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Birth and Early History of Nonlinear Dynamics in Economics

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Abstract

Since the 1980s, nonlinear dynamic modelling is becoming a popular methodology in economics. However, it is not as new as many researchers seem to believe. Before the linear approach dominated economic theory around the 1950s, many economists were actively involved in the development of nonlinear models, this tendency being particularly strong during the period 1930-1950. The main objective of this essay is to offer a systematic and comprehensive survey of the early developments in nonlinear dynamics in economics, ranging form Frisch's original impulse and propagation model in 1933, to Goodwin's formalisation of the limit cycle in 1951.

Key words: nonlinear modelling, economic cycles, macrodynamics, endogeneous/exogenous fluctuations, history of thought 1930-1950.
JEL classification: B2, N1

Resumen

Desde comienzos de los '80, la elaboración de modelos no lineales se está volviendo una metodología cada vez más popular en economía. Sin embar-

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go, no es tan novedosa como muchos investigadores parecen creer. Antes de que el enfoque lineal dominara a la teoría económica alrededor de los '50, muchos economistas se encontraban comprometidos con el desarrollo de modelos no lineales, especialmente durante el período 1930-1950. El principal objetivo de este ensayo es proporcionar una revisión sistemática de los desarrollos pioneros en dinámica no lineal en economía, desde el modelo original de impulso y propagación de Frisch en 1933, hasta la formalización de Goodwin del ciclo límite en 1951.

Palabras Claves: modelos no lineales, ciclos económicos, macrodinámica, fluctuaciones endógenas/exógenas, historia del pensamiento 1930-1950.

Clasificación JEL: B2, N1

I. NONLINEAR MODELLING IN ECONOMICS: OLD WINE IN NEW BOTTLES?

Nonlinear dynamic modelling is becoming a popular methodology among economists doing research on various areas such as market dynamics, growth, financial markets, social networks, regional economics and environmental economics among others. Since the 1980s, there has been an explosion in the literature on nonlinear dynamics including chaos theory and, more recently, complexity theory[1]. The reasons for such an interest are varied, but contributors usually mention: a) the limitations of standard (neoclassical) models which seem to be incapable of addressing certain features of economic reality, b) the disappointment with traditional linear stochastic models which do not perform quite well empirically, and/or c) a growing awareness that the internal dynamics of an economy follows a very complex behaviour which is endogenously generated (see e.g. Arthur et al. 1997; Day and Chen 1993; Colander 2000a, 2000b; Rosser 2004).

Many reasons have been posited to explain the boom in nonlinear modelling over the last two decades. Philip Mirowski (1998, 2002), for instance, is well known for his thesis that all sciences, and in particular economics, are on their way to become 'cyborg sciences'. Others argue that nonlinear modelling has been successfully applied within natural sciences like physics, chemistry or evolutionary biology, and therefore it is only natural for economics to follow the path opened by its more prestigious sisters (Anderson 1988, Foster 1997). In addition, many authors suggest that the

[1] For a survey see Kiel and Elliott (1998), Arthur et al. (1997), Rosser (2004) and Perona (2005).

key factor explaining the rise in nonlinear dynamics in economics has been the massive increase in computing power during the second half of the twentieth century.

In a sense all the above arguments are right. In this paper, however, I would like to focus on a rather different aspect. In my view one of the most interesting features of nonlinear dynamics in economics is that it is, in fact, not new. Indeed, before the linear approach dominated economic theory around the 1950s, many economists were actively involved in the development of nonlinear models, this tendency being particularly strong during the period 1930-1950 (Baumol and Benhabib 1989, Day 1993, Tvede 1998). Certainly, this inclination was associated with the rise in macroeconomics and, in particular, with the study of business cycles and fluctuations, a theme which was, at the time, high on the economists' agenda. Dynamic analysis was thus closely related to the rise and expansion of macroeconomics[2], to the extent that it was common to speak of 'macrodynamics', a term coined by Frisch as early as in 1933.

It is in effect understandable that the study of economic cycles flourished during the first decades of the twentieth century: the central economies were experiencing strong fluctuations and instability, especially over the inter-war period. A large number of articles – published in the first issues of many well-known journals – were devoted to the discussion of the problem of the trade cycle, showing the extent to what there was a need to understand this phenomenon. However, most of the very first papers on economic cycles were, in the opinion of some authors, literary discussions that could sometimes be vague and imprecise (Baumol and Benhabib 1989). It was not until the 1930s that the first attempts to give a more formal expression to aggregate fluctuations were undertaken.

In a nutshell, the history of macrodynamic models of the business cycle started with the most simple dynamic theories describing the path towards a stable equilibrium. Those simple models were followed by some more complicated ones, with equilibrium being achieved after a process of oscillatory adjustment. Eventually, explicit nonlinear models leading to limit cycle solutions appeared for the first time in economics. The long-lasting

[2] On the contrary, microeconomics was slower to incorporate dynamic features due to the dominance of the (neoclassical) general equilibrium framework, which was static in nature and therefore, not quite amenable to dynamic analysis (Nagatani 1981). In the early times, the only attempts to apply dynamic analysis to microeconomics were some simple cobweb models showing the process leading to market equilibrium. Today both linear and nonlinear models are thriving in various traditionally microeconomic areas, such as the dynamic theory of markets, or game theory.

debate about the exogenous/endogenous origin of fluctuations has always been in the background of this evolution. On the one hand, the attempts to model cycles using linear models – where fluctuations were artificially generated through the introduction of time-lags – supported the exogenous position. On the other hand, nonlinear modelling emphasised explicitly the endogenous nature of cycles.

The main objective of this paper is to provide a comprehensive survey of these early developments in nonlinear dynamics in economics. I believe that this is an important task for at least two reasons. First, it shows clearly that nonlinear modelling in economics is not as new as many scholars unaware of the history of the discipline seem to believe; in effect, nonlinear models have been around for quite a long time before they were 'rediscovered' by supporters of chaos theory in the early 1980s. Second, it serves to illustrate the different motivations of economists throughout the century: whilst the early developments in macrodynamics emerged as a response to the necessity to understand contemporary economic phenomena, today's interest in nonlinear dynamics is mostly associated with methodological needs, i.e. with an attempt to find more sophisticated tools of analysis, which can address the limitations in traditional methods without relinquishing the ideal of formalisation.

In addition, the story of the birth of nonlinear dynamics in economics leads naturally to a historical question: why did nonlinear models practically disappear between 1950 and 1980 and had to be re-introduced in the 1980s by borrowing them from the natural sciences? Although the answer to this question is beyond the scope of the present essay, I will briefly sketch an (evolutionary) explanation towards the end of the last section. For the time being my intention is just to carry out a systematic revision of early macrodynamic models, to remind us of the roots and the (forgotten) fathers of nonlinear modelling in economics.

The structure of the paper is as follows. Section II deals with the original linear models of Frisch and Samuelson, who were among the first economists searching for mathematical expressions to describe fluctuations. At the same time other economists, such as Kalecki and Harrod, were trying to explain the mechanics of business cycles by taking into account a number of endogenous factors influencing macroeconomic behaviour. The endeavours leading to the emergence of the first (implicit) nonlinear models are examined in section III. Section IV is devoted to the discussion of some pioneering works in nonlinear dynamics in economics. The first economist to notice the importance of introducing nonlinearities to explain economic fluctuations was Kaldor in 1940. Ten years later, in his famous theory of the trade cycle, Hicks introduced two basic nonlinearities in the investment function. Finally it was R. Goodwin who developed a formal model of the limit cycle in 1951. Section V concludes with an overview of the reasons explaining why nonlinear modelling in economics came to an end in spite of previous successes.

