GROWTH THEORY AS AN UNWANTED CHILD OF MACRO-DYNAMICS[1]

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ABSTRACT

The story this paper tells, attempts to explain 1) how Neo-Classical growth theory happened to be simultaneously born by the hands of Solow (1956) and Swan (1956); and 2) how it carried its signature characteristics, *i.e.* the exogeneity of the long run growth driver. Both issues are ascribed to the heritage of unsolved problems from Macroeconomics, in its most mature version a non-linear theory of fluctuations.

Key words: Macro-dynamics, economic cycles, technological change, economic growth.

JEL Classification: E27, E39, O39, O40.

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LA TEORÍA DEL CRECIMIENTO COMO UN VÁSTAGO
NO DESEADO DE LA MACRODINÁMICA

RESUMEN

Este artículo narra e intenta explicar: 1) cómo la teoría neoclásica del crecimiento surgió de forma simultánea de las manos de Solow (1956) y de Swan (1956) y 2) cómo llevó consigo sus características distintivas, *i.e.*, la exogeneidad de su fuerza motriz de largo plazo. Ambas cuestiones se atribuyen a la herencia de problemas no resueltos de la macroeconomía en su versión más madura, una teoría no lineal de las fluctuaciones cíclicas.

Palabras clave: macrodinámica, ciclos económicos, cambio tecnológico, crecimiento económico.

Clasificación JEL: E27, E39, O39, O40.

1. INTRODUCTION

This paper, a shorter version of a previously published one[2] with which it largely overlaps at least in the theme, tells the story of how it came about that growth theory of the Neoclassical brand (NC, associated with Robert Solow and Trevor Swan) was born out of some logical difficulties of the *classical Macro-dynamic theory* of fluctuations (associated with the names of Michal Kalecki, Nicholas Kaldor, John R. Hicks and, finally, with Richard M. Goodwin).

A reconstruction of the evolution of the mathematized theories (models) of fluctuations is offered in the way it led to the macroeconomic theory of growth[3]. In particular, a reason will be suggested for why the driver of growth, technological progress (hencefort, TP), was left *exogenous*; in other words, how it was eventually accepted that the explanation of growth be left outside the theory that was built to account for it. This generated a conundrum that explains much of the subsequent

[2] A nonlinear history of growth and cycle theories. *History of Political Economy* 2009, 41 (annual supplement), pp. 88-106.

[3] For more details, I will have to refer to Punzo (2006), in particular for the intellectual route of Richard M. Goodwin against the backdrop of the development of Macro-dynamics.

evolution of the growth approach and the continuing survival of the exogenous approach.

My main thesis is that Neoclassical modelling of the growth phenomenon was born from the failed attempt of classical Macro-dynamics to construct a continuous-time theory jointly explaining, from a unique set of principles, fluctuations and growth. Such a failure left *unexplained* the driver of the latter. It was to accomplish such an ambitious project, that the marriage of Macro-dynamics with Keynes' aggregative style of analysis had been thought, at the time, useful. The marriage ended up in an almost disaster, questioning the very empirical foundations of the approach, justifying the exit towards Neoclassical axiomatic.

As Solow was later (1994) to say [and, perhaps, Giambattista Vico (1668-1744) said, too, well before him], the "same perennial issues seem to come back". So far, the emergence and often re-emergence of a variety of theories of economic fluctuations has been witnessed, and three peaks of interest in growth, suggesting a peculiar association that demands explanation. To find one, we looked at a common path leading back to the origins of formal dynamics. In our "nonlinear view of history", some interpretive models often re-appear until certain theoretical issues remain unsolved, though each time they might receive a distinct answer. By mid-1960s growth theorizing seemed to have reached the end of its fruitful years (Hahn and Matthews, 1964). As expectations turned out to be ill founded, we might again be in the middle of another swing. I address one of the unsolved issues that generate these sorts of swings.

