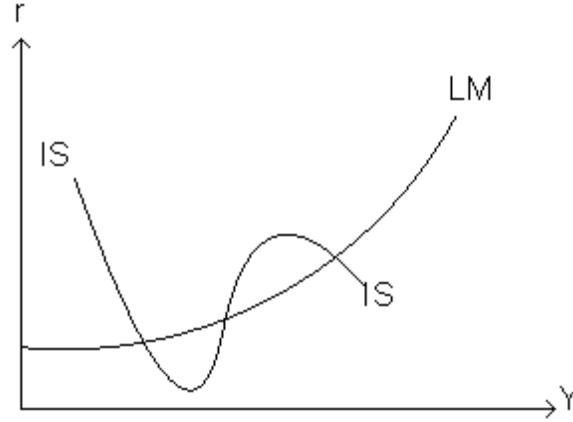
Figure 1: Cubic 'Characteristic'

Figure 2: Non-linear IS-LM interaction

This was done, via the IS-LM apparatus, in the closing chapters of *CTTC*, almost as an afterthought. Hugh Hudson (ibid), using as a direct analogy Kaldor's method of going from Kalecki's $S$-curve for Investment decisions to his own highly ingenious I-S dynamics in the K-Y phase-plane, generalized, in an imaginative way, the traditional IS-LM interaction non-linearly as in Figure 2.

Hudson's 'derivation' of the non-linear IS-curve was exactly parallel to the way Kaldor 'derived' his non-linear IY-curve (*pari passu* for the LM and SY analogies); thus they were long-run loci, based on the short-run equilibrium relations for a given capital stock. Kaldorian dynamics, in the KY-phase-plane was determined by the dynamics of the pair:

$$\dot{Y} = F\left[I\left(Y,K\right) - S\left(Y,K\right)\right] \tag{6}$$

$$\dot{K} = I\left(Y,K\right) \tag{7}$$

Hudson's analogy suggests, for his generalized IS-LM model, purely formally:

$$\dot{y} = \Xi(y,i) \tag{8}$$

$$\dot{i} = \Omega(y,i) \tag{9}$$

Hudson himself notes, perceptively:

"If the interest-sensitiveness of saving and investment are significant, then *a close approximation to Kaldor's model* can be analyzed

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in terms of the IS and LM schedules [see Figure 2], with the advantage that monetary effects are explicitly introduced into the analysis.

[In the above figure], the upper equilibrium position occurs at a stage when costs of borrowing are rising - the LM schedule is rising - and costs of capital goods construction are rising, so that $I_y$ is $< S_y$ and the IS schedule is falling. *A cycle can be produced on the basis of the schedules [in the above figure] in exactly the same way ...*"

[40], pp. 387-8; italics added.

Invoking the classic Poincaré-Bendixson theorem, it is as easy to prove the existence of a limit cycle in Hudson's generalized IS-LM system as it has become customary to do in all sorts of variations of the Kaldor system.

Almost a quarter of a century later, in 1981, Garry Schinasi [64], repeated Hudson's exercise, less illuminatingly and more formally. His claim to originality may have been the more explicit specification of the assumption for the constituent mathematical functions and equations, in addition to the fact that he proved the existence of maintained oscillations - i.e., the existence of a limit cycle - by reducing his non-linear system to Liénard's equation and, then, invoked the Levinson-Smith theorem. Apart from this particular finesse and the greater mathematical sophistication, there were no advantages to the economist in being exposed to the same story in alternate clothes; moreover, Figure 1 in [64], p.651 is the same as Fig. 6 in [40], p.388 (reproduced above)[27]

## 3.3 Hicksian Chaos!

"The Keynesian business cycle follows a straight forward scenario"

[6], p.2

Richard Day, in a series of coauthored papers, (for example, [6], [7]) has utilized the IS-LM building blocks in fertile ways to construct simple dynamical mappings of output that are capable of generating chaotic dynamics in the sense of Li and Yorke ([51]). The general strategy is almost mechanically simple and is as follows. The basic aim is to construct, from basic macroeconomic building blocks of the IS-LM family, a mapping of output dynamics depending on crucial parameters:

$$Y_{t+1} = \Theta\left(Y_t; \boldsymbol{\xi}\right) \tag{10}$$

where, $\boldsymbol{\xi}$ : is a vector of parameters.

The general starting point is the accounting relation for expenditure flows:

$$Y_t = C_t(r, Y) + I_t(r, Y; \mu) + A \tag{11}$$

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[27] There are a couple of infelicities in Schinasi's Figure 1 in that the turning points, D and B, are not 'smooth'. Also he is less than felicitous in his rendering of Liénard's name - in a paper that is almost exclusively about the equation that bears that great man's name!

where, $A$: exogenously determined *parameter* encapsulating autonomous consumption and investment expenditures;

and, $\mu$: a tuning parameter signifying the *intensity* of induced investment;

Either appending an IS-LM sub-block to determine the r-Y equilibrium locus and assuming an expenditure or income lag of one of the conventional varieties (Robertsonian, Lundbergian, Hicksian, etc.,), *or* assuming an exogenous determination of money supply and using only the LM equation to derive the short-rum equilibrium value of $r$, together with the same kind of lag assumption, reduces the parametrized output dynamics to:

$$Y_{t+1} = \Theta(Y_t; \mu, A, \bar{M}) \tag{12}$$

At this point there are many ways to guarantee the existence of erratic dynamics for such a mapping. One of the simplest and, at the same time, easily verifiable way is to use the 'Li-Yorke criterion' on '*Period 3 Implies Chaos*'(assuming, firstly, that $\Theta$ satisfies the usual regularity conditions):

$$\exists Y^* : \ \Theta^3(Y^*) \geq Y^* > \Theta(Y^*) > \Theta^2(Y^*) \tag{13}$$

or, its 'converse':

$$\exists Y^* : \ \Theta^3(Y^*) \leq Y^* < \Theta(Y^*) < \Theta^2(Y^*) \tag{14}$$

where, $\Theta^i$: denotes the $i$-th iterate of the mapping $\Theta$;

The condition can be guaranteed by the same criteria that assure the non-linearity of the IS-curve in the previous examples (those of Hudson and Schinasi) such that, in those cases, either appeal was made to the theorem of Levinson and Smith for the Liénard equation or, more generally, to the Poincaré-Bendixson theorem for planar dynamical systems.

Purists may carp at the slight scent of *ad hockery* in the assumption of the expenditure lag; but, surely, any assumption of lags does give the odour of an *ad hockery* lurking somewhere? Hicks, himself, was candid about the need for lags to smoothen economic adjustments and Goodwin's introduction of the dynamic multiplier had a similar purpose.