II. The original (linear) dynamic models of the cycle

II.a. Frisch's pioneering work

The first attempts to come up with a mathematical expression to describe economic fluctuations were carried out using linear models. By the late 1920s, mathematical models involving a time dimension were very simple; in general, they merely described elementary adjustment processes towards an (either stable or unstable) equilibrium, which was usually done by employing a first order linear difference equation. It soon became apparent that these models could only reproduce non-cyclical behaviour, and thus were not suitable to represent the fluctuations observed in the real economy. However, second order linear difference or differential equations could produce cycles to some extent, and it is in this line that the initial efforts were directed. Two prominent theoretical devices developed at that time, i.e. the keynesian multiplier and the acceleration principle, also contributed to render second order linear processes a likely alternative to start modelling oscillations.

On the empirical side, the most active area of research in economics (preceding the birth of econometrics in the 1930s) was the construction of simple time series models. Nevertheless, the ad-hoc nature of those models was heavily criticised by many researchers doing empirical economics, who insisted on the importance of adopting a 'structural' model-building strategy, in order to develop systems of equations capable of describing the functioning of the whole economy. In other words theory, rather than data analysis, should be given priority. Both visions, however, – namely the early works in the time series tradition, notably the works by Yule (1927) and Slutsky (1927), *and* the approach centred on the theoretical (structural) aspects of the business cycle – contributed to the rise of dynamic modelling in economic theory during the early 1930s (Hendry and Morgan 1995).

One of the seminal papers in this vein was Frisch's 1933 *"Propagation problems and impulse problems in dynamics economics"*. According to Frisch, the study of cycles was essentially dynamic[3], and could be decomposed into two problems: propagation and impulse. To describe the propagation mechanism, he used a very simple structural framework, consisting of the following equations:

Investment equation:

$y = mx + \mu\, dx/dt$ (1)

Consumption equation:

$dx/dt = c - \lambda w = c - \lambda(rx + sy)$ (2)

where:

y = annual production of capital goods

x = annual production of consumption goods

w = rx + sy = demand for money [4], representing the cash needed for the purchase of consumption and capital goods, as a constant proportion (r and s) of their production

λ = positive parameter

c = autonomous component of consumption

m = coefficient reflecting the demand for capital goods due to direct and indirect depreciation [5]

μ = direct and indirect requirements of capital goods due to the change in the production of consumption goods

Equation (1) simply states that the demand for capital goods is a demand for reposition plus a demand due to the rate of change in the

[3] Frisch (1929) defined 'dynamics' as the analysis of variations comparing one point in time to the next.

[4] Called by Frisch, following the Walrasian terminology, the *encaisse desirée* (Frisch 1933:179).

[5] In Frisch's words:

> the constant *m* represents the *total* depreciation on the capital stock associated with the production of a unit of consumer's goods, when we take account not only of the *direct* depreciation due to the fact that fixed capital is used in the production of consumer goods, but also take account of the fact that fixed capital has to be used in the production of those capital goods that must be produced for replacement purposes (Frisch 1933:176; his italics).

production of consumption goods. It is therefore a version of the accelerator, an idea which Frisch borrowed from Clark. In turn, equation (2) states that the rate of change in consumption is always positively related to an autonomous component 'c', which may increase or remain constant over time. The second term in the right hand side represents a countervailing effect, since the demand for money 'w' can only grow up to a limit given by the total stock of money, which cannot increase indefinitely. Therefore, this factor introduces a tension in the system: dx/dt grows at a decreasing rate and whenever the limit is reached, consumption falls and consequently, the demand for new capital goods falls as well.

Indeed, Frisch was thinking of a cyclical process. He actually drew in his paper (1933:178) a graphic showing the oscillations in his model's main variables, that is, consumption and capital production. However, because of the way in which Frisch formulated the equations, he failed to find a mathematical expression for the cycle he had in mind. Substituting (2) into (1) for dx/dt, the system yields a linear relationship between 'x' and 'y'. Conversely, by replacing (1) into (2) for 'y', it is possible to end up with a first order differential equation in 'x', which yields an exponential (convergent or explosive) solution for the two variables involved.

Frisch concluded that the system was too simple to generate oscillations and thus it was necessary to make it more general. He did not realise, however, that the system he had in mind was a nonlinear one. Had he included the money restriction into his equations, he would have found that the system produced endogenous oscillations. But even for him – whose analytical skills were notorious – some notions were beyond the mathematical knowledge that economists possessed at that time.

Faced with this situation, Frisch thought of several courses of action to make his (linear) system more complex. One possibility was to accept that, at a point in time, savings and investment could differ[6]. There was a simpler way to amend the system in order to produce oscillations though, which was via the introduction of time-lags in the investment process. Frisch opted for the latter solution, following

> Aftalion's point of view... [consisting] in making a distinction between the quantity of capital goods whose production is *started* and the

[6] This idea was later adopted by most economists engaged in the modelling of the trade cycle.

> activity needed in order to *carry to completion* the production of those capital goods whose production was started at an earlier moment (Frisch 1933:181; his italics).

Hence, he introduced a third equation into the system as follows: Carry-on-activity function:

$$z_t = \int_{\tau=0}^{\infty} D_\tau \, y_{t-\tau} \, d\tau \qquad (3)$$

meaning that the amount of production of capital goods or 'carry-on-activity' at time 't' (represented by z_t), is the activity needed to continue with the production of capital goods started in previous periods. The fraction of production to be completed in each period is D_τ, called the 'advancement function'. Assuming a constant amount of carry-on activity: $D_\tau = 1/\varepsilon$ (for $0<\tau<\varepsilon$) and $D_\tau = 0$ (for $\tau \geq \varepsilon$).

Differentiating in (3):

$$\varepsilon \, dz/dt_t = y_t - y_{t-\varepsilon} \qquad (3')$$

Combining (1), (2) and (3'), and replacing 'y' by 'z' in (2), Frisch obtained a mixed differential-difference equation system. It is easy to show that this system can be expressed as a higher order linear equation and therefore, it is capable to produce cycles so long as the roots are complex numbers. Frisch then investigated the solution empirically, substituting the structural coefficients in his model by some plausible values of the parameters. The result was that, in effect, the variables displayed a damped oscillatory behaviour, and he was able to identify three waves (whose mean duration was approximately 8.6, 3.5 and 2.2 years respectively), corresponding to long and short economic cycles.

However, Frisch's ingenious model suffered from a fatal weakness, namely the fact that the oscillations brought about by his propagation mechanism were damped; in other words, cycles died out in a finite time. To sort this problem out, Frisch borrowed an idea from Slutsky, who had shown that cycles could be represented by means of the accumulation of random shocks. It was at this point that he decided to add an exogenous impulse to his propagation mechanism:

> one way which I believe is particularly fruitful and promising is to study what would become of the solution of a determinate dynamic

> system if it were exposed to a stream of erratic shocks that constantly upsets the continuous evolution, and by so doing introduces into the system the energy necessary to maintain the swings (Frisch 1933:197)[7].

In short, the pioneering model advanced by Frisch was a deterministic linear model accompanied by random shocks, where the deterministic part (i.e. the propagation mechanism) combined a version of the accelerator with time-lags in the investment function. This makes Frisch's theory an interesting starting point to study the early nonlinear macrodynamic models because it is both rich in its implications and simple in its format. In addition, his essay of 1933 can be seen as a prominent landmark in the long-lasting debate about the endogenous/exogenous nature of macroeconomic cycles.

Following Frisch's seminal work, many other papers appeared which both criticised and extended some of its assumptions. Central to the criticisms was the fact that the model did not include any notion analogous to the keynesian multiplier. In the 1930s, this was considered an important omission. In effect, Frisch made his investment function react to changes in consumption, but the model did not allow for any impact the other way round, running *from* investment *to* consumption. Such an omission was intentional though: the author regarded consumption as a totally exogenous variable, determined only by custom and institutions on the one hand, and the liquidity constraint on the other. In his view, the indirect effect of an increase in output due to the initial growth in investment played no significant role in the economy.