2. WAVES OF GROWTH THEORY

Hahn and Matthews's classical review of growth theories begins with Harrod's 1939 book and Domar's 1946 article. Although important differences between them are often recalled, probably a universal consensus treats them as basically a single model, which would mark the *birth* of the *formal* theory of growth. Rarely is it recalled that Harrod's 1939 *Essay* was preceded by his 1936 *The Trade Cycle*, a book generally considered an unsuccessful and short-lived dash into the theory of fluctuations (see *e.g.* Hansen, 1937; Solow, 1988). The book seems to have only left *Harrodian knife-edge* to ponder on. The Harrod-Domar model would, on the other hand, be the ancestor of NC growth theory,

as with the latter it focuses on proving the existence of a full employment equilibrium path.

Naturally, to be practically relevant as a *predictor*, an equilibrium has to be (at least, locally) unique and the economy must be shown to be stable towards it. That's precisely what Solow successfully set out to do. His neoclassical growth theory ("the *neoclassical response* to the *Harrod-Domar impulse*", Solow (1994) would be the successful end of a long term quest: Conventional history inserts the episode of the birth of NC theory into the process that produced the modern General Equilibrium program[4].

The new perspective I offer hereafter, indicates *that episode* as a temporary station of a longer intellectual process, and points to a new *genealogical* thread. To follow such thread, entering an unexpected theoretical scenario, we need to go back to Harrod of the *Trade Cycle* 1936 (TC). The latter represents the conceptual distillate of a broad reflection on how to theorize economic dynamics[5]; it is at the crossroad of different strands of thought (the Keynesian being just one of them), largely in the making. It was for the latter reason that Harrod could swiftly switch from his general dynamics of 1936, to the theory of growth of the 1939 *Essay*, and later (1948) to his new book.

TC claimed to offer a "new theory" with a "precise and definite diagnosis" of dynamics of a system out of its equilibrium path. There, we find probably the earliest conscious attempt to analyse both growth and business cycle within a Keynesian framework, along with some mostly intuitions as to how they were to interact. Harrod's own aim there was to deal with a something much more complex than the mere (knife-edge) stability issue that was going to be read in it, later. The difficulty in the search for an altogether new approach and a definitely poor command of the required mathematics did not help to attain such ambitious goals. The propositions in TC are exactly dual to those of Neoclassical growth theory[6]. With the hindsight of the modern theory of dynamic complex-

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[4] In particular, linking it with the then ongoing research on stability with Samuelson's dynamical approach on the front-line (documented in *e.g.* Weintraub, 1991).

[5] See Young (1989) and Besomi (1999) for a detailed account of the evolution of Harrod's ideas leading to *The Trade Cycle*. See in particular chapters 4 and 5 in Young (1987).

[6] See Punzo (1988). A similar thesis is expressed by Besomi (2001).

ity (that we have been taught by the chaos literature of the 90s), in the TC the issue is not the instability of an equilibrium path[7]. The capitalist system is simply not capable of being stable[8].

In Harrod's international intellectual milieu, the Classics were thought to have already provided an acceptable explanation of the long run (this was what really equilibrium analysis was about). The explanation of the "shorter than long run" of economies was still amiss. This did not look at all like equilibrium. All in such varied milieu set out looking for explanations of the more complex dynamics exhibited by "reality". This might lead either to discontinuities and structural changes[9], or else it had to have some self-limiting behaviour. Pessimistically, they surmised that the economy would run into the former, if the latter failed to set in. Ragnar Frisch's (1933) Manifesto for an empirically founded Macro-dynamics[10] was well aware of the *drama* of the alternative; even Solow saw it in his celebrated growth paper.

Macro-dynamics placed its hopes in finding self-limiting kinds of unstable dynamics. Doing so, it swept under the theoretical carpet the issue of structural change.

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[7] The latter amounts to saying more than simply that, an economy "once it strays from equilibrium growth" never returns. (*e.g.* Solow, 1988, p. 310). Solow acknowledges that the issue of Harrod's (in)stability has two aspects, and that this latter aspect was not dealt with by neoclassical growth theory.

[8] This intuitive notion belonged to the perception of reality in a broad intellectual milieu, with Keynes, but spread between Oxford, Cambridge, United Kingdom, probably the London School of Economics, and dominant in most Middle-European circles. It was waiting to have a precise expression, which is what Harrod was trying to do. TC is a good illustration of the fact that "successful (like, often, also unsuccessful) ideas are a group product" (Solow, 1988; Young, 1987).