## 3.4   Brief reflections

I believe I have discussed the pioneering Hicks-inspired (i.e., IS-LM and M-NLA inspired) contributions to the non-linear theory of macroeconomic fluctuations. There are legions of variations on similar themes; but the 'Japanese School', Hugh Hudson and Richard Day were there, at the beginning, to harness the tools and concepts that John Hicks had introduced to the macroeconomist. All other variations were played on the themes that these pioneers broached. Their themes introduced to the macrodynamic theorist those mathematical tools that have become standard for anyone interested in non-linear dynamics. There is one omission: bifurcation theory. This, too, had its place in a Hicksian theme (cf. for example, [74]). I have left it out of this section because I shall have

use for the tool in the next section and that will be the appropriate place to introduce it. Together with bifurcation theory, any macroeconomist who masters the art of applying the Poincaré-Bendixson theorem, who understands the art of constructing and studying the phase-plane dynamics of Rayleigh-type equations, who is able to reduce simple macrodynamic systems to generalized Liénard equations so as to be able to apply the Levinson-Smith theorem, and a few other relatively simple non-linear concepts (Hartman-Grobman theorem, hyperbolicity, homocliinic-heteroclinic orbits, etc.,) could feel reasonably confident that a gateway into a weird and wonderful world of mathematical modelling of dynamics is within his or her grasp. The greatness in the deceptively simple macroeconomic tools and frameworks that were constructed by John Hicks lies in the way they are pedagogical repositories for advanced modelling techniques that may lead to the development of tools and frameworks that can help tame the unruly stylized facts of economic life. I suspect that this very idea may have been an underlying motivation for many of his subtle but exceedingly - even deceptively - simple macroeconomic constructions. That readers and scholars tend to read into them deeper theoretical significance than were intended has been a cause of untold mischief. But the greatness of John Hicks is that he ignored the mischief and continued to construct pedagogical masterpieces that have been of immense value in providing understandable visions of unruly worlds.

## 4   A Neglected Basin of Attraction in *CTTC*

> "Of all the concepts which are used in [*CTTC*], that which has caused the most trouble is *Autonomous Investment*; and here I must admit to having brought the trouble upon myself, for I do not think that I was entirely consistent in the use which made of the term. ....
> I am afraid that I do occasionally talk as if one could tell whether a particular piece of investment was autonomous just by looking at it; this is quite wrong."
> [31], pp. vi-viii[*Preface to the Third Impression* of *CTTC*].

*CTTC* was copiously reviewed almost within a few months of its publication and that, too, by some of the most eminent of trade cycle theorists: Duesenberry ([12]), Fels ([13]), Goodwin ([19]), Kaldor ([43]), Lundberg ([53]), just to mention the obvious names that immediately come to mind[28]. There were three

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[28] A few years ago I had occasion to correspond with Professor Solow about Richard Goodwin. I took the chance to ask him whether he was aware of any discussion that took place between Goodwin and Duesenberry on their respective reviews of *CTTC*. Solow wrote back as follows:

> "To answer your question about Jim Duesenberry, I called him up. He said yes, of course, he had many discussions with Dick about business cycles, about dynamic modelling, and about Hicks's book. ... He could not remember details of their talks about Hicks. Except in one respect. Duesenberry did not like the *Trade Cycle* book, found it mechanical. He ended his review with a remark that

fundamental criticisms of the conceptual and technical underpinnings of the *CTTC* model in these (and other) reviews: the unsatisfactory definition, heavy burden placed on, and the imprecise role of *autonomous investment*, particularly in its action in the neighbourhood of the *floor*; the mathematical sufficiency (and necessity) of *one* of the 'constraints', either the *ceiling* or the *floor* (but not of both), for the cycle implied in *CTTC*; the unsatisfactoriness, in inducement to invest, of relying on the *acceleration principle*. Kaldor emphasized the latter; Lundberg and Duesenberry the first; Duesenberry, Fels and Goodwin, the second. I shall return to the proposition on the necessity and sufficiency of one (or two 'constraints') for the existence of maintained oscillations in the next section. My concern here is on the imprecise role and definition of autonomous investment. With this in mind, it might be useful to reflect on the objections to its definition made by Duesenberry and Lundberg (but many others did so, as well). Duesenberry pointed out, quite directly, that:

> "Hicks's argument and many similar ones are based on a division of investment into three classes: autonomous investment, induced investment, and replacement investment. Like many other concepts in business cycle theory the above classification is somewhat poorly related to the underlying micro-theory of investment. .....
> In fact, we cannot make a clear distinction between these three types of investment except in certain rather special cases."
> ibid, p. 473.

Lundberg was even more disturbed about the assumption of observable, measurable, differences between identifiable categories of *induced* and *autonomous* elements in total (gross) investment:

> "[There] is the question of the distinction between induced and non-induced ('autonomous') investment. Hicks gives an extremely unsatisfactory description of the latter, and all that we can discover is that it is not determined by the increase in production from year to year, and that it is a necessary condition for continuous expansion that autonomous investment should increase in step with national income. ... But as far as I can see there is no firm basis for dividing total investment into these two categories. ..[T]his division [into induced and autonomous investment] can be expected to vary during the course of expansion. I consider, therefore, that this division of investment activity into categories, which is used by Harrod and Hicks, is a useless method for empirical investigation, and therefore probably an unfruitful hypothesis for a business cycle model."

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Arthur Smithies liked, because he interpreted it as a sarcastic dig at Hicks. But Dick said: '*how could you be unkind to a nice man like Hicks?*' So Jim deleted or changed the offending comment."
[69]

Note, however, that neither Duesenberry nor Lundberg (nor anyone else who has pronounced on this thorny issue) deny the existence of different categories of investment, at least two of which can be defined as induced and autonomous investment, respectively. The objections are to the assumption of observable, empirically measurable, *additive* definitions of their *levels* as identifiable and distinguishable national accounting categories. I think these are valid and serious objections and to the best of my knowledge no one has dealt with them satisfactorily within the framework of a (non-linear) $CTTC$ model. These objections must also be viewed against the backdrop of a neglected aspect of the role of autonomous investment in a complete model of $CTTC$, at least as envisaged by Hicks (and referring to Figure 3, which is Fig. 13, p. 121, in $CTTC$) :

> "[T]he actual course of autonomous investment cannot possibly be so very regular - it must experience autonomous fluctuations on its own account. These fluctuations, and their consequences, are superimposed upon the cycle which we have been studying. ...
>
> Fluctuations in autonomous investment will be reflected in corresponding fluctuations in the equilibrium lines - both in the upper equilibrium line $EE$ and in the lower equilibrium line $LL$. With this amendment [to the original model as described in Fig.12, p.97], the theory stands; it will still be true that *the upper equilibrium is unstable, the lower stable* - so that a cycle of the kind we have been describing can still be generated."
>
> [31], pp. 120-1; italics added

In other words, in the complete $CTTC$ model, which is one of multiple equilibria, there is a locally stable equilibrium coexisting with an unstable equilibrium; the former coming into play at the lower turning point when the '*floor*' is subject to 'autonomous' fluctuations of a particular kind of intensity:

> "The discovery of a new investment opportunity is itself likely to be followed by a stream of net investment with the characteristic time-shape of the hump and the tail. Thus, if such a discovery become effective in a period of depression, there will be a hump in the $AA$ line, and a corresponding hump in the $LL$ line, as shown in [the figure below]. Output is following along the $LL$ line, and it will follow it up the hump. If the hump is *large enough*, or comes *late enough*, the mere following-over of the hump *will raise output to such an extent that the accelerator will come back into action* - and will do so at an earlier date than if no hump had occurred. But, if the hump is only a small one, or comes too early, the expansion in output *may never reach this critical point*. Output would then fail to leave the $LL$ line until after the hump was completed. What had happened would then look like a weak boom - an expansion in output which fell away again without reaching the ceiling. But
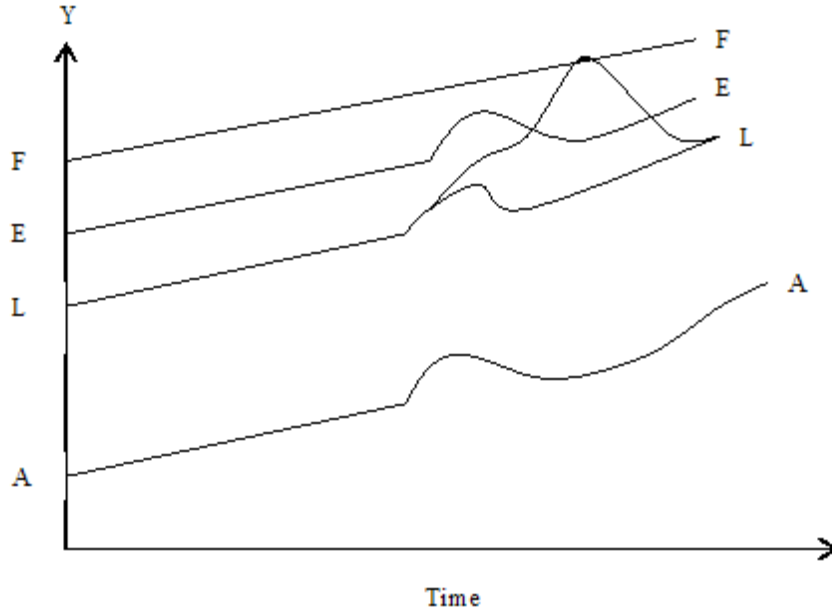
Figure 3: Multiple equilibria in $CTTC$

theoretically considered, it would not be a boom at all; t*he economy
would have remained in its depression equilibrium all the time.*"
    ibid, pp.121-2; italics added.

So far as I know, this Hicksian refinement to his basic model has never been
discussed in the vast literature on $CTTC$.

These suggestive ideas, to be modelled in the non-linear $CTTC$ model that
has become standard in the mathematical macrodynamic literature as a *Rayleigh-
like* equation, need serious re-considerations of the original non-linear invest-
ment function, $\psi(\dot{y})$, consisting only of the induced part of investment, to which
autonomous investment, $L$ was tagged on as an additive component (cf. [20],
p.9; [57], p.167 or [41], p.200):

$$\dot{K} = \psi(\dot{y}) + L \tag{15}$$

It is this kind of additive, separable, assumption that leads to the forced
Rayleigh-like equation for the non-linear dynamics of output (see, above, equa-
tion (1)). Such non-linear equations, without simple, explicit assumptions on
the forcing function are impossible to solve or characterize. All of the pioneers
of non-linear trade cycle theory who analyzed the $CTTC$ model assumed that

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the autonomous investment was *constant* to reduce the inhomogeneous, forced, nature of (2) to a homogenous, second order, non-linear, differential equation of the Rayleigh-type, for which there were known methods of solution. On the other hand, if we take seriously some of the constructive implications of the criticisms by Duesenberry, Lundberg and others on the possible formalizations of autonomous investment, it is almost clear that a *multiplicative* assumption to include it inseparably in the functional form $\psi(.)$ might be a way out of some of the definitional infelicities. I opt, therefore, to include a parameter, say $\gamma$, to encapsulate the effects of autonomous investment, as the simplest alternative, as $\psi(\dot{y}; \lambda)$. With this formulation (2) can be re-written, in generalized form, as (cf. [19], equation (7b) or (7c), p. 13):

$$\ddot{y} + \chi(\dot{y}; \lambda)\dot{y} + \zeta(y) = 0 \tag{16}$$

In this form, it is fairly easy to make sense of Hicks's desiderata for his generalized model to encapsulate multiple equilibria of the type described above: a locally stable equilibrium coexisting with an unstable equilibrium giving rise to maintained oscillations. Hicks is not explicit about the geometry of the locally stable equilibrium; i.e., he does not suggest a characterization of the basin of attraction of the locally stable attractor: it could be a stable focus or a stable node, although reading between lines it appears as it there is a preference for the former. Before I state the relevant proposition formally, summarizing these ideas, let me show the geometry that is envisaged for a simple, standard, parametrized Rayleigh equation:

$$\ddot{x} + \dot{x}^3 - 2\lambda\dot{x} + x = 0 \tag{17}$$

The equivalent first order system for this equation is:

$$\dot{x}_1 = \dot{x} \tag{18}$$

$$\dot{x}_2 = -x_1 + 2\lambda x_2 - x_2^3 \tag{19}$$

The phase-plane dynamics in the two variables, for the same initial conditions but for two values of the parameter are shown in Figure 4.

**Remark 1** *For $\lambda < 0$, the stable attractor is a focus; for $\lambda > 0$, the phase paths converge to a stable limit cycle; for $\lambda = 0$, (see, again, Figure 4), the equilibrium continues to act as an attractor of the stable focus type instead of becoming, as expected, a centre.*

With these geometric intuitions at hand, and making only those assumptions that have been made in the standard literature on the canonical *CTTC* model, the following proposition summarizes the main result for the generalized Hicks model with the required two equilibria.

For the following formalization of the *CTTC model:*

$$\ddot{y} + \vartheta\dot{y} - \frac{\varphi(\dot{y}; \lambda)}{\epsilon\theta} + \left[\frac{(1-\alpha)}{\epsilon\theta}\right]y = 0 \tag{20}$$

Figure 4: Stable Focus ($\lambda < 0$) and Stable Limit Cycle ($\lambda > 0$)

where the dynamical system depending on the scalar parameter, $\lambda$, denotes the equivalent $\mathbb{C}^3$ first order system as:

$$\dot{x}_1 = F_1(x_1, x_2; \lambda) \tag{21}$$

$$\dot{x}_2 = F_2(x_1, x_2; \lambda) \tag{22}$$

Let the vector-matrix representation of this system, explicitly separating and denoting the linear part, be:

$$\dot{x} = A(\lambda)x + F(x; \lambda) \tag{23}$$

such that:

$$F(0; \lambda) = 0 \text{ and } D_x F(0; \lambda) = 0, \ \forall \ |\lambda| \text{ sufficiently small} \tag{24}$$

- Assumption 1: At the origin $A(\lambda)$ has the eigenvalues $\alpha(\lambda) \pm i\beta(\lambda)$, with $\alpha(0) = 0$ and $\beta(0) \neq 0$;

- Assumption 2: The eigenvalues cross the imaginary axis with nonzero speed; i.e.,

$$\frac{d\alpha}{d\lambda}(0) \neq 0 \tag{25}$$

Then:

**Proposition 2** *In any neighbourhood $N_{\{0\}}$ of the origin in the plane and for any given $\lambda_0 > 0, \exists \bar{\lambda} < \lambda_0$ such that (23) with the value $\bar{\lambda}$ has a non-trivial periodic orbit.*

**Remark 3** *The assumptions are entirely consistent with those in CTTC (and those made by the "Japanese School" and implicit in [19]).*