This was clearly in contrast to the vast majority of models in the aftermath of the keynesian revolution, which usually combined different versions of the multiplier and the accelerator to explain macro cycles. In his survey on business cycles, Zarnowitz emphasises this point:

> the 1930s and 1940s saw a proliferation of formal models of essentially endogenous cycles in aggregate output, which use various versions of the investment accelerator and the consumption multiplier and let the two interact (1985:539).

It is interesting to notice that the reluctance by Frisch to include something like the Keynesian multiplier prevented him from building a truly

[7] To make his point, Frisch used an analogy with the oscillating pendulum as an intuitive justification. Although he did not include an explanation as to what causes the exogenous shocks in his model, in the last section of his paper he briefly refers to Schumpeter's ideas on innovation.

endogenous model of the cycle. This point was made by Thalberg (1990) who explored the model in depth and performed several estimations for different values of the parameters. He demonstrated that, as Frisch had shown sixty years earlier, oscillations were heavily damped for any parameter specifications. Also and more importantly, Thalberg showed that it was the failure to include any feedback effect from investment to consumption the reason why the model was inevitably stable, since condition (2) prevented the system from exploding. Had the multiplier been included, the system could have displayed more complicated behaviours[8].

II.b. Samuelson's generalisation

A second relevant landmark in the history of early macrodynamics in economics is Samuelson's article of 1939, thought to be the most elegant representation of a multiplier-accelerator model. Samuelson gave mathematical form to Hansen's intuitions, being his model – except perhaps for the inclusion of the accelerator[9] – completely keynesian in nature. His system also consisted of three equations:

$$Y_t = g_t + C_t + I_t \qquad (4)$$

$$C_t = \alpha Y_{t-1} \qquad (5)$$

$$I_t = \beta\,(C_t - C_{t-1}) \qquad (6)$$

where:

g_t = government expenditure

C_t = consumption

I_t = induced private investment

Y_t = national income

a, b = positive parameters representing the propensity to consume and the 'relation', respectively (see footnote 9)

In this case, it is straightforward to solve the system in order to obtain a second order difference equation:

$$Y_t = g_t + \alpha(1+\beta)Y_{t-1} - \alpha\beta Y_{t-2} \qquad (7)$$

[8] Thalberg shows this fact using a numerical example (1990:110-11).

[9] Samuelson (1939:75) called the accelerator the 'relation', representing it by b.

Depending on the values of a and b, (7) will have real or complex roots leading to exponential or cyclical behaviour (either explosive or damped), respectively. Samuelson then found the mathematical expression for the boundaries of (α,β), corresponding to the four regions in which the variables exhibit different sorts of qualitative behaviour.

Samuelson's model was, then, a more general representation than that of Frisch, and a benchmark to which all subsequent theories would refer to. There was a crucial difference between the two models though. Whilst Frisch had rendered his system dynamic by means of lagging the investment function – something that *a priori* seemed to be a plausible description of the workings of the real economy –, Samuelson introduced time-lags in his model in a much more unrealistic fashion, since there was no substantial economic justification for the particular form adopted in (5) and (6). The advancement function used by Frisch was certainly ad-hoc (he assumed D_τ constant), but he had in mind the more general meaning of the carry-on-activity function.

The only reason why Samuelson could have chosen such a representation, is that he was looking for a simple and elegant way to transform a linear system in a second order difference equation, capable of producing cycles for certain values of the two parameters involved. The dynamics displayed by his model were thus mostly mechanical and devoid of empirical meaning. Relevance had been sacrificed for the sake of mathematical tractability, something that would be raised as a criticism against all time-lag models of the cycle in the years to come.

III. THE TRANSITION TOWARDS NONLINEAR MODELLING

III.a. Kalecki's intuitions about the limit cycle

Samuelson's multiplier-accelerator model made it apparent that the use of second order linear systems to model fluctuations was limited. In effect this sort of model could produce only four types of time path: a) oscillatory (stable or explosive) and b) non-oscillatory (stable or explosive), plus two particular cases including a stationary equilibrium and a cycle of constant amplitude. Being the latter an unstable equilibrium, it could not be considered as a realistic possibility. Explosive cases were ruled out as well, both on empirical *and* theoretical grounds, since such behaviour would violate the linear approximation (see Puu 1993:3). As a result, it was concluded that if such a type of linear system was used to model cycles, it could only realistically represent damped oscillations. Consequently the only way to explain sustained fluctuations was to accept the existence of some sort of exogenous shock to keep the cycle 'alive'[10].

What about higher order systems? Perhaps second order difference equations were still too unsophisticated to model the upturns and downturns of the real economy. The truth is that higher order systems fared no better than second order ones. In Baumol and Benhabib's words:

> it was soon recognized that linear equations of even more complex (that is, of higher order) than Samuelson's would not generate any time paths basically different from these four. This range of possible time path configurations simply was not sufficiently rich for the economists' purposes, since in reality time paths are often more complicated and many oscillations do not seem either to explode or dampen toward disappearance (1989:79).

The perceived limitations in linear systems paved then the way for more complex (i.e. nonlinear) exercises in dynamic modelling.

One of the leading economists taking a pioneering step in this direction was M. Kalecki, who offered an interesting and innovative interpretation of the mechanisms describing the functioning of the economy of his times. Kalecki presented his macrodynamic study at the meeting of the Econometric Society in 1933, and a mathematical version of this paper was published later in the third volume of *Econometrica*. However, it was only in another paper of 1937, that the author's ideas on cycles were intuitively explained.

At first sight, Kalecki's model seemed to be unquestionably different from the linear models developed by Frisch and others, since it looked like a truly endogenous model capable of displaying sustained oscillations (see below). For Kalecki the main reason for the existence of cycles was, once again, the nature of investment in a capitalist economy, in particular the time-lag between investment decisions and the effective production of capital goods. However, the *mechanism* through which the process operated was very different from Frisch's. As it was pointed out in II.a., the production of capital goods was, for the latter, determined by changes in consumption (i.e. Clark's accelerator), whilst the multiplier effect was largely absent. Kalecki, on the contrary, did not rely on the acceleration principle to explain fluctuations and, although he explicitly acknowledged the keynesian multiplier,

[10] This shows the connection between linear models and the so-called exogenous approach.

this device played no significant role in his explanation of cycles. Kalecki, in fact, had in mind the notion of a 'limit cycle'.

Essentially, his theory stated that the rate of investment decisions 'D' is a positive function of the gap between the prospective rate of profit – which depends on expectations and supply prices of investment goods (p_k) – and the rate of interest – influenced by lenders' confidence as well as the money market. Both the prospective rate of profit and the rate of interest are in turn a function of investment 'I' and the stock of capital 'K'. Therefore: $D = f(I, K)$. Once investment decisions are made, the effective production of capital goods lasts for several periods at the end of which they are added to the stock of new capital. Meanwhile, new investment decisions are made and new projects are undertaken during each period, so that the process is repeated continuously.

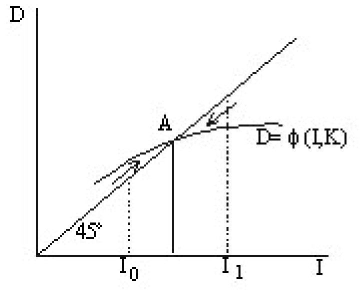
Kalecki illustrated the nature of the cyclical process with the aid of two graphs[11]. For a given stock of capital, the function D is as shown in Figure 1. A small increase in investment has a positive impact on expectations and p_k as well. The final effect on the prospective rate of profits is thus not clear, but the model assumes that starting from a low level of investment – such as I_0 – the first effect will predominate and the prospective rate of profits will grow. With respect to the rate of interest 'r', a small increase in investment will raise the demand for money on the one hand, and have a positive effect on lenders' confidence on the other. Again, the final impact on 'r' is not clear, but starting from I_0 it is likely that it will fall. Therefore, at a level of investment such as I_0, we have $D_0>I_0$. In the following (theoretical) period, investment decisions are carried out so that $D_0 = I_1$, and so on until the point where D=I is reached. Conversely, starting from a high level of investment I_T, the increase in p_k will predominate making the prospective rate of profits fall, whilst the interest rate will be growing due to the monetary restriction. Therefore $D_T<I_T$ and investment will drop until D=I. So far, the process described can be represented by a standard first order linear difference equation, converging to a stable equilibrium.