[9] Leontief (1934), quoted in Goodwin (1947).

[10] Macro-dynamics can be defined (after Frisch, 1933) as the theory of the manifold dynamics exhibited by *real* economies when they are represented at certain levels of (dis-) *aggregation*. Programmatically, its research field extends over growth as well as fluctuations at various frequencies, including business cycles as special cases. Typical is its view (imported from Physics) that fluctuations are intimately related with growth identified as an exponential motion, as the latter is but a specimen of the former. If growth is a fluctuation with a singular frequency, they ought to get a unified mathematical treatment. Moreover, classical Macro-dynamics has to be credited for trying to deal with non- or out-of-equilibrium dynamics.

3. HARROD'S TC AND MACRO-DYNAMICS

Macrodynamic formalism had two simple ingredients: A given empirical structure and the dual notion that dynamics was generated by *exogenous impulse*(s) activating a *propagation mechanism*, *i.e.* by the system own structure. Such scheme placed greater weight onto the *exogenous* impulse, the real *engine* of dynamics, and relatively less onto *propagation*, basically seen as a damping mechanism, to be repeatedly re-initialised in order to generate oscillations at finite frequencies. *Any motion, including exponential growth, has both its cause and the cause of its persistence in some specifically assumed, often ad hoc, impulse profile.* For our history, the project of classical Macro-dynamics could be usefully read against the backdrop of Keynes's attack against the Classical Stability Postulate, a postulate said to imply the view that an economy is a naturally stable system that, unless disturbed from outside, always lingers around a (possibly, locally unique) equilibrium state. The Classical Stability Postulate was implicit in Frisch's view of a damping propagation mechanism: His analysis of dynamics had oscillations on the forefront, but a stable, though possibly moving, equilibrium lied at its core.

However, such scheme could be easily reverted by exchanging relative weights: The propagation mechanism being the dynamic engine and the impulse a mere disturbance. This, I believe, is the key to really understand Harrod's 1936 book, its distance from classical Macro-dynamics (and perhaps Keynes). Still, the notion of a given system (macro-) structure made a marriage with Keynesian analysis of real variables relatively easy. It only needed the simplest structure, with the lowest dimension of one state variable.

Therefore, the simultaneous presence of growth and fluctuations places Harrod's (1936) contribution within the research program of Macro-dynamics. However, his rejection of global stability is the basic reason behind the view of the trade cycle as an explosive but time over recurrent path systematically away from equilibrium, the *warranted growth path*. If intellectual history can at times be told as a spy story, Roy Harrod was the man who really tried to resolve the ambiguity in the classical Macro-dynamics program and to attack that stability postulate head on. The result of such attack was the earliest programmatic attempt at founding an *endogenous* theory of economic dynamics, but

also its temporary failure. Armed with the intuition that an economy should be conceived as an *autonomous system* (Punzo, 2006) and that, therefore, the *propagation* mechanism embedded in its own structure would be capable, alone, of *sustained* fluctuations, Harrod launched a wave of business cycle analysis.

Voicing a latent demand, with perhaps a *bad* book and the poor mathematics he commanded, he put an intellectual generation to work.

4. A PROJECT

Thus, Harrod did try to formulate his view of system instability but could not handle the complications. Perhaps realising his personal inadequacy to the new standards of mathematization (but also due to other events), he gave up his attempt. According to my story, it was disappointment that produced the 1939 *Essay* and later work[11].

How to solve his problem, *i.e.* how to get growth and fluctuations together, became the core issue in the thinking of a whole generation: The mathematical problem inadequately dealt with in the TC seems to have been the fundamental motivation leading certain people to move on to a nonlinear formulation of business cycles (BCs) and in continuous time (respecting Harrod's strong argument against the *ad hoc* use of lags).

Ironically, while Harrod was looking to salve himself with that theory of growth which made him famous, he had contributed to shape the research agenda of the nonlinear endogenous approach to business cycle and other oscillations[12].

