Exogeneity Unveiled: A Critique of the Dynamical Foundations of New Classical Macroeconomics*

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Abstract

This paper discusses and critiques different exogenous business cycle theories (RBC, Lucas,..) on both the analytical and foundational grounds. The paper argues that the recent surge in interest in business cycles reopens the question posed earlier in the century on the incorporation of business cycles into economic theory (Kuznets, 1931). Moreover, the modern exogenous theories take the Frischian impulse and propagation mechanisms dichotomy and reduce it further into an impulse-only theory of the business cycle. The Frischian view of impulses as reinforcement mechanisms (Frisch, 1933) that prevent the cycle from dying down is replaced by a system that generates cycles upon impulse only. An alternative modeling strategy is proposed that calls for the inclusion of time delays and nonlinearities into the models of the business cycle. Moreover, it is argued that these time delays and nonlinearities depend essentially on the specificity of capitalist production and relations. Given the historical, methodological, and analytical arguments that contradict the claim that pure exogenous cycle theory is any kind of scientific progress, the dominance of these models in macroeconomics should be called into question.

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"Analyzing business cycles means neither more nor less than analyzing the economic process of the capitalist era...Cycles are not like tonsils, separable things that might be treated by themselves, but are, like the beat of the heart, the essence of the organism that displays them"

Joseph Schumpeter

1.1. Introduction

All theoretical models of the economic system are dynamical models that describe the general paths of the economy through time. Even static general equilibrium theories, that exclude dynamics from their analysis, are a limiting case of a dynamical system--one in which the response of "prices" to excess demand or supply functions is infinitely elastic. Although the mathematical exercise of modeling the economic system can be considered as an attempt at "professional snobbishness" or a necessary step towards the professionalization of economics, it can also be read as an attempt by the different schools of economics--the protagonists of such models--to prove a priori assumptions (and derive additional results) on the workings of the economy.[1] Often, policy implications are a by-product of such an exercise. In this light, we can understand the vitality of the mathematically inclined research project of proving the existence, stability, and efficiency of competitive equilibrium. This project occupied the best minds in the neoclassical camp for a long period of time culminating in the required proofs. These attempts according to Weintraub (1991) solidified the core, in the Lakatosian sense, of the neoclassical research program proving that competitive equilibrium (1) exists (Arrow & Debreu, 1954); (2) is efficient and optimal (Arrow, 1951), and (3) stable (Arrow and Hurwicz, 1958).

The desirability of the existence, stability, and efficiency of the equilibrium outcome of the competitive markets occupy central stage in Neoclassical equilibrium theory. The emergence of this modern "holy trinity", as a necessary outcome of the dynamics of free markets, ensures that capitalism, as envisioned by Adam Smith, transforms the individualistic selfish acts of the economic agents inhabiting the economy into a collective social good. In these mathematical exercises, the invisible hand reemerges as the condition of Pareto Optimality of the free market economic organization. The existence of equilibrium means that a vector of prices exists clearing all markets. If this set of prices does not exist, then the condition of Pareto optimality ($\frac{MU_a}{MU_b} = \frac{MC_a}{MC_b}$) never be established. However, the mere existence of something is never a guarantee that it will be found or that it will be found with minimal costs. Suppose that in a hypothetical economy an equilibrium vector of prices exists but that it would take an expenditures of half of the agents endowments to discover it, then this equilibrium would not be Pareto optimal. This observation throws a shadow of doubt on the supremacy of the free market organization to other forms of economic organization. However, the one thing that will guarantee that the economy will find equilibrium at

[1] The evolving Neoclassical tools of reasoning; Smith (literary), Walras (algebra), Marshall (the marginal revolution),..., Debreu (analysis and topology) have only, with minor variations, consolidated the initial findings of Smith.

minimum costs is the *stability of equilibrium*. If the equilibrium is stable (a zero-dimensional attractor), then the economy, modelled as a dynamical system, will converge to the equilibrium point and will stay, for all times thereafter, in that position. Furthermore, if the system is perturbed, it will return to equilibrium instantaneously.[2]

The desired outcomes of the existence, stability and actual realization of equilibrium are hence interconnected with the claim that free markets, as an economic organization set up, in the context of scarcity, will solve, in an optimal manner, the so-called economic problem.[3] David Laidler, a self-styled monetarist, says that if the assumptions of general equilibrium theory are plausible enough to warrant the achievement of equilibrium, then it is "but a short step to defending market mechanisms as a practical means of solving the economic problem in the real world, as well as in the imaginary world of the economic theorist." (Laidler, 1982).[4]

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[2]In elementary textbooks this fact is shown as follows: if the intersection of supply and demand exists then the actual equilibrium is found through the actions of the hypothetical auctioneer. Any disturbance generated by outside forces (inter-market or exogenous) is eliminated through the effects of excess supply and demand on the price.