It is only when changes in the stock of capital are allowed for, that the model's cyclical nature is rendered evident. Kalecki explained the process as follows. For a constant level of investment, the function D shifts downwards whenever K increases. The reason is that, if investment does not change neither does income, and thus a higher stock of capital is associated with

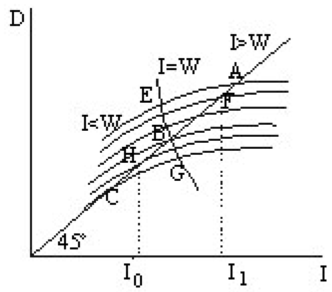
[11] For a thorough explanation see Kalecki (1937).

a constant level of income, meaning that the prospective rate of profits has to be decreasing (for every I, D is lower).

Figure 1 **Figure 2**



Source: Fig. 3, Kalecki (1937:88).



Source: Fig. 8, Kalecki (1937:95).

In addition, Kalecki introduced another function, namely the curve I=W. This function shows that for every K there is a level of investment W which keeps the economy's capacity constant (i.e. accounts for depreciation). If I>W, K is increasing and D will shift downwards; if I<W, investment is insufficient to replace existing capital and consequently K will fall and D will shift upwards.

Now it is easy to understand the mechanism that Kalecki had in mind in his endeavours to explain the origin of trade cycles. In Figure 2, beginning at I_0 (point H), D>I but at the same time I<W. Hence investment is raising fast, aided by the fact that the stock of capital is falling, what affects expectations and the rate of profits in a positive direction. Once point E is reached (I=W), it still holds that D>I, but as investment expands it becomes I>W, K grows, D starts shifting to the right, and investment increases but at a decreasing rate. Eventually at point F we find that D=I, but K is still growing and it soon happens that D<I. In this period investment is falling rapidly and hence there is a recession. Finally, beyond point G, D is still less than I and investment is diminishing, but its fall is partially remedied by the fact that the stock of capital is falling (I<W), and thus stimulating the recovery. In this fashion, the process goes on indefinitely.

To sum up, Kalecki's ideas on the limit cycle represented a fundamental contribution compared to previous (linear) models. Although he also explained the cycle by means of time-lags in the investment function, Kalecki was able, by contrast to Frisch or Samuelson, to model continuous oscillations. Nevertheless he could not complete his project, in the sense that he could not formalise his intuitions appropriately, meaning that nonlinear models of the cycle had to wait a few more years to be formally introduced in economics.

It is interesting (and sad) to know the details of Kalecki's failing to show his intuition mathematically. In his 1935 paper – when the essay presented at the Econometric Society was published – he attempted to carry out a mathematical representation of his model. However he made the mistake of choosing a *linear* form for the investment decision function (D) and thus ended up with a higher order linear system. A simplified version of Kalecki's mathematical representation is discussed below to illustrate the point. Assuming that there is only a one-period lag, it is possible to show that his system leads to a second order difference equation in D:

$$D_t = \alpha + \beta I_t - \gamma K_t \qquad (8)$$

$$D_t = I_{t+1} \qquad (9)$$

$$K_{t+1} - K_t = I_t - W_t \qquad (10)$$

Equation (8) is a linear version of the investment decision function $D = \phi\ (I, K)$. Equation (9) states that all investment projects are completed within one period time. Finally, equation (10) is the condition that the stock of capital be growing when investment exceeds its replacement level (net investment is positive). Making appropriate substitutions:

$$D_{t+1} - (1+\beta)D_t + (\beta+\gamma)D_{t-1} = \gamma W_t \qquad (11)$$

which is the second order analogue to equation [17] in Kalecki's 1935 paper.

Not only did Kalecki fall in the trap of linearity but, worse than that, when he was faced with the dilemma of explaining how a linear system producing damped oscillations could account for his intuitions about sustained cyclical processes, he made use of what seemed to be a bizarre argument. Essentially he *assumed* that the system behaved according to the very particular case of complex roots with a null real part (constant oscillations). Such assumption was soon pointed out and criticised by Frisch (1935), who attacked Kalecki in public and again remarked on the necessity of relying on exogenous shocks.

In fact, Kalecki did not discard the possibility of his model producing damped oscillations when he affirmed that "...clearly it is an arbitrary assumption that the moving point comes back to its initial position E – the trajectory need not be a closed curve but may also be a spiral" (1937:95). His point was, however, totally different from his predecessors: he wanted to show that the system was intrinsically unstable, emphasising the operation of endogenous forces in economic cycles. He was not sufficiently understood though.

IV. THE EARLY NONLINEAR MODELS

IV.a. Kaldor's first nonlinear macrodynamic model

Had Kalecki used a nonlinear specification for his investment decision function, he could have certainly found the way to model a limit cycle. It was only in 1940 when N. Kaldor, an economist who was aware of the relevance of Kalecki's work, became convinced of the necessity of employing nonlinearities to account for sustained fluctuations.

The model advanced by Kaldor was different from the previous ones in that the author did not make it dynamic by imposing a time-lag structure on the investment function. Due to the widespread acceptance of Keynesian ideas in those years, and being himself based at Cambridge and thus under their direct influence, Kaldor followed a different path. Essentially, his model was rendered dynamic by making savings (S) differ from investment (I), a course of action that was to be subsequently chosen by most economists modelling cycles. In effect it was observed that the discrepancy between ex-ante S and I, together with some version of the multiplier-accelerator, yielded a model that could produce – in a purely endogenous fashion – cyclical fluctuations in aggregate output. Let me discuss Kaldor's model a bit more in detail.

Given that S=I holds as an identity ex-post, Kaldor pointed out that if planned (ex-ante) investment exceeds planned (ex-ante) savings ($I^d > S^d$), then it must be either $I < I^d$, or $S > S^d$, or a combination of both. In this case output increases so long as there is an undesired fall in stocks and households are buying more consumers' goods due to the higher income. The opposite result occurs whenever $I^d < S^d$ and there is a reduction in the level of activity. Furthermore, Kaldor assumed that both I^d and S^d are functions of income:

$$I^d = f(Y) \qquad \text{with} \quad \partial I^d / \partial Y > 0 \qquad (12)$$

$$S^d = g(Y) \qquad \text{with} \quad \partial S^d / \partial Y > 0 \qquad (13)$$

Nevertheless he soon realised that if these functions were linear, there would be only one equilibrium which might be either stable or unstable, depending on their relative slopes. For instance if $\partial S^d/\partial Y > \partial I^d/\partial Y$ (S is steeper), then: a) $I^d>S^d$ to the left of the equilibrium point and output would be expanding towards its equilibrium level Y*, whereas b) $S^d>I^d$ to the right of the equilibrium point and output would be contracting until reaching the stable equilibrium Y* (Kaldor 1940).

In fact equation (12) – which states that I depends on the *level* of activity rather than the rate of change in Y – means that Kaldor's model did not include a proper version of the accelerator mechanism. Had he used an alternative formulation, he would have ended up with a second order linear differential system similar to Samuelson's. However, it was not necessary for him to follow such a procedure to realise that linear models were incapable of producing continuous oscillations and therefore the only possible way to generate them was using a *nonlinear* specification[12].

His model consisted of two continuous S-shaped curves describing the behaviour of savings and investment, which he believed were justified on empirical grounds, since the slope of I^d (S^d) should be smaller (larger) both for low and high levels of output. In addition, he maintained that when activity is high, investment is high too, and consequently the stock of capital in the economy is increasing. In this case the S^d curve shifts upwards – because people are consuming and saving more for every Y – and the I^d curve shifts downwards – because there are fewer opportunities for profitable investment. By drawing the two curves in a graph like the one shown below (Figure 3), Kaldor noticed that I^d and S^d intersected at three points: A, B (two stable equilibria) and C (an unstable equilibrium).