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[11] Tinbergen's (1937) review of the *Trade Cycle* pointed out that its mathematical formulation was insufficient to yield fluctuations, and in fact it could only produce exponential growth. According to *e.g.* Goodwin (1982, pp. vii-viii), and Goodwin (1985), this led Harrod to abandon business cycle and to elaborate the growth interpretation that appears in the *Essay,* published shortly afterwards. This anecdotal episode shows the existence of an actual link between Harrod and the Middle-European milieu associated with the Macro-dynamics program. Young (1989) produces convincing evidence of the atmosphere in which *Trade Cycle* was conceived, in particular of Harrod's great expectations that his book would be the centre of debate at the forthcoming meeting of the Econometric Society in Oxford, just four months after its publication. Probably, the disillusion with that meeting contributed to Harrod's change of mind as much as the realization of the mathematical difficulties he could not cope with.

[12] In fact, well after the publication (1950) of Hicks' *A contribution to the theory of the trade*

Classical business cycle analysis generated a family of models trying to address and to give analytical answers to questions such as: Can self-sustained fluctuations be the *generic* motions whose explanation is buried deep in the structure of the economy? Non-linear mathematics was the adequate tool to represent such persistent "disequilibrium".

The twenty-year time interval between TC and the birth of neoclassical growth theory saw the development of such a research programme. Practically the totality of such models were aggregate, the off-springs of that marriage of convenience between classical Macro-dynamics and Keynesian analysis. What at the time appeared to be reciprocal convenience was the fact that the one-dimensional Keynesian model was a mathematically simpler version of the n-dimensional *Tableau Economique* envisaged in Frisch's article. A whole group of researchers conspired in arranging for such a marriage. Unfortunately, the two frameworks eventually came to be identified[13].

Classical business cycle analysis was basically about a *thought experiment*: Trying to construct a wholly endogenous explanation of dynamics *as if* an economy could be observed *in vitro*, insulated from all sorts of exogenous, even stochastic impulses. There was no clear predictive content in its models. This lack was felt to be one of its weakest points in an intellectual atmosphere where growing familiarity with modelling raised increasing expectations and demand for testable hypotheses.

Its models, on the other hand, were all deterministic, built around a common set of properties that could only be represented in nonlinear formulations more adequate than Harrod's own. Equilibrium growth (*e.g.* steady state) paths were considered to be *empirically irrelevant*, rarely or never actually observed, and had to be understood as some of the modes in the complicated modal interlocking typical of large systems. They could be intellectual constructs, or be caused by long run forces *exogenous* to the natural mechanics of the economy. The task of explaining them, therefore, would be well beyond the realm of theory, as not having any

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cycle, for instance, it was Harrod's *Trade Cycle* that was still cited as the key reference for dynamics.

[13] Goodwin's lifelong work in linear economics, in parallel with his nonlinear cycle modelling, shows the uneasiness generated by such marriage and an unresolved contradiction (Punzo, 2006).

intrinsic interest (Goodwin, 1951b). At times, reference was eclectically made to Harrod's full employment path, sometimes to Schumpeter's view of innovation swarms (as *e.g.* in Kaldor, 1954), or else to autonomous investment expenditure, perhaps linked to technological progress (Hicks, 1950). Observed fluctuations, instead, were interpreted as the visible manifestation of an inherent tendency to dis-equilibrium, their causes being ingrained into the internal wiring of an economic system. In other words, this approach to business cycle coordinated an endogenous explanation of fluctuations with an exogenous explanation of growth.

However, the relationship between growth as a trend and fluctuation was left unclear[14]. Nonlinearity *per se* was not adequate to the task, for a single state-variable model simply cannot yield at the same time fluctuations and exponential growth. Thus, it cannot solve the problem that haunted the key argument in the TC. Sticking to nonlinearity *only* for mathematical convenience, eventually led to focus on *fluctuations* and to give up all attempts to explain (*empirically irrelevant,* as they thought) equilibrium growth paths. The approach of a whole generation failed to reach the objective it had set to itself, a unified theory of dynamics. A generation's disillusion, though not the only one! Such was the costs of the *initially promising* marriage of convenience.

This left a *hole* in the theory of economic dynamics, and somebody was to look into it.