[3]Of course, we cannot be oblivious to the political element in equilibrium economics. Hahn (1970) says, "equilibrium economics, because of its well known welfare economic implication, is easily convertible to an apologia for existing economic arrangements and it is frequently so converted" (p. 1).

[4] Laidler brings to our attention the relationship between economic models and reality. Without going into the methodology of modeling in economics and its relationship to reality--a potentially very interesting problem that I am sure have been dealt elsewhere in the literature--I postulate that economists take their models seriously. A quick random glance at economic journals will verify this contention. Most economic arguments on theory or policy are discussed in the context of solutions to models made up of a set of static or dynamic equations. More interestingly, sometimes economists disagree with the models that they set up if it happens that they fail to reinforce their beliefs. Given the seriousness of these allegations, I think that an example is over due. Morishima (1981) using Walras' tattonement process of achieving equilibrium, finds that in a two good economy with certain excess demand functions, the adjustment process paths are given by an equation $P(t+1) = F(P(t))$. The dynamical system has a two-period cycle solution and hence no equilibrium price can be found through the tatonnement process. However, Morishima adds, "this is a disappointing conclusion: the tatonnement process does not necessarily enable us to find any set of equilibrium prices ..... nevertheless we may still believe that in the real world the tatonnement method of finding equilibrium prices will work more powerfully and effectively than any mathematical theory of price adjustment" (p. 42). He adds that if the resulting difference equation for the tatonnement process is approximated by a differential equation then the dynamics will be stable and the limit cycle will be eliminated, "the conventional stability analysis in terms of differential equations implicitly assumes that required ability

Two examples of economic models that, depending on their dynamic properties, imply different view of the world are the Patinkin view of Keynesian economics, and the overlapping generations model of monetary economies (OLG).

Example 1: Patinkin and the Neoclassical Synthesis

For Patinkin (1965), "the central question which divides classical and Keynesian economics (is): the efficacy of an automatically functioning market system with flexible money wages in eliminating involuntary unemployment." (p. 315). Since involuntary unemployment means that workers are not on their supply function of labor (Laidler, 1982), then the Pareto optimality of free markets cannot be proved. As was stated earlier, the classical and neoclassical models of free markets assume an instantaneous price response to excess supplies and demand. This permits the modelers to delete 'time" from their models. From these assumptions follows the result that the classical world produces a full employment outcome rendering policy ineffective in producing a Pareto improving output mixture. Patinkin (1965) considers that the Keynesian revolution in economics put the possibility of disequilibrium on the agenda of economic research. However, the neoclassical synthesis came as a compromise between the two groups: the neoclassicals with their super-dynamic model (that makes it equivalent to a static one), and the Keynesians with their underemployment

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of the caller, so that economists have already assumed a measure of stability at the outset of their analysis when they decide to adopt the differential equation version of price adjustment equations." (p. 45). To drive home the point made earlier about the relationship between economists and their models, and hence the place of mathematical modeling in economics, we only have to observe the state of macroeconomics today. Differences pertaining to the choice of an appropriate model are not resolved empirically but methodologically. In a recent NBER conference on business cycles, Alan Blinder (and others) observed that the "New Classical counter-revolutionaries sacked the Keynesian temple not because of its empirical content was found wanting....but because its theoretical underbelly was soft. The challengers did not offer superior empirical predictions, but rather models that were smaller, cuter, and more consistent with maximizing behavior. It was therefore rational for younger economists seeking to defend the Keynesian tradition to work on strengthening the theory, not the empirics which is precisely what they did." (Blinder, 1992, p. 192). Hence, it is clear that economists take their mathematical models seriously especially if they are useful in defending their positions--to use Blinder's phrase--at their "ideological barricades". This observation on method and theoretical developments in macroeconomics has far reaching implications even from the New Keynesian perspective. Methodological debates on the business cycle in the end should resolve the issue of the institutional impediments to achieving equilibrium. If markets function a la Walras then the orthodox economists are right--the cycle can only arise for outside shocks. Hence the rigidity of prices, wages, etc. should be explained from within the institutional structure of modern capitalism--something the Marxists have been doing since Marx himself. For example, time lags in production (or sales, etc.) are a function of social relations.