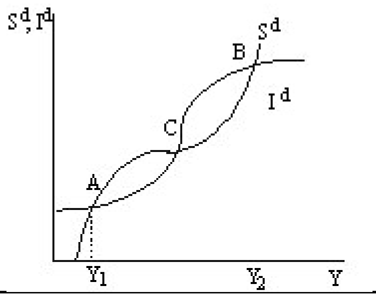
His explanation of the cycle runs as follows[13]. In an equilibrium like B, output is high (Y_2) and then S^d is continuously shifting to the left and I^d to the right; in consequence Y is contracting and B approaches C, the unstable equilibrium. When point C is reached, $S^d>I^d$ and a cumulative downward process is generated with income falling rapidly until A. Once in the new (stable) equilibrium A, the process goes the other way round. Income is very low (Y_1) and net investment is negative, so that S^d is

[12] In the same way linear models are associated with the exogenous (or impulse) approach, endogenous explanations of the cycle presuppose nonlinear modelling.

[13] For a thorough description of the cyclical process see Kaldor (1940:83-85). The necessary conditions for this process to generate cycles are stated in p.85.

continuously shifting to the right and I^d to the left, whilst Y is expanding. Finally, when A=C, $S^d < I^d$ and a cumulative upward process takes place with income growing rapidly back to B. Then the process repeats itself.

Figure 3



Source: Fig. 5, Kaldor (1940:83).

It is interesting to note that if S^d was linear and oscillations were produced only by a nonlinear *investment* curve, the model above would closely resemble Kalecki's explanation of the trade cycle. Indeed Kaldor carried out a comparison between the two models in an appendix to his paper. However, he was perceptive enough to point out that whilst his own system was truly endogenous, Kalecki's dynamics were somewhat artificial, i.e. they arose as a consequence of positing time-lags in investment processes. For Kaldor, in such models

> the existence of the cycle was explained as a result of the operation of certain time-lags which prevented the new equilibrium from being reached, once the old equilibrium, for some external cause, had been disturbed. In this sense all these theories may be regarded as being derived from the 'cobweb theorem' (1940:91)[14].

Although Kaldor sustained a different point of view on the appropriate way to model oscillations, he was aware of the importance of other variables

[14] In addition, Kaldor criticised the fact that in Kalecki's model it must be either assumed or demonstrated, that the impact of current investment on the stock of capital is large enough to allow for the continuous shifting of the investment decision function (D). Otherwise, the process would be heavily damped and convergence would be rapidly achieved.

determining cycles, and also of the limitations of his own model. For instance, with respect to the capacity of his model to represent real-world economic fluctuations, he recognised that although it is plausible to think of the end of a boom, it is less intuitive to explain how a recession comes to an end. In the latter case, he was not unwilling to admit the existence of external factors contributing to the generation of cycles.

IV.b. A brief note on Harrod's fundamental equation

Harrod was another economist who criticised dynamic models based on time-lags since, in his opinion, such models were essentially static. Harrod associated them with the econometricians – in a clear allusion to Frisch, whose definition of dynamics had been largely adopted in econometrics[15] – pointing out that in the case of time-lag models, oscillations were produced by the lag itself rather than by mechanisms endogenous to the system. In contrast to seeing dynamics as mere variations taking place between different points in time, Harrod developed a distinct notion in terms of rates of change (i.e. systems where variables were continuously evolving). To put it differently, in Harrod's view dynamics were not caused by the *lag* structure, but depended on the *internal* structure of the model – something that the author called the 'antinomy'[16].

To explain this 'antinomy' and the way in which endogenous cycles were generated, Harrod also devised a very simple model, based on the multiplier-accelerator mechanism and the discrepancy between (planned) investment and savings. In this fashion he derived his well-known fundamental equation:

$$G_w = s/C \qquad [\text{ or: } \quad (\Delta Y/Y) = s \,/\, (\Delta Y/\Delta K) \;] \qquad (14)$$

where:

G_w = warranted rate of growth

C = incremental capital-output relationship ($\Delta Y/\Delta K$)

s = fraction of income which is saved

[15] See Harrod (1951:262; par.6) and Besomi (1998:139; Note 1).

[16] Harrod did not reject the role of time-lags a priori, in the same way that he recognised the relevance of the distinction between consumption and capital goods. However, he believed that those factors were of secondary importance, since the fundamental forces determining fluctuations would be in operation even in their absence.

In addition to explaining how the accumulation/decumulation of stocks could affect output (in the usual way), Harrod also showed that for a static equilibrium, the process was essentially unstable. Moreover he conjectured that this instability, together with the variation in the structural values of the fundamental relations (C and s), were responsible for the generation of cycles. Clearly he also had in mind a nonlinear model.

Nevertheless Harrod's model had many flaws. First, he did not provide any satisfactory explanation for the change in 's' and 'C' (as Kaldor did)[17]. Second, the way he formalised his ideas was rather simplistic. As Besomi claims: "Harrod had indeed a nonlinear interpretation of the trade cycle. His mathematics, however, was totally inadequate for dealing with it" (1998:127).

In fact, equation (14) was the object of discussion by several economists after Harrod introduced it (see for instance Baumol 1948, and Alexander 1950). Some of them translated it into a second order linear differential equation, showing its analogy with Samuelson's model. Others transformed the fundamental relation in a first order linear differential equation, which did not exhibit cycles but exponential growth. In spite of Harrod's repeated complaints about such (mis)interpretations of his theory of the trade cycle, his model was to be considered a benchmark in growth theory in the following decades.

IV.c. Hicks's two nonlinearities: the notions of 'ceiling' and 'floor'

A fundamental contribution to the development of early nonlinear models was J. Hicks's 1950 book, *A Contribution to the Theory of the Trade Cycle*. Essentially, Hicks defended a sort of mid-way position between Harrod's (endogenous) theory and the time-lag vision sustained by most econometricians; i.e. the latter should be regarded, in his opinion, as a complement to the former[18]. In his view, a combination of both approaches represented a likely way to model cycles without relinquishing economic meaning. In particular, Hicks emphasised that the study of cycles should be about a growing economy, not a static one. In his own words:

> the econometrists have revealed to us an engine which is capable of inducing general fluctuations... nevertheless... there still seems to be

[17] Baumol (1948) noticed this problem, suggesting that in Harrod's model cyclical behaviour had to be imposed from the outside.

[18] Harrod (1951) acknowledged Hick's mid-way position.

> some missing pieces... what one wants to do is so to enlarge the econometrists' model that it takes into account, or can take into account, all the major aspects of the economic process which look like being relevant (1950:6).

Hicks's model actually combined the standard multiplier-accelerator with time-lags in the multiplier process. In short, Hicks thought that fluctuations in investment – caused by changes in autonomous investment, and the acceleration principle governing induced investment – led to an adjustment process taking place throughout many periods, due to lags in the multiplier. Overall, the whole process was believed to be stabilising. So far Hicks encountered the four standard solutions of a higher order linear system: either exponential or cyclical paths, which could be either convergent or explosive.

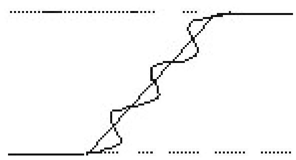
However, he did not agree with Frisch (and econometricians in general) that the most realistic way to model economic fluctuations was to allow for damped oscillations plus a source of exogenous shocks. On the contrary, he was convinced that this situation was rather rare in the real economy and would only hold under very particular circumstances. He thus concluded that the meaningful solution for his model was not the one yielding damped oscillations, but *the one producing explosive cycles* (because it was a more appropriate representation of the inherent instability described by Harrod).