The failure in addressing satisfactorily Harrod's issue is, therefore, the first key to understand subsequent developments, among them the birth of growth theory as a dedicated, separate dynamic theory. On the other hand, even mathematically more robust formulations eventually elaborated by *e.g.* Goodwin, only produced regular, symmetric oscillations, which in fact still represented some sort of equilibrium. Thus, although looking for a disequilibrium theory, the fluctuations that were obtained were far *too* regular and did not look like the real ones. The explanation of their irregularity was missing, at times receiving *ad hoc* and unconvincing treatment. Finally, the difficulties associated with nonlinear formulations led to the introduction of qualitative analysis (in

[14] In fact, even in the most mature formulations, they were linearly superimposed to one another.

the style of Poincaré, Andronov *et al.*). Important as it was in the history of economic analysis, such a passage was perceived as the abandonment of a quantitative approach, the divorce of theory and econometrics.

Thus, the project of a theory of a self-sustained, endogenously generated or structural dynamics proved to be impossible to realise. At the same time, also the aim of a *quantitative* theory proved to be unreachable with the mathematics then familiar to most economists.

Two facts that jointly declared the (temporary) end of classical business cycle analysis, and thus liberated growth from the embrace with fluctuations. The awareness of that double failure contributed to the intellectual atmosphere in which was to see the light a theory of growth as an exogenously driven phenomenon.

5. A PLACE FOR EQUILIBRIUM GROWTH

Recalling the relevance of the full employment equilibrium path as a predictor, one can appreciate how far neoclassical growth went from classical business cycle theory. The latter did not deny the possibility of equilibrium, but it assigned to it the much limited role of a reference or benchmark behaviour. In fact, there was no obstacle to make it fit into its framework, *e.g.* keeping self-sustained fluctuations coupled with exogenously-driven growth (in other words, using the model of a nonlinear *forced* instead of a *free* oscillator). This would simply require assuming an unstable equilibrium, which could be Harrod's own path (Kaldor, 1940; Goodwin, 1953, 1955), a response to some other exogenously given forcing function (*e.g.* Hicks' autonomous demand trend), or else having the full employment path acting as a ceiling (Hicks, 1950; Goodwin 1951a). Not everybody, at the time, found this solution satisfactory (*e.g.* Goodwin, 1950), but apparently there was no other. There was, in fact, a mathematical problem in endogenizing growth[15]. Still, the conceptual question remains: Why was the explanation of growth placed outside dynamic theory?

[15] A problem that the multisectoral theories of growth did not encounter, precisely due to their higher dimension, a fact that partially explains their blossoming in the 1960s-1970s, at the time of a relative quiescence of aggregate growth theory.

This is linked, I surmise, to the attitude of business cycle theorists of the time towards "relevant" exogenous impulses. These and generally any forces exerting effects onto equilibrium were deemed to have also other major effects. Even with greater determination than in Macro-dynamics, classical business cycle theory was looking for structural explanations referring to basic mechanisms supposedly regulating system functioning. Thus, consistently, it was thought that, behind any long run tendency, there would be processes slowly shaping and most likely also changing the very economic structure. Demographic movements, structural change (of resource re-allocation type, and/or sectoral capital accumulation), technological progress and innovation were conceptualizations of such forces, which would interact with one another in a complicated way (a notion very close to Schumpeter's view, see Kaldor, 1954). Though with different formulations, various models put forward the view that all those were discontinuous processes, possibly of a stochastic nature. At any rate, their interaction would produce complicated outcomes: *e.g.* deforming an otherwise regular fluctuation: Shifting equilibrium in unpredictable directions (Goodwin, 1946), bringing about abrupt or catastrophic changes, and the like. All such non-systematic forces at work, most unlikely would produce anything similar to an equilibrium or a smooth trend. Structural and technological change was the only explanation for irregularity left to classical business cycle analysis. On the other hand, *realism* seemed to be justification for leaving them outside a theory of systematic dynamics.

Thus, shortly before the appearance of neoclassical theory, for theorists and theories in the business cycle program the issue was not to explain growth but, rather, how to combine an accepted though unexplained growth driver with seemingly better understood endogenous oscillations. One way to go about it was to make also growth *look* endogenous, *i.e.* by identifying it with a *statistical* average over *actual* oscillations. Alternatively, to this practical answer, it could be argued, *à la* Schumpeter, that it was the oscillation that generated growth (Kaldor, 1954). Neither of these solutions was really available for formal analysis, the former being premature, the latter implying formidable mathematical complications[16].