**Remark 4** *It is, in fact, possible, if given explicit functional forms and values for the constants, to determine a bifurcation diagram to compute the radius of the relevant periodic orbit. Moreover, it is also possible to derive a formal proposition on the value of the radius of the 'corridor' within which the locally stable attractor contains its dynamics (cf. [71]).*

# 5 The 'Dead' Accelerator and the 'One-Sided' Oscillator!

"The setting up of dynamic systems of equations and the discussion of of their solutions in the form of explosive and cyclic processes, is undoubtedly a valuable branch of economic research. .... The

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danger with dynamic theory is that, as with Hicks, the logic of the
mechanism is unproductive, and the beauty of the model seduces
the researcher into attaching an unreasonably exaggerated weight to
the results."
    [53], p.105

In their masterly and detailed reviews of *CTTC*, Duesenberry, Kaldor, Fels,
Lundberg and Tsiang ([12], [43], [13], [53] and [75], respectively) all took is-
sue with the assumptions on, and the role of, the acceleration principle in the
cyclical process of the model developed by Hicks. Duesenberry coupled that
discussion and critique with the observation that the hypothesis of the '*ceiling*'
may well be redundant; but he does not suggest that a real cycle of the kind
Hicks tried to construct (at least for all but the last two chapters of *CTTC*),
with the tools of piecewise linear difference equations, is a feasible technical
enterprise. In other words, if Hicks takes Duesenberry's advice and drops the
assumption of a '*ceiling*', it is almost certain that a piecewise difference equa-
tion system cannot be shown to generate maintained oscillations as a necessary
mathematical property. Lundberg, Fels and Tsiang are more concerned with
the variability of the accelerator coefficient, even in its tranquil region - the
variability at the extremes was taken care of by means of non-linearities.

But it is in Goodwin's review that one finds a critique, and a constructive
remedy to the critique, of the role of the accelerator when the system approaches
the '*floor*'. For purely *economic reasons*, he objected to the role of the acceler-
ator in the lower reaches of the downturn. His objections were, perhaps, best
expressed in his own appreciative piece on Hicks in *The Legacy of Hicks* (cf.,
[22]):

> "When output has fallen, leaving general excess capacity, there
> is no reason to invest and *the accelerator is dead*: it can take 15, 50
> or more years for the excess capacity to disappear, so that the cycle
> would be spending most of its time in depression."
>     ibid, p.77; italics added.

To this he, too, like the other reviewers, coupled his objections to the *deus
ex machina* of *autonomous investment*, which together with a *dead accelerator*
was to revive the economy and set it on its recovery path. These considerations
convinced him that, from a purely economic dynamic point of view, there was
only justification for the assumption of the '*ceiling*'. However, this left him
in a quandary. To explain the quandary I will have to indulge in some minor
circumlocution. In the review itself he had noted:

> "Since Professor Hicks proposes a theory which will explain *the
> maintenance of oscillations*, we can be sure, *on formal grounds*, that
> this *implies a non-linearity*. In fact, he assumes two - the lower limit
> of zero in gross investment and the upper limit of full employment
> in real income. .... It is evident that full employment constitutes a

barrier because in order to get there we must have a rate of expansion, and hence a level of investment, of real income which cannot be maintained when once we have attained it. Hence induced investment must fall with consequent drops in income and employment. *This one barrier would suffice to maintain a steady oscillation*, but in fact there is a second barrier of a quite different sort form the first."

[19], p.318; italics added

Goodwin's quandary was the formal one of devising a (second-order) non-linear dynamical system that would exhibit maintained oscillations without the need for a *cubic characteristic*! There was no known second-order planar dynamical system, at the time he was writing the review, that was capable of maintained oscillations - i.e., of generating limit cycles - without assuming a cubic-like characteristic. If this could not be avoided, then a formal, second-order, non-linear differential equation economic model, claiming to be capable of maintained oscillations, then it was necessary to assume 'two barriers'.

This is where, proverbially, genius met with necessity and the result was serendipitous! Being the competent geometrist he was, he experimented with alternative *characteristics*; through this process of trial and error, but convinced that there was one to be found, he discovered, purely by construction, the feasibility of generating stable limit cycles in a planar dynamical system with a non-cubic characteristic which, to be consistent with the model in *CTTA* had to be linear, piecewise continuous. One of the first he experimented with was the following dynamical system (cf. [10]:

$$\ddot{y} + \chi(y)\dot{y} + y = 0 \tag{26}$$

with:

$$\chi(y) = \{ \begin{matrix} -m_1, & y < y_\alpha \\ m_2, & y \geq y_\alpha \end{matrix} \tag{27}$$

where, $y_\alpha, m_1, m_2$ are positive constants and:

$$4 > m_2^2 > \frac{4m_1}{(1 - m_1)^2} \tag{28}$$

More generally, inspired by Goodwin's economically motivated, successful, construction of a one-sided oscillator, Le Corbeiller posed the general problem of proving the existence and uniqueness of a limit cycle for the above dynamical system with:

**Condition 5** $\chi(\dot{y})$: *a real valued function having a piecewise continuous derivative*

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One of the implications of the proof (cf. [9], p.270) was that a particular condition in the Levinson-Smith theorem that the *characteristic* has to be an odd function became unnecessary. This was - and continues to be - assumed in many of the non-linear macrodynamic models underpinning endogenous trade cycle theories (as in the work of the 'Japanese School', Schinasi, etc.).

The following dynamical system is one of the first that was discovered, in the sense of being formally defined (by Philippe Le Corbeiller) and, indeed, its *characteristic* was named (again by Le Corbeiller) a *Goodwin characteristic*[29]:

$$\dot{x}_1 = x_2 \tag{29}$$

$$\dot{x}_2 = 0.5\left[(2x_2 - x_2 e^{x_2})\right] - x_1 \tag{30}$$

The equation for the unimodal *Goodwin characteristic* is[30]:

$$y = m(2x - xe^x) \tag{31}$$

The graph of this equation is given in Figure 5 and the stable limit cycle underpinned by this *Goodwin characteristic* is given in Figure 6.

Thus was born the one-sided oscillator which resolved Goodwin's quandary. His economic intuition rebelled against the assumption of the dead accelerator reviving, in conjunction with an unacceptable autonomous investment component, a prostrate economy. He had absorbed the Harrodian precept of the inescapable one upper bound of fully employed resources and, hence, the accptability - indeed the inevitability - of the '*ceiling*'. He was, however, a 'modeler'; and needed a handle on a feasible dynamical system that would generate maintained oscillations to make the theory stand on its own feet, rather than invoke monetary rigidities and other 'exogenous' factors for the economic system to live without one or the other of the 'barriers'. The serendipitous result of a conjunction of these considerations resulted in the discovery and construction of the *Goodwin characteristic*

## 5.1 Epilogue: From Hicks to Hilbert!

There this story - or, rather the economic part of it - should end. But I shall not let it rest there! There is an unlikely sequel to this discovery. A few months ago, quite out of the proverbial blue, I received an e-mail message from an old friend and former colleague in Mexico, Professor Felipe Bello, in which

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[29] Part of the personal aspects of this story has been reported and narrated in [76] and [78]. Goodwin's two straight lines were 'smoothed' by Le Corbeiller who correctly surmised that the key property that Goodwin wanted to encapsulate was unimodality rather than piecewise continuity.