A straightforward question is, then, how could Hicks account for the fact that real economies do not actually explode in either direction? And the answer is that, in a very clever movement, he ruled out the possibility that his model could explode by introducing two limits, namely a *ceiling* and a *floor*, which forced the system to stay within a certain range of values. The ceiling was given by full employment of resources – which in a growing economy displays an upward trend – whilst the floor reflected the fact that net investment could not be lower than a (negative) amount given by depreciation.

Economic cycles were thus explained by Hicks in the following way. Once the system is disturbed, e.g. as a result of an increase in autonomous investment, output expands in an oscillatory fashion (under the explosive cyclical assumption), until full employment is achieved and therefore the ceiling is hit. The downturn is then inevitable because the difference between the actual level of output and its equilibrium level is narrowing, and thus induced investment is not enough to support such a rate of growth.

Consequently, GDP falls producing ‘disinvestment’ which takes place through a process of wearing-out until gross investment is zero meaning that the floor has been reached[19]. After the system hits the floor, autonomous investment rises, the acceleration principle starts working again and the cycle is restarted (for a thorough discussion see Hicks 1950; Chapter VIII).

Figure 4



Source: based on Hicks (1950) and Goodwin (1950).

Hence by adding a lag structure to Harrod’s instability principle, Hicks developed his own endogenous model of the limit cycle. The crucial aspect of his model, and the reason why his book represented a fundamental step in the evolution of macrodynamic nonlinear models, was the inclusion by Hicks of two explicit nonlinearities in the investment function. In effect, the addition of the notions of ‘ceiling’ and ‘floor’ determined that output behaved as a piecewise linear function. To put it differently, output would follow an oscillatory path given by a second order linear system, but only between those two (non-fixed) limits. This is nothing but an elementary sort of nonlinear model (Figure 4).

However, Hicks was not aware that his dynamic system was actually nonlinear[20]. To sum up, since the 1930s economists had been trying to model persistent oscillations. Some of them did it using linear models plus exogenous shocks in a rather mechanical way. Others had an intuition that any more realistic explanation of the cycle should account for the endogenous mechanisms producing fluctuations in the economy. This is the case of Kalecki, Kaldor and Harrod’s theories discussed in the previous sections.

[19] One of the main criticisms to Hicks’s adjustment mechanism was that, if it was necessary to wait for all capital to depreciate, the system would spend an unusually long time in recession. In addition, Harrod (1951) pointed out the difficulty in distinguishing autonomous from induced investment.

[20] It was Richard Goodwin who later noticed this feature (see below).

Many aspects remained obscure though. Finally, in 1950, Hicks advanced a detailed explanation of the nature of fluctuations, but one last step was missing for the success to be complete: to find a mathematical expression of the limit cycle.

IV.d. Goodwin's mathematical formalisation

Richard Goodwin was the first economist to develop an explicit (mathematical) nonlinear model of the trade cycle. As Kaldor, he became aware throughout his life that the only way to model sustained fluctuations was to use nonlinear functions. In the beginning, and before coming up with his mathematical formalisation of the trade cycle, Goodwin published a few articles using linear models, such as his essay about the flexible accelerator in 1948. However, following the review of Harrod's paper by Tinbergen (1937), where the latter (misleadingly) interpreted Harrod's model as a first order linear differential equation, Goodwin eventually realised the extent to what Harrod's intuition about dynamic instability was a more accurate vision of the dynamics of capitalism.

Goodwin's main objective was to find a way to account for the historical facts characterising *actual* business cycles, in a rigorous and analytic fashion, and taking into account three essential aspects: i) Schumpeterian innovations as the base of growth theory, b) a flexible version of the accelerator, and c) a mathematical representation for a stable equilibrium motion (Goodwin 1982; see especially the Preface).

In order to do this, Goodwin realised that linear dynamic systems did not produce the kind of dynamics he was looking for, and then he devoted himself to study the theory of oscillators. Being far from an expert mathematician himself, he was influenced by his communication with mathematician P. Le Corbeiller who, in the first issue of *Econometrica*, published an article discussing the virtues of employing (relaxation) oscillators in economic models. Equations were of a very simple type:

$$\partial^2 x/\partial t^2 + F(dx/dt) + x = 0 \qquad (15)$$ [21]

Goodwin followed several paths in his research on nonlinear dynamics. In 1950, he published a review of Hicks's book on the trade cycle, stating

[21] Two particular cases of equation (15) which are very well known are: 1) the Lord Rayleigh equation: $F(dx/dt) = \rho[(x^3/3) - (dx/dt)]$, and 2) the Van der Pol equation: $F(dx/dt) = \varepsilon\,(x^2-b)\,(dx/dt)$.

explicitly that Hicks had included two nonlinearities in his model, and supplying a graphical interpretation of the way the model functioned. Goodwin also explored alternative ways to model cyclical behaviour – besides the standard assumptions of time-lags and/or a discrepancy between ex-ante savings and investment. For instance, he considered the possibility that oscillations were caused by changes in income distribution and, in one of his most celebrated articles, "*A growth cycle*" (1967), he framed his theory as a nonlinear predator-prey system leading to a limit cycle.

Another important contribution – and perhaps more in line with the works by other economists discussed above – was Goodwin's paper on the nonlinear accelerator (1951), where he could summarise fairly well the original idea Hicks had in mind. The model is basically described by the following equations, which look like a continuous version of Samuelson's (discrete) model:

$$Y = C + dK/dt \qquad (16)$$

$$C = \beta(t) + \alpha Y - \varepsilon(dY/dt) \qquad (17)$$

$$dK/dt = \delta(t) + \varphi(dY/dt) \qquad (18)$$

where $\beta(t)$ and $\delta(t)$ stand for autonomous consumption and autonomous investment, respectively. The last term in (17) arises from the fact that the model is continuous rather than discrete, and the last term in (18) is a function describing the behaviour of induced investment, in the following manner:

a) $dK/dt = \mu(dY/dt)$ holds for middle ranges of income. In this interval: $\varphi = \mu(dY/dt)$, and $\varphi'(dY/dt) = \mu$ (i.e. this is the acceleration principle)

b) $\varphi'(dY/dt) = 0$ holds for (very) low and high income levels, i.e. the accelerator does not operate. Close to the upper limit (full employment), the desired capital stock (ξ) exceeds the actual capital stock (K), that is $\xi>K$, and thus the rate of investment approaches its capacity level $dK/dt = K'_{*}$. Close to the lower limit, $\xi<K$, and capital is depleted at the rate $dK/dt = K'_{**}$.

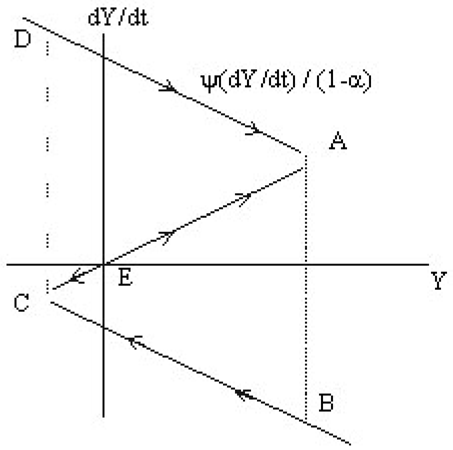
Assuming that the autonomous components equal zero, i.e. $\beta(t) = \delta(t) = 0$, and making appropriate substitutions in (16) to (18), it is possible to derive the equation:

$$Y = [\varphi(dY/dt) - \varepsilon(dY/dt)] / (1-\alpha) = \psi(dY/dt) / (1-\alpha) \qquad (19)$$

When φ is within the range of operation of the standard accelerator, (19) assumes the following particular form:

$$Y = [(\mu-\varepsilon) / (1-\alpha)] (dY/dt) \quad ; \text{with } \mu>\varepsilon \qquad (20)$$

Figure 5



Source: Figure 6.5, Goodwin (1982:89).