[16] Although hinted at in various writings, the notion of a purely statistical trend came in the open only much later.

A third conceivable solution was to have an economy switch frequency, from fluctuation to growth, and thus simply get rid of fluctuations from then onwards. This was impossible when structural change was not contemplated or even methodologically conceivable, as it was the case in classical Macro-dynamics, but it was the only alternative left. It implied a mechanism by which an explosive motion (*e.g.* an oscillation) would be converted into smooth growth (for example, growth along the full employment path) —an explosive growth limited by Hicks' and Goodwin's ceiling. Designing and embedding a *new* mechanism of structural adjustment was required.

6. THE NEOCLASSICAL *REVOLUTION*: CONTINUOUS TECHNOLOGICAL PROGRESS

Seen from the point of view of the classical business cycle program, this was the true innovation of neoclassical growth theory: Introducing exogenous but continuous Technological Progress. Classical business cycle program had it exogenous but discontinuous, hence could only generate pulses, not continuous growth. NC proved to propose a relatively simple solution, with far reaching consequences, though. Seen against the relatively poor performance of the agenda of classical business cycle, the equilibrium path had suddenly become worth to have a closer look at.

A *silent paradigm shift* took place launching the creation of modern Macro-dynamics endowed with what later, after a process of further enrichment, was to be called the "choice theoretical framework". The interpretation of the formal model was altered accordingly. With the introduction of a production function, the structure upon which a Macro-dynamic model was to be constructed would no longer be an observed set of relations and *realized* values for variables and structural parameters. It became founded upon *realizable* values of an ex-ante functional approach. This produced the well-known analysis of the dynamical properties around that equilibrium. It also proved that, under the assumed conditions and in a time horizon sufficiently long to be called *long run*, certain key variables of Harrod and the then contemporary business cycle research would cease to play a role, and be replaced by other "determinants" of growth. In particular, the self-feeding process of capital accumulation was superseded by exogenous technical progress as

the explanation of growth performance. The divorce (of TP from capital accumulation) was a surprising consequence.

As said, that Technical Progress could be a driving force of long run equilibrium was not foreign to people in business cycle research. However, they were inclined to think that it induced an irregularly fluctuating path, hard to distinguish from endogenously driven oscillations (as in Schumpeter's theory, formalised in *e.g.* Goodwin, 1946; Kaldor, 1954). Only a steady and continuous process (at a positive rate) of productivity enhancement is compatible with smooth growth, though. It is this assumption that generates the steady state path associated with the neoclassical predictions. The basic difference of neoclassical growth theory from classical business cycle analysis, therefore, is in its treatment of *exogenous* technical progress as a continuous process.

Such hypothesis appears hardly consistent with the exogeneity assumed, but is also necessary to support the other hardly intuitive but *logical* implication of the neoclassical model: That the pace of capital accumulation does *not* contribute to shape long run growth. With this move, a whole tradition of thought, which had Schumpeter as perhaps its most prominent representative, was buried.

However, something was felt to be wrong: Either in the very conception of technical progress and, hence, of its relation with investment and with the role of the latter, or else in the concept of a time horizon long enough to make capital disappear from the growth scenario. The often hot debate following the birth of neoclassical theory shows the varied reactions to those implications, with the appearance of theories of vintage capital goods, the discussion about embodied versus disembodied technical progress, Kaldor's attempt at formalising a new technical progress (investment) function, finally Arrow's learning by doing[17].

This debate never really abated. The third and most recent of the waves of interest in growth theory sprung up as a re-definition and re-interpretation of the role of capital accumulation in growth performance.

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[17] "Where recent discoveries have made a decisive contribution is in the recognition that investing and technical progress may be Siamese twins" (Hahn and Matthews, 1964, pp. 888-889).

7. CYCLING?

Harrod's thesis, that growth would arise from the very same endogenous sources of fluctuations, was left unproved. Neoclassical growth theory did not address it nor did it share it. Simply, the profession's theoretical attention shifted from fluctuations without growth to growth without fluctuation.