[30] For years I had been trying to graph this function with a negative exponent because that was how it was written down in Le Corbeiller's letter to Goodwin. Then, a chance simulation with a colleague at UNAM in Mexico, led me to try a simulation with a plus sign!
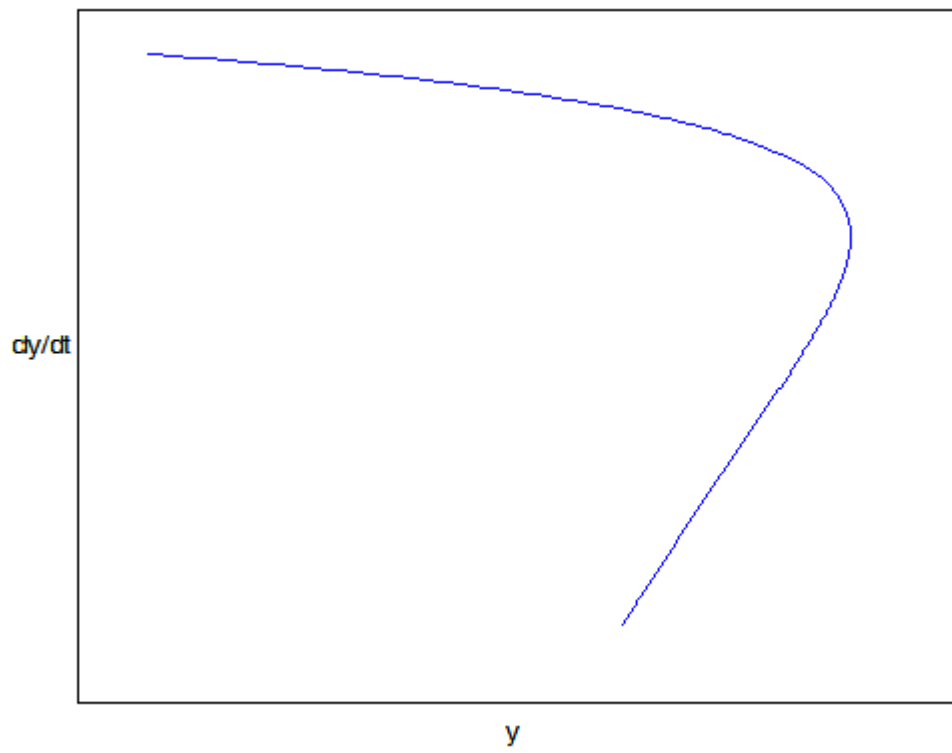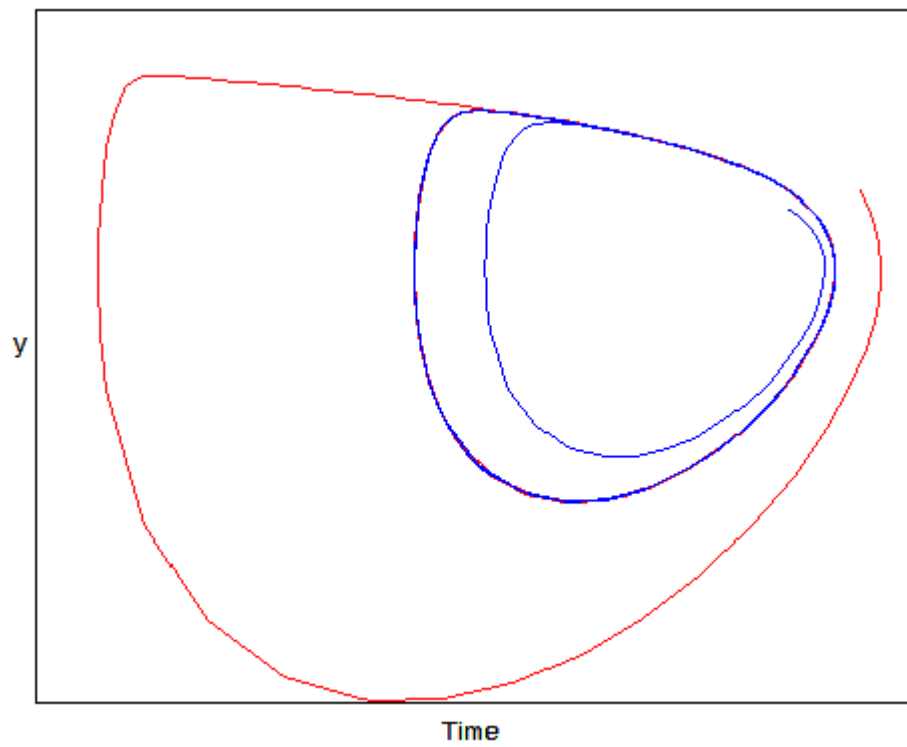
Figure 5: Goodwin Characteristic

Figure 6: Stable Limit Cycle for the 'Goodwin Characteristic'

he was wondering whether I had heard about the brouhaha surrounding Elin Oxenhielm's controversial claims to have solved *the second part of Hilbert's 16th Problem*. In passing, he also mentioned his vivid recollection of a lecture I had given at UDLA[31], in Puebla, about six years ago on a related topic[32]. I confessed, in my reply, that I had not kept in touch with progress on solving *Hilbert's 16th Problem* but promised to try to follow up what seemed to be an interesting development. I did not realize, when I made that innocuous promise, the kind of mare's nest I would have to enter in trying to learn about progress on solving the problem![33] However, one minor - almost trivial - point in the saga, as played out in web sites and pages, caught my eye. In one of the web pages there is a photograph of Elin Oxenhielm standing next to a seemingly old-fashioned blackboard and pointing to the phrase '*Liénard's eq*' (see above, equation (3)) with a generalized version of this celebrated equation written below.[34]

It so happens that Le Corbeiller, to whom Goodwin had reported his discovery of the one-sided oscillator, had presented the task of formalizing and rigorously proving the existence and uniqueness of limit cycles for such oscillators to one of his outstanding graduate students, Rui Pacheco de Figueiredo, around 1956 (Goodwin's conversation with Le Corbeiller had taken place in December, 1950 - which means a few months after he had written the *Hicks review*). de Figueiredo's Harvard University doctoral dissertation on the subject, in the Faculty of Applied Mathematics, was submitted in 1958 (cf. [8]). Over the years I have acquainted myself with de Figueiredo's methods of construction and proof and have found it intriguing to read his references to the above construction of the one-sided oscillator by Goodwin, inspired by the conundrums of a 'dead accelerator' and a constantly revived economy. The first such reference was in the above thesis (ibid, p.6-4); a few years later, in [10], p.319, footnote 2:

> "The Goodwin oscillator is an example of a 'two-stroke' oscillator introduced by Le Corbeiller and amply discussed in the literature."

I am not sure what de Figueiredo means by 'amply', but to the best of my knowledge the only two people who refer to a *Goodwin oscillator* are, in fact,

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[31] Universidad **d**e **L**as **A**mericas, **P**uebla, Mexico.

[32] The title of the seminar I gave, in the economics department at UDLA, in autumn 1998 was *Nonlinear Business Cycles and Hilbert's 16th Problem*.