The dynamic system Goodwin had in mind is shown in Figure 5, and his explanation is as follows[22]. The equilibrium point E (where dY/dt=0) is unstable. Any small positive deviation from E will make output to grow – via the accelerator mechanism – in an explosive fashion until point A (representing full capacity and hence, employment) is reached. When the upper limit is hit, capital has been accumulating very fast and $\xi<K$; therefore investment falls at the rate K'_{**}, producing a discontinuous jump in the rate of growth of output (dY/dt) and leading the system to point B. Now income is decreasing towards the lower limit C, where $\xi>K$ and investment starts to grow at the rate K'_{*}, producing another jump this time leading the system to point D. From then on, the system behaves as a closed limit cycle in ABCD.

In a second step, Goodwin re-introduced $\beta(t)$ and $\delta(t)$ – the latter denoting Schumpeterian innovation – and then the model did not behave as a closed system any more, but instead, as a cycle continuously shifting to

[22] For a more detailed explanation of the process, see Goodwin (1982:87-90).

the right. This idea of a 'growth cycle' was a very original one (and represented a genuine progress in relation to Hicks's model) because in this way a) the problem of long-lasting recessions was avoided, and b) his model captured a real feature of the economy, namely the fact that in general growth and cycles come together and are indistinguishable from each other.

Nevertheless, Goodwin could not escape (in a sense) the trap of exogeneity. In effect, his growth cycle was triggered by shocks in autonomous investment (i.e. innovations à-la- Schumpeter) whose origin was not explained endogenously[23]. Whilst the early linear models relied on exogenous forces to keep cycles alive, Goodwin's nonlinear model required a kind of shock from the outside, to explain growth cycles and to differentiate each fluctuation from the other. In this sense, early nonlinear macrodynamic models were limited, something that modern supporters of nonlinear dynamics in economics – in possession of more sophisticated tools and theories – have largely recognised[24].

It is only fair, however, to emphasise how great an achievement it was for economists in the early decades of the twentieth century to be able to think of the notion of a limit cycle and eventually formalise it. The lack of mathematical skills and computing power prevented them from achieving more spectacular successes and, for example, discovering notions such as aperiodic and chaotic behaviour. Goodwin devoted his whole academic life to the study of nonlinear systems and later, in the 1980s, he was one of the first to publish papers on chaos theory and complex systems. He has always been aware of the limitation of nonlinear models though, which is another reason to appreciate his work. For him:

> it is not that we can expect any simple, mathematical model to explain the wave-like character of economic history, but rather merely to explain the remarkable fact that there is some degree of uniformity in the otherwise unique course of development (Goodwin 1982:127).

[23] Schumpeter himself has been criticised for failing to provide an endogenous explanation of innovation (see e.g. Witt 1995).

[24] For instance, Day (1994) classifies limit cycles together with the stationary state as 'simple dynamics', by contrast to 'complex dynamics' which includes dynamic behaviours that are non-periodic, non-balanced and non-convergent.

V. Concluding remarks: how early nonlinear models were forgotten by economics

The main objective of this essay was to offer a systematic overview of the early history of nonlinear modelling in economics. For about twenty years, dynamic models of the business cycle evolved from Frisch's original linear formulation in 1933 to Goodwin's formalisation of the limit cycle in 1951[25]. As shown in sections 2 to 4, nearly all the early nonlinear models included some version of the multiplier-accelerator mechanism, focusing (one way or another) on investment as the main variable explaining fluctuations[26].

In spite of Goodwin's success in providing a nonlinear theory of cycles, after 1950 progress in economics followed a different path. Economists – with the exception of Goodwin and a few disciples who continued doing research on nonlinear modelling – were not interested in explaining the origin of business cycles any more, and all previous efforts in this direction were virtually abandoned. It is beyond the scope of this paper to provide a thorough explanation of the reason why nonlinear modelling came to a halt, but I do think an evolutionary explanation is in order.

By an evolutionary explanation I mean, essentially, that a group or population is selected as a consequence of changes in the environment[27]. In this case, the 'populations' are given by the different practices in economics, namely nonlinear modelling vs. other types of modelling. The 'environment' in turn, includes all the aspects that could have an impact, at a moment in time, on the way economists deal with problems. For our purposes these aspects include changes in: a) the world economy, b) economic methodology, and c) technology. It is my contention that drastic changes in all the three

[25] An interesting and concise summary of early contributions on the business cycle can be found at the New School's website: http://cepa.newschool.edu/het/schools/business.htm

[26] These models did not show a great concern with financial variables and expectations. In fact, around the 1930s there were several studies explaining cycles based on monetary factors. However, it was multiplier-accelerator models that were relevant for the early progresses in nonlinear dynamic modelling.

[27] For a thorough explanation of the evolutionary hypothesis see Lawson (2003, Ch.5). In Chapter 10, the author also uses an evolutionary model to account for a different episode in the history of economic thought, namely the mathematisation of economics over the twentieth century.

aspects constituting the environment in which economists worked during the 1950s, determined the decline in nonlinear modelling and its eventual abandonment.

First of all, the sustained expansion of the world economy following the end of the Second World War together with the vanishing ghost of depression, originated a great interest in the study of growth processes, to the detriment of cycles and recessions. In his *Economics in Perspective*, J.K. Galbraith (1987) quotes from the *Economic Report of the President 1969*:

> The nation is now in its 95th month of continuous economic advance. Both in strength and length, this prosperity is without parallel in our history. We have steered clear of the business-cycle recessions which for generations derailed us repeatedly from the path of growth and progress.... No longer do we view our economic life as a relentless tide of ups and downs... a solid foundation has been built for continued growth in the years ahead.

In the same way the Great Depression had encouraged research on economic fluctuations twenty years before, the impressive rates of steady economic growth experienced by the central economies from the 1950s onwards, took the interest of economists away from cycles.

As to the second aspect, i.e. the advances in economic methodology, it is also during these years that econometrics became firmly established in the discipline (Morgan 1991, Christ 1994). Consequently, the construction of large *linear* structural models – whose dynamics were induced by means of time-lags and (exogenous) random socks – was widely accepted. Given the technology available at the moment, nonlinear models would have been very difficult to estimate. Moreover it was observed that many of those basic linear models produced time-series that performed quite well compared to the actual ones[28]. Finally, on the theoretical side, the growing emphasis on equilibrium and optimising behaviour, contributed to persuade researchers to remain attached to the linear approach on the grounds of simplicity.

There is yet another reason why nonlinear modelling came to an end. The efforts made by early macroeconomists to develop comprehensive models of the trade cycle and to give their ideas a mathematical expression, were truly remarkable. However, they were not totally successful from a formal

[28] E.g. it was shown that a second order autoregressive process provided a reasonably accurate description of US output trends.

point of view because the mathematical training of economists at that turning point in the history of economics, was (of course) not as substantial as it is now. In fact, the case of Samuelson – who found an analytic expression for the four zones delimiting the behaviour of his multiplier-accelerator model –, Frisch – who based his theory of cycles in an analogy with the physical pendulum –, or Goodwin – who worked with nonlinear oscillators –, were exceptions. Most economists were not mathematicians but social theorists, who just endeavoured to systematise the intuitions they had about what was going on 'out there', in the real world.

The growing interest in formal modelling that took place during the mid-twentieth century, meant that many of the early macrodynamic models were thus discarded due to their allegedly intuitive rather than formal presentation. In this way the analytical richness implicit in the great theories about the cycle – such as Harrod's instability principle – was subsequently lost. Had the concern with mathematisation of economics not become so strong, it is likely that a more open and eclectic approach would have ensued. On the other hand, if those pioneering economists had had enough mathematical skills, they would have probably found out ways to develop more interesting models, and many of the notions about complex dynamic systems that are today commonplace – and for which the basis already existed in those years – would have been developed without having to wait until the 1980s. In other words, the radical change from incipient nonlinear, to sophisticated linear stochastic modelling, merely reflected the entirely different interests displayed, and skills possessed, by different generations of economists.