equilibrium generating static model that amounts to, according to its modern arch-critic, the "determination of output at a point in time, taking history as given" (Lucas, 1978, p. 7). Patinkin (1965) puts the dichotomy in a clear fashion,

> "Equilibrium means full employment, or, equivalently, unemployment means disequilibrium. Hence our study of the corrective market forces generated by the presence of involuntary unemployment is a study of the dynamic workings of an economy in disequilibrium. And the assumption made until now, that granted flexibility, these forces will restore the economy to a state of full employment, is an assumption that the economy is consistent and stable; and that, in other words, an equilibrium position always exists and that the economy always converges to it." (p. 328).

Patinkin (1965) shows that if interest rates and prices adjust sluggishly to, for example, an underemployment producing demand shock, then the economy can persist, for a long period of time, in a situation of disequilibrium. However, in the Patinkin synthesis, the economy, through a real balance effect, can generate a slow, albeit sure, dynamic towards full employment again. Economic policy can be effective in such an economic environment if it can help the sluggishly adjusting economy pick up speed towards achieving full employment; policy becomes a Pareto improving act. Weintraub (1991) considers that the neoclassical synthesis ceded the theoretical ground to the neoclassicals while the Keynesians kept the policy aspect of the Keynesian theory.

Example 2: The OLG model of Monetary Economies

The OLG model of monetary economics forms the basis for many policy recommendations on the optimal stock of fiat money. However, the implications of the model depend on choosing a stationary solution for the dynamic equations derived, from first principles (optimizing agents). The solution of the model, given first order conditions and market clearing conditions, gives rise to the following dynamic equation for the value of real balances

$$q_{t+1} = \frac{U_1(q_t, y)}{nU_2(q_{t+1}, n)} q_t \qquad (1.1)$$

where q= real money balances

y= initial endowments

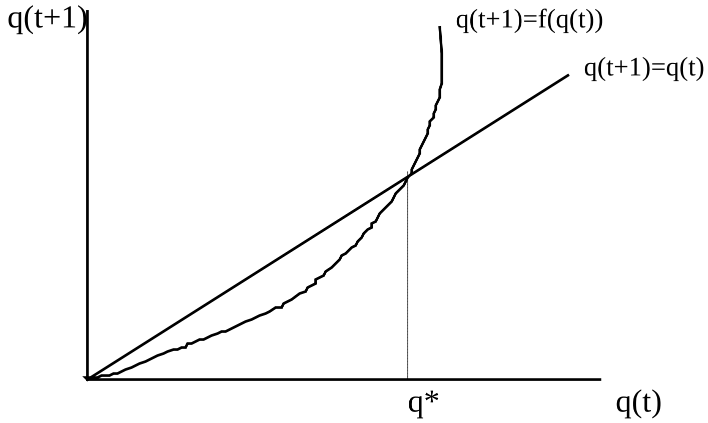
n= rate of growth of the population

U_1 and U_2 = partial derivatives of the utility function.

Equation (1.1) has different solutions depending on the relative dominance of substitution and wealth effects. If wealth effects dominate, then chaotic behavior can result

(Woodford, 1990) or (Azariadis, 1981). However, if substitution effects dominate, then $\frac{dq_{t+1}}{dq_t} > 0$ for all times. Fig 1.1 shows the phase space of eq. (1.1) when substitution effects are assumed to dominate

Fig. 1.1. Plot of q(t+1) vs. q(t)