[33] Unfortunately, I remain as ignorant now as I was before I received the message from my Mexican friend due to the strange turns this saga seems to have taken. Elin Oxienhjelm's paper was first accepted and published in the internet version of the Journal, **Nonlinear Analysis**. Subsequently, due to doubts cast on the rigour underlying the reasoning and proof employed by the author, the paper was withdrawn from circulation. The author has her own rather aggressive web page which the interested reader can explore and go on from there to related links, to get more current information on the state of play.

[34] On the site: http://www.memefirst.com/000172.html. Interestingly, in her interview with Dr David Whitehouse, the BBC's News Online science editor, Ms. Oxenhielm states that her solution 'may improve the way scientists use computers to simulate such diverse phenomena as global warming and economies'. (cf. http://newsvote.bbc.co.uk/mpapps/pagetools/print/news.bbc.co.uk/2/hi/science/nature; italics added).

de Figueiredo and Le Corbeiller (cfr. for example, [49],p.388, ff)

Starting from the generalized form of the *Goodwin characteristic,* de Figueiredo developed a generic theory of one-sided oscillators and, eventually, used that theory in an approach to the solution of the Second Part of Hilbert's 16th Problem for equations of *Liénard-type* (for convenience I repeat equation (3) again):

$$\ddot{x} + f(x)\dot{x} + g(x) = 0 \tag{32}$$

which, in the *Liénard plane* (as those of us old enough to have learned our nonlinear dynamics in the pre-PC era were taught to call it), is equivalent to:

$$\dot{x} = y - F(x), \dot{y} = -g(x) \tag{33}$$

More precisely, the assumptions underpinning $F(x)$ (which is the *characteristic* for this system) were formally similar to the *Goodwin characteristic,* where:

$$f(x) = F'(x) \tag{34}$$

Indeed, in concluding one of his first contributions towards an approach and a partial solution in a particular case of the Liénard equation, de Figueiredo concluded by noting ([11], p.499; italics added):

> "Other examples of systems of type [(26) or (29)-(30)], such as the *Goodwin oscillator* and some exponential oscillators [such as the one I have graphed above], possessing a single periodic solution of the two-stroke type, have been proposed by Le Corbeiller and the author."

Now, I might as well paraphrase the relevant second part of Hilbert's 16th Problem for the convenience of readers who may not have it at their proverbial fingertips [35]. The second part of Hilbert's 16th Problem seeks an estimate of the maximum of, or the bound for, the number of limit cycles of a polynomial vector field of degree $n$, and to determine their relative positions. In the above

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[35] The formal statement, in its English translation, by Hilbert is as follows:

> "...[T]he question as to the maximum number and position of of Poincaré's boundary cycles (cycles limites) for a differential equation of the first order and degree of the form

$$\frac{dy}{dx} = \frac{Y}{X} \tag{35}$$

> where X and Y are rational integral functions of the nth degree in x and y."

> David Hilbert: '*Mathematical Problems*', **Bulletin of the American Mathematical Society**, p.465; Vol. 8, July, 1902; pp.437-79.

It is interesting to recall that, in the actual delivery of the lecture to the Second International Congress of Mathematicians, in Paris, on August 8, 1900, he had to shorten his talk due to time pressures and mentioned only ten of the twenty-three that were finally presented in the published version. The 16th is one of the ten that were mentioned in the actual, delivered, lecture. (cf. Constance Reid: **Hilbert**, p.81; Springer-Verlag, Berlin & Heidelberg, 1970)

notation, if $F$ is of degree $n$, part of the problem is to find a bound for the number of limit cycles for the system as a function of the degree, $n$.

The difficulty in solving this problem, surely, lies in the fact that the hypotheses are algebraic whereas the desired conclusions are topological (geometric). I have always felt that, analogous to the negative solution to Hilbert's 10th Problem, the 16th, too, 'begs for an unsolvability proof' (cf: Martin Davis:'*Foreword*' to [**55**], p.xiii), especially due to the algebraic part. The determination of the singular points of a polynomial means, in any reasonable interpretation, the need for an explicit computational algorithm that will locate the relevant zeros. This, so far as I can see, must be susceptible to an impossibility result and I was simply waiting 'for a clever young mathematician - Russian or otherwise' (*pace* Martin Davis, *ibid*) to provide it.

My own memories went back to the notes I had prepared for that lecture in Puebla and a passing sentence I had inserted in a fairly extended obituary for *The Independent* of London, on 9 August, 1996, of Richard Goodwin:

> "It is, ironically, one of the great unwritten chapters of the development of modern applied mathematics that Goodwin's economically motivated use of, and contribution to, *non-linear* dynamical systems theory .... , was instrumental in partially resolving the 16th, one of the most obdurate of the 23 'Mathematical Problems' posed by David Hilbert in 1900, as challenges to the mathematicians of the 20th century."

Paul Samuelson, who had known Goodwin well, and to whom I sent a copy of the obituary, wrote back almost immediately, with characteristically warm words of appreciation on its content, and went on:

> "You must fill me in on exactly how Goodwin contributed toward solving Hilbert's 16th problem."

> [63]

Unsurprisingly, no one else reacted to my throwaway remark except Paul Samuelson![36]

# 6   The Pleasures of Reading Hicks

> "John Hicks is an economist in the great classical tradition .... .
> He is a pure economist in the sense that his interest is in developing
> general economic theory by improving the framework of assumptions

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[36] I have been trying, for quite some time, to write up this story in a form that would be interesting to a numerate audience, but getting the wording and form adequately organised has been difficult. The provisional title is (and has been for several years!): *Non-Linear Dynamics and Mathematical Business Cycle Theory: The Ways of Serendipity.* Any interested reader can always write for a copy of the 'work in progress' and I might oblige - but the request will have to be in the old-fashioned way!

whenever the case for such an improvement is established, and in exploring their implications as fully as logical reasoning, *aided by mathematics*, makes possible. Unlike others, whose interest in economics is more pedestrian, Hicks' main aim is the pursuit of knowledge as such."

[44],pp.187-8; italics added.

Reading Hicks has always given me a feeling of a writer trying to engage a reader in gentle dialogue, of give and take, and reminded me of the writings and style of a Neville Cardus discoursing on cricket and music. Harrod described it best in his review of *Value and Capital*:

> "Can the austere theorist , his mind wrapt in mathematical symbolism, ever obtain perfect command of the human palpitating instrument of English prose? Edgeworth gave a notable demonstration of the possibility, and Professor Hicks has now confirmed it.
>
> His mood is a mellow and expansive one; there are friendly interchanges of confidence with the reader, which do not in the least detract from his dignity and precision; his companionly relation never lapses into the coy or the hearty. He achieves great feats of lucidity in many passages, even at times to the point of arousing the false hope that his volume will be easy reading. When the light grows dimmer, as it does sometimes, one feels that it is because there are some unstated though doubtless important problems being resolved by implication, that there is matter to be read between the lines to which one has no clue ...."
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> [27]

The countless number of times when 'false hopes of easy reading' have been aroused in me, when tackling a new book by Hicks, never raising alarm bells simply because each of the books conveyed that 'mellow and expansive mood' and lulled me into yet another somnambulant confidence that mastery of a difficult subject was close at hand.