During the last two decades or so, the growing dissatisfaction of economists with (the now traditional) linear models encouraged them to explore new modelling possibilities. In this way, the interest in complex (nonlinear) dynamics has re-emerged, this time stimulated by some promising developments and applications of nonlinear models within different branches of the natural sciences since the late 1960s. However, young economists excited by the new fancy models and computational tools, should keep in mind that nonlinear dynamics has a longer history in economics. This essay is a modest tribute to those who could see beyond their times. Their motivation was also more admirable: they were deeply concerned to understand the world in which they lived.

VI. REFERENCES

Alexander S (1950) “Mr. Harrod’s dynamic model”. *Economic Journal* **60**(240), pp.724-39

Anderson PW (1988) “A physicist looks at economics: an overview of the workshop”. In: PW Anderson, KJ Arrow and D Pines (eds.), *The Economy as an Evolving Complex System*, Addison-Wesley, Redwood City

Arthur B, Durlauf S and Lane D (1997) *The Economy as an Evolving Complex System II*. Perseus Books, Reading

Baumol W (1948) “Notes on some dynamic models”. *Economic Journal* **58**(232), pp.506-21

Baumol W and Benhabib J (1989) “Chaos: significance, mechanism and economic applications”. *Journal of Economic Perspectives* **3**(1), pp.77-105

Besomi D (1998) “Failing to win consent: Harrod’s dynamics in the eyes of his readers”. In: G Rampa, L Stella and AP Thirlwall (eds.), *Economic Dynamics, Trade and Growth: Essays on Harrodian Themes*, Macmillan, Basingstoke

Bjerkholt O (1995) *Foundations of Modern Econometrics: The Selected Essays of Ragnar Frisch*. Edward Elgar, Cheltenham and Northampton

Christ CF (1994) “The Cowles Commission’s Contributions to Econometrics at Chicago, 1939-1955”. *Journal of Economic Literature* **32**(1), pp. 30-59

Colander D (2000a) *Complexity and the History of Economic Thought*. Routledge, London

Colander D (2000b) *The Complexity Vision and the Teaching of Economics*. Edward Elgar, Cheltenham and Northampton

Day RH (1993) “Nonlinear dynamics and evolutionary economics”. In: RH Day and P Chen (eds.), op.cit.

Day RH (1994) *Complex Economic Dynamics*. The MIT Press, Cambridge (MA) and London

Day RH and Chen P (1993) *Nonlinear Dynamics and Evolutionary Economics*. Oxford University Press, New York and Oxford

Flaschel P, Franke R and Semmler W (1997) *Dynamic Macroeconomics: Instability, Fluctuation, and Growth in Monetary Economies*. The MIT Press, Cambridge (MA) and London

Foster J (1997) “The analytical foundation of evolutionary economics: from biological analogy to economic self-organization”. *Structural Change and Economic Dynamics* **8**(4), pp.427-51

Frisch R (1929) “Statics and dynamics in economic theory”. Reproduced in: O Bjerkholt (1995), op.cit.

Frisch R (1933) “Propagation problems and impulse problems in dynamic economics”. Reproduced in: O Bjerkholt (1995), op.cit.

Frisch R and Holme H (1935) “The characteristic solutions of a mixed difference and differential equation occurring in economic dynamics”. *Econometrica* **3**(2), pp.225-39

Galbraith JK (1987) *Economics in Perspective. A Critical History*. Houghton Mifflin, Boston

Goodwin R (1948) “An oscillatory mechanism with a flexible accelerator”. Reproduced in: R Goodwin (1982), op.cit.

Goodwin R (1950) “A non-linear theory of the cycle”. *Review of Economics and Statistics* **32**(4), pp.316-20.

Goodwin R (1951) “The nonlinear accelerator and the persistence of business cycles”. *Econometrica* **19**(1), pp.1-17.

Goodwin R (1967) “A growth cycle”. Reproduced in: R Goodwin (1982), op.cit.

Goodwin R (1982) *Essays in Economic Dynamics*. Macmillan, London and Basingstoke

Goodwin R (1994) “A reformulation and extension of Hicksian dynamics”. In: H Hagemann and OF Hamouda (eds.), *The Legacy of Hicks: His Contribution to Economic Analysis*. Routledge, London

Grandmont JM (1987) *Nonlinear Economic Dynamics*. Academic Press, Boston and London

Harrod R (1939) “An essay in dynamic theory”. *Economic Journal* **49**(193), pp.14-33

Harrod R (1951) “Notes on trade cycle theory”. *Economic Journal* **61**(242), pp.261-75

Hendry D and Morgan M (1995) *The Foundations of Econometric Analysis*. Cambridge University Press, Cambridge

Hicks J (1950) *A Contribution to the theory of the Trade Cycle*. Clarendon Press, Oxford

Kaldor N (1940) “A model of the trade cycle”. *Economic Journal* **50**(197), pp.78-92

Kalecki M (1935) “A macrodynamic theory of business cycles”. *Econometrica* **3**(3), pp.327-44

Kalecki M (1937) “A theory of the business cycle”. *Review of Economic Studies* **4**(2), pp.77-97

Kiel LD and Elliott E (1998) *Chaos Theory in the Social Sciences. Foundations and Applications*. The University of Michigan Press, Ann Arbor

Lawson T (2003) *Reorienting Economics*. Routledge, London and New York

Le Corbeiller P (1933) “Les systemes autoentretenus et les oscillations de relaxation”. *Econometrica* **1**(3), pp.328-32

Morgan M (1991) *The History of Econometric Ideas*. Cambridge University Press, Cambridge

Medio A (1992) *Chaotic Dynamics: Theory and Applications to Economics*. Cambridge University Press, Cambridge

Mirowski P (1998) "Machine dreams: economic agents as cyborgs". In: JB Davis (ed.), *New Economics and Its History*. Duke University Press, Durham and London

Mirowski P (2002) *Machine Dreams. Economics Becomes a Cyborg Science*. Cambridge University Press, Cambridge

Nagatani K (1981) *Macroeconomic Dynamics*. Cambridge University Press, Cambridge

Perona E (2005) "Ciencias de la complejidad: ¿La economía del siglo 21?". *Documento de Trabajo* Nº 22, Departamento de Economía, U.N. Córdoba

Puu T (1993) *Nonlinear Economic Dynamics*. Springer-Verlag, Heidelberg and New York

Rosser JB (2004) *Complexity in economics*. Edward Elgar, Cheltenham and Northampton

Samuelson P (1939) "Interactions between the multiplier analysis and the principle of acceleration". *Review of Economics and Statistics* **21**(2), pp.75-78

Scheinkman J (1990) "Nonlinearities in economic dynamics". *Economic Journal* **100**(400), Conference Papers, pp.33-48

Slutzky E (1927) "The summation of random causes as the source of cyclic processes". *Econometrica* **5**(2), pp.105-46

Thalberg B (1990) "A reconsideration of Frisch's original cycle model". In: K Velupillai (1990a), op.cit.

Tinbergen J (1937) "(Review of) Harrod, R.F., *The Trade Cycle. An Essay*". *Weltwirtschaftliches Archiv* **45**(3), pp.89-91

Tvede L (1998) *Business Cycles. The Business Cycle Problem from John Law to Chaos Theory*. Harwood Academic Publishers, Amsterdam

Velupillai K (1990a) *Nonlinear and Multisectoral Macrodynamics: Essays in Honour of Richard Goodwin*. Macmillan, Basingstoke

Velupillai K (1990b) "The (nonlinear) life and (economic) times of Richard M. Goodwin". In: K Velupillai (1990a), op.cit.

Witt U (1995) "Schumpeter vs. Hayek: two approaches to evolutionary economics". In: G Meijer (ed.), *New Perspectives on Austrian Economics*. Routledge, London

Zarnowitz V (1985) "Recent work on business cycles in historical perspective: a review of theories and evidence". *Journal of Economic Literature* **XXIII**(2), pp.523-80