Correspondingly, the out-of-equilibrium interpretation of observed dynamics was abandoned in favour of one of equilibrium; and correspondingly the view spread around, according to which economic dynamics, rather than being essentially endogenously-driven, responds to *external* forces. This opened a new phase in an intellectual cycle that had already started with an early phase with endogeneity (*i.e.* an endogenous explanation) as the dominating paradigm, followed by the unsatisfactory mix produced by the classical business cycle analysis. The articulation, at about the same time, of the linear econometric methodology associated with the Cowles Commission (Morgan, 1990) and its wide acceptance were the other logical consequences of the same fact. Like in neoclassical growth theory, a similarly exogenous theory of fluctuations was proposed, refreshing a view that has been at the origins of Macro-dynamics. Oscillations would again be transient deviations from a (globally) stable equilibrium, kept alive by an unexplained tendency to be hit by stochastic impulse(s), a view that had been contemplated already but soon dismissed.

The *conceptual* match with neoclassical theory was practically perfect. The econometric models of the 1960s got along very well with a dynamic theory that focussed only upon stable equilibria and therefore guaranteed predictability in econometric models so that they complemented each other. The 1970s and the very early part of the 1980s happily lived in a new, finally unified paradigm of exogeneity. It was a sort of middle season in the nonlinear history of dynamics I am sketching out.

A *winter of discontent* arrived with a third growth wave, when the so called new theories of endogenous growth became ripe. It was *regretted* that, in the effort to get rid of fluctuations, neoclassical growth theory had also got rid of capital accumulation, there was no longer such necessity. A search for a novel role for capital accumulation and technological

progress in explaining growth performance, also in conjunction with the release of a massive new data bank to tap, ignited the *wild fire* of the so called theory of endogenous growth. A set of older ideas made a comeback as growth determinants: *e.g.* in particular, the centrality of investment (though qualified to suit modern times, with education and research and development entering into it), innovation processes, international trade, and many others. What widely different models shared, was their success in bending manifold phenomena, even the vision of the innovation process as essentially discontinuous (as was Schumpeter's), to fit steady state analysis. They were the product of a discontent with certain, also empirical, implications of the neoclassical theory, more than the product of the search for a new theoretical framework. However, at the peak of a relatively short fashion season, *endogenous* growth got coupled with the *exogenously*-driven fluctuations of the still surviving standard or linear econometric model and the new monetarist theory.

Each of these waves or "seasons" of dynamics represented a temporary equilibrium station as they had their own built-in de-stabiliser: The initial one was the search for a unified theory of growth and cycle; the second, the season of the classical analysis of business cycle ended in the failure of dealing with such issue. This last season started from the discontent with the poor relevance given to various forms of investment and to innovation, but it left the same open issue as the second. Giving opposite explanations for the modes of actual dynamics, it did not respond to the apparent demand for a unique dynamic paradigm.

A step further, into a fourth season, and a *new* brand of Macro-dynamics was produced. This fully adheres to the classical self-regulating principle: According to the real business cycle approach, all sorts of dynamics are taken to be optimal equilibrium responses to exogenous shocks, and explained by the use of the choice theoretic framework. The possibility of dis-equilibrium is not denied but is seen to be just a transient phenomenon.

In this last but one phase, there are many ideas that already belonged to Classical Macro-dynamics, the first one being that the distinction between growth and fluctuations is not really relevant: Both are types of fluctuations with different frequencies. Moreover, just like in the older approach, real business cycle theory promotes a linear view of dynamics, and in its extreme blown version (where growth as well as fluctua-

tions are stochastically induced), the unifying explanatory principle is exogeneity, once again[18]. ◄

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[18] The fundamental debt of real business cycle theory to Slutsky is clearly acknowledged and well-studied. I argued (2006) that once the engine of dynamics had been placed (by Frisch) into the *impulse*, a coherent approach could regard the propagation mechanism as of minor interest. In this light (in which it was perceived *e.g.* by Goodwin, 1951b), Slutsky's contribution can be considered as an extreme version of Frisch's oscillatory system, designed to explain persistence rather than existence of observed fluctuations. The latter, more general, category of dynamics includes regular cycles (and growth) as special cases. I am grateful to an anonymous referee for pointing out the necessity of this clarification.

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