Yet, the first three of his more important books received vitriolic reviews[37] - even for the style and the tone they contained and conveyed - by eminent

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[37] Successful books have, not infrequently, received less than generous reviews by eminent contemporaries. Whether there is a particular pattern to be detected by an exhaustive case study of a decent sample, I do not know. My mind goes back, almost routinely, to Stigler's review of Samuelson's **Foundations of Economic Analysis** ([**70**]; interestingly, *JASA* reviewed Samuelson's book *twice*, an earlier one by Gerhard Tintner appearing in the issue previous to the one in which Stigler's appeared - and, in its conclusions, more diplomatically couched, but almost equally negative!) and to Pigou's ultra-brief and largely negative review of Wicksell's 'Lectures' (Vol. 1), in the **Economic Journal** in 1913:

> "In general character it is a critical exposition rather than an independent contribution to learning. .... The somewhat laborious character of the exposition, coupled with the general familiarity of the ground covered, makes it unlikely that this new text-book will find many English readers - unless, indeed, an English as well as a German translation is produced."
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> [60], pp. 605-6.

theorists: Gerald Shove on *The Theory of Wages*; Oskar Morgenstern on *Value and Capital* ([59]); and Erik Lundberg on *CTTC*. Indeed, Lundberg's remarkable comments on *Value and Capital*, in his review of *CTTC* suggest something close to total incomprehension of a book that owed much to 'Swedish' concepts, tools and method[38]:

> "[H]is *Value and Capital*, [is] to my mind a much overrated book.
> Its sterile problems and its dead logic have already bored to tears
> ten generations of students and a generation of teachers."
> [53], p.109

Then there are those who harp and carp and cavil about the lack of attention to this or that work on the same subject; or the lack of references to anyone since Pareto or Walras or Thornton or Ricardo. These are the latter-day reincarnations of that nitpicking Subuthi who cavil about Sraffa reinventing the von Neumann wheel or Goodwin paraphrasing Felix Klein without acknowledgement. In the case of *CTTC* it may well be remarkable that there are no references to Schumpeter's monumental *Business Cycles* or to Kalecki's *Theory of Economic Fluctuations* or even to Hayek's writings on the *Cycle* which had inspired Hicks to write his very first article on *Business Cycle Theory* (cf. [29] and, above all, the absence of serious references to Robertson's two classics. Johan Åkerman and Erik Lundberg point out, caustically, and Richard Goodwin in a mild mannered way, the absence of any reference to Schumpeter and Lundberg, (ibid, p. 109), laments:

> "It is unfortunate and typical of Hicks' early 'static isolation',
> that in his book [*CTTC*] he does not even mention the name of
> Schumpeter, the man who since 1910 devoted the larger part of his
> great research work to the study of the cycle as a consequence of
> economic development (expansion)."

I recall, with pleasure, Hicks' lecture ([39]) on the occasion of the celebrations to commemorate the fiftieth anniversary of Ohlin's papers that codified, for the 'outside world' the work that was being done in 'Stockholm'. The title of the lecture was: *The Swedish Influence on Value and Capital*. My fading memories, even at that time, was still able to remember that *Value and Capital* had absolutely no references whatsoever to any of the more important works of the Swedes who had influenced Hicks, right through the 30s: Lindahl, Myrdal, Ohlin, Hammarskjöld and, no doubt, Lundberg himself. But I also remembered that over the many subsequent years he had found ways to pay tribute to the

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Pigou must, surely, have been deceived by Wicksell's humble style whereby he went out of his way, usually, to find predecessors to whatever he discovered or devised. Wicksell, Hicks, Samuelson - at the hands of Pigou, Stigler, Morgenstern; and many prior and subsequent classics have been savaged in equal measure by equally eminent luminaries, almost contemporaneously. I suppose the lesson I have drawn is that reviews are best left for a later stage in the life of a book; let a book make its own way and be at the mercy of the larger audience and not be subverted by the idiosyncrasies of singular reviewers!

[38]Perhaps *because* of it?

way their influences had been decisive in the development of his own thought and writings - in particular, the influences of Lindahl and Myrdal. A future historian of economic thought would find meticulously detailed documentation and indications of the exact influences, almost with precise datings, of 'Swedish influences', not just on '*Value & Capital*', but on the genesis and evolution of Hicksian contributions to Monetary, Capital and Methodological issues over the whole span of his life. The same story could easily be repeated, with exact and detailed references, for the influences of Robertson, Keynes, Hayek, Hawtrey and, of course, the continental neo classical masters, Edgeworth and Marshall. That Lundberg and others find it mysterious that a book on *Trade Cycle Theory*, written scarcely over a decade after Schumpeter's monumental two volume *opus* on the same subject, does not refer to it - or to many of the other classics of the 30s except, of course, to Harrod's book - does seem justifiable[39]. But as Hicks acknowledged in the 'Preface to the Third Impression' of *CTTC (p.v):*

> "It is an excercise in a particular method, and if I were to adopt a different method ...... I should have to write a different book."

Moreover, even in the 'Prefact' to the original version of *CTTC*, Hicks was explicit on the 'provisional' nature of the contents of the book:

> "Even on the purely theoretical side, I am very conscious that much remains to be done. If *a provisional answer* is given to the main question, that answer raises further questions, and many of these are left unexplored. The main argument itself has got some weak links, which need strengthening. ..... At the point where I leave it, the inquiry looks like branching out in many directions. That is a good point at which to write *a progress report, which is all that this 'contribution' claims to be.*" *CTTC*, p.xi; italics added.

A 'progress report on an exercise in a particular method' and an implicit catalogue of unexplored questions suggests, in my opinion, a challenge to extend the method and attempt to answer the unanswered questions - using, if necessary, the wisdom of those whose contributions were not harnessed in *CTTC*, such as Schumpeter and Lundberg, or those whose work on trade cycle theory were given only a casual nod, such as Kalecki and Tinbergen. Legions of imaginative non-linear trade cycle theorists have used *CTTC* in exactly this

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[39]My own lack of mystification on these fronts may have had a great deal to do with the fact that I read Goodwin's review of *CTTC before* I became familiar with the book itself. Goodwin's opening paragraph, after listing five of the significant theoretical 'accomplishments' of *CTTC*, goes on to observe that ([19], p. 316; italics added):

> Such a theoretical offering, so well written, in such narrow compass (168 pages), for so little ($2.25), is most welcome, especially if we compare it with the ponderous, uninspired, expensive texts which are continually being hurled at us. ..... *Professor Hicks launches his model with a humility rare amongst economists* ..."

constructive way, to extend economic methodology in its conceptual, mathematical and empirical frontiers, as I have tried to indicate, albeit incompletely, in this paper.

There is another aspect to this business of lack of reference to all and sundry. It is not very likely that I would read in a book by Lucas that 'he could kick himself for not having seen it before', say in Schumpeter; Hicks was 'kicking himself' for 'not having seen' what Harrod helped him see (*CTTC*, p.7). This is the style that Harrod so poignantly outlined above. One reads Hicks and does not wonder why he does not refer to Schumpeter or Lundberg; one takes part in a dialogue and leaves, at the end, with a cleared mind and fresh attitudes to old problems, perhaps even with one or two answers to them. The pleasant conversations of daily life are not about footnotes; they are about 'dis & dat', without that proverbial 'rhyme or reason'.

What is the moral of the story - of not paying adequate attention to prior work? I had the personal experience of asking Hicks whether, in writing *Capital and Time*, he had prepared himself first by going back to look at Böhm-Bawerk and Irving Fisher's *Rate of Interest*. His answer was illuminating. He told me that he no longer had easy access to such works and his style of working was, as it always had been, to read the classics but then put them aside and think about a problem that preoccupied his mind at any particular point in time with such readings as the backdrop. What he wrote on any particular topic would, of course, have been coloured by what he had read but, on the whole he would try to work out the solutions to the problems he had posed himself on his own and with his own intrinsic resources. But, later, when the time came to reflect upon his own work and go beyond it, he would try to organize the influences that had inspired a particular method of attack or a particular way of viewing history. I was witness to the same kind of method of working by Goodwin. This is, of course, quite the opposite to my own method of working; I am a lesser mortal and rely heavily on the work of others, not only for the problems I pose for myself but also for finding methods to solve them. I am rarely able to formulate original problems and even more rarely to find solutions to them or if, indeed, I did find solutions, more often than not, do not recognize them as solutions without help from others. Hence I am forced to pepper my writings with umpteen references, in which poor readers are drowned, as witness this very paper!

Thus, in re-reading several of the writings on Trade Cycles by Hicks written by him over a period of almost 50 years, I recall his own words in *Capital and Time* ([32], p.v):

> "[W]herever one starts, it is hard to bring more than a few [aspects of a large subject] into view. It is just as if one were making pictures of a building; though it is the same building , it looks quite different from different angles. As I now realize, I have been walking round my subject, taking different views of it. Thus that which is presented here is just another view, it turns out to be quite useful

in fitting the others together."[40]

It has helped me avoid rash conclusions about inconsistency or incoherence in Hicksian writings on Trade Cycles. Even more importantly, his remarkably humble 'confession', in the *Festschrift* to Georgescu-Roegen ([34], pp. 299-300; italics in the original), was crucial for me to remember whenever I felt carried away by mathematical niceties and tended to forget the economic and historical contexts within which, and from which, Hicks was 'making pictures of a building':

> "For my part, I am very ignorant of science[41]; though I have dabbled in mathematics my spiritual home is in the Humanities. It is because I want to make economics more human that I was approaching the task from that end and I am content with a more earthy way of going about it. ... It is the *new* things that humanity has discovered which makes its history exciting; and the new things that may be found in the future, before humanity blows itself up, or settles down to some ghastly 'equilibrium', make a future worth praying for, and worth working for."

In all my many readings of *CTTC* I have always found it a refreshingly 'earthy' book, capable of being polished in various ways - sometimes with newer mathematics; at other times by deepening the tentative conceptual definitions of economic ideas or institutions; at still other times by reflections on method and methodology. In each of these ways *CTTC* and the other Hicksian *Visions and Vignettes on Non-Linear Trade Cycle Theories* has furnished macrodynamic theorists with suggestions for the 'new things that may be found in the future' and for avoiding settlements in 'ghastly equilibria', whilst squarely staying within the fold of the humanities.

## A    A Brief Postscript on Business Cycle Methodology[42]

> "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

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[40] Although Hicks, in this passage, is talking about his changing views of *capital*, I have come to believe that this particular attitude permeates his methodology and philosophy of theorizing in economics in general.

[41] That the author of *Causality in Economics* ([35]) can 'confess' to be 'ignorant of science' is severely humbling to contemplate.

[42] This appendix was 'inspired' by two events that transpired during the Hicks Centennial. The first was the assertion, without any kind of substantiation, that any kind of modelling of dynamics requires the use of differential equations or jump or switching dynamics (of the sort implied by the Pontryagin maximum principle) by Christopher Bliss, in his paper at the 'Workshop'. The second was, of course, the announcement of the 'Nobel' award in economics for 2004. The Bliss assertion is simply false; the 'Nobel' award was only sad.

theory of *erratic shocks*[43] ... does not explain *enough*."
     *CTTC*, pp. 90-1; italics in the original.

In no uncertain terms, based on lucid economic and mathematical reasoning, Hicks pointed out (*CTTC*, 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 essay: *non-linear theory*. But even as I was delivering this paper on 11 October, 2004 in the grand seminar room of the *Cassa di Risparmio di Bologna* there was the annual announcement from Stockholm being broadcast to a world-wide audience of economists and others celebrating work that had, for the past three decades[44], extolled the virtues of 'erratic shocks and damped fluctuations'.

The first 'Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel'[45] was shared by Tinbergen and Frisch in 1969. The citation for Frisch stated that he was awarded the Prize 'for having developed and applied *dynamic models for the analysis of economic processes*'. Now, 35 years later, on the closing day of the Hicks Centennial, we read that the 2004 Prize was to be shared by Prescott with another Norwegian, Finn Kydland, and that they were awarded it 'for their contributions to *dynamic macroeconomics*: the time consistency of economic policy and *the driving forces behind business cycles*'. 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'. That much maligned metaphor was incorrectly attributed, by Frisch (cf. [14], footnote 5, p.178) to Wicksell's famous lecture in Oslo, to the Stats$\phi$konomisk F$\phi$rening, on May 6, 1907 (cf. [81]). No amount of fine-toothed combing of that fine lecture will unearth any reference to a rocking (or, more appropriately, an unrocking[46]) horse. Wicksell invoked the metaphor of the rocking horse in a review of an obscure and best-forgotten book titled

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[43] Or, in Richard Day's more felicitous, if slightly less complementary, phrasing: the theory of '*ad-hoc shockeries*' (cf. [5], p. 180).

[44] Counting the years since the birth of what I have always called 'Lucasian Macroeconomics', rather than new classical macroeconomics, from about 1975.

[45] 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 & 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 to honour and celebrate excellence in economics!

[46] Zambelli ([84]) 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 - and I am not referring to Frisch in this case.

*Goda och Dåliga Tider*[47] by a long-forgotten minor Swedish economist by the name of Karl Petander (cf. [82], p. 71, footnote 1)[48].

Hicks was, of course, not alone in finding the 'the theory of damped fluctuations and erratic shocks unacceptable'; the names that I have invoked in the pages of this paper are a testimony to that fact, as well as the many who have worked out a non-linear theory of the trade cycle without relying on Hicksian economic foundations.

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 to depict possible worlds in starkly contrasting colours; his was a world of shades and many colours and this was so even in the theory of the trade cycle. Even although he opted for the alternative of theorising without reliance on *ad-hoc shockeries*, he did add the characteristic caveat (*CTTC*, 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. 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 deterministic randomness[49].

An elaboration of such issues will take me beyond the limits and themes to which I confined this essay. However, it was necessary to make this brief excursion, a little beyond the stipulated limits and themes of the paper, in view of the inopportune announcement in Stockholm and the shadow it cast on the underlying methodology of *CTTC*.

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[47] 'God and Bad Times'.

[48] 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 (ibid; italics in the original):

> "Om man slår på en gunghäst mede en klubba, så bli gunghästens rörelser mycket olika klubbans. Stöten är *orsaken* till rörelsen, men föremålets egna jämiktsbetingelser äro *förutsättningarna* för rörelsens *form*.

[49] To be clearly distinguished from 'deterministic chaos'.

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