The equilibrium point (stationary solution), q*, is an unstable equilibrium, i.e., a repellor. If we assume that initially (or that later for any reason), the economy deviates from q*, then the economy will generate an expectations-driven hyper-inflation if q-q*< 0, or hyper-deflation if q-q* > 0. Hence, in this monetary economy, the dynamics lead to either hyper-inflation or hyper-deflation. This result of a rational expectations-driven unstable economy would not find many sympathizers within mainstream economics. Positive feedback mechanisms are frowned upon within the economics profession.[5] Hence, the question that presents itself is: how do economists justify the existence of the stationary state upon which the whole menu of fiscal and monetary policy recommendations depends. In the OLG model, there are initial endowments y owned by the young generation, and since real money balances q cannot exceed the initial endowments of the economy (feasibility conditions), then the hyper-deflationary outcome is ruled out because rational agents "know" that q cannot exceed y and hence q cannot go to infinity (hyper-deflation). However, the question remains: what prevents q from going to zero (Pareto sub-optimal hyper-inflation)? The answer does not come from the fundamentals of this model but from an assumption by Wallace (1981) that fiat money has some infinitesimal value, say ε, that is guaranteed by some governmental authority. Given ε, q cannot go to zero because money has an intrinsic value ε > 0. Thus, rational agents knowing that q cannot approach zero, they do not expect hyper-inflation at all. By eliminating both possibilities, the OLG theorists argue then that the only possible attainable monetary equilibrium is q*. What originally was a repellor becomes the only possible state of the economy.

[5]Palmer (1988), a physicist by training, observed that "positive feedback is apparently frowned upon by some economic practitioners because of its conflict with a belief in a basically stable economy. Arthur's examples of positive feedback mechanisms seem unremarkable to physical scientists, but apparently generate strong opposition in some quarters." (p. 258).

However, this "proof" suffers from a fundamental error. From dynamical systems theory, the following dynamical system that represents the modified OLG model

$$q_{t+1} = \frac{U_1(q_t, y)}{nU_2(q_{t+1}, n)} q_t$$

$$\varepsilon \leq q_t \leq y$$

does not have the stable equilibrium q* as a solution.[6] Apart from that, the assumption that fiat money has some intrinsic value ε goes contradictory to what the model started out to achieve in the first place--the possibility of existence of Fiat (valueless) money in exchange economies!

Often, as in the case of the OLG model, the desired outcomes of models are achieved at tremendous costs in terms of assumptions. Kenneth Arrow, in confessing to such fact, argues that the economic assumptions (e.g., decreasing returns to scale) of general competitive equilibrium theory tended to "emphasize amplitude reducing behavior" whether the resultant dynamical system was described by a difference equations model or a differential equations one (Arrow, 1988, p. 275). Given the linear assumptions made, and defended on the grounds of practicality and computational efficiency (Lucas and Sargent, 1978), Arrow argues that many empirical systems are not explained well by linear systems. He says

> The presence and persistence of cyclical fluctuations in the economy as a whole of irregular timing and amplitude are not consistent with a view that an economy tends to return to equilibrium states after any disturbance. The persistence of unemployment undermines the assumption that prices and wages work to reduce imbalances between supply and demand." (Arrow, 1988, p. 278).

This observation by Arrow brings us to the central question in business cycle theory: the problem of incorporating business cycle theory into economic theory. The problem reduces to reconciling the equilibrium theory of economics, as formulated by the classical and neoclassical economists, with the observed facts of fluctuating prices, employment, and income, that have characterized capitalist economies since the nineteenth century. The "aberrations" of the real economy represented a thorn in the underlying belly of classical economics; if the economy is supposed to be stable, and at equilibrium, generating a vector of prices clearing all markets, how can we explain the recurrent and persistent cycles? Early on, during the times of Ricardo, Smith, and J. S. Mill, business cycles were dismissed as exogenous disturbances to an otherwise stable economy. Kuznets (1930) says, "as F. E. Trautmann points out, one of the reasons why classical economists (Adam Smith, Ricardo, J. S. Mill) have assigned to the problem of business crises such a small place in their system of economic science, was that most of the violent disturbances during the years from 1763 to

[6]Private conversation with Professor Rafael de la Llave, department of mathematics, University of Texas at Austin.

1847 were associated with outside causes, such as wars, technical changes, crops, speculative manias." (p. 383).[7] The dominant exogenous explanations in the nineteenth century were those of Moore, who associated the business cycle with movements of the planet Venus, and of Jevons who attributed the cycle to sunspots (Morgan, 1990).

However, with the business cycle phenomena asserting itself in the course of the next eighty years or so, as a fundamental aspect of the capitalist system, the classical economists were on the defensive and equilibrium theory proceeded independently and largely discarded the issue from its fold. [8]Again, the issue for the classical economists at that time was incorporating cycles into a system that otherwise generates stable outcomes. So, it comes as no surprise that the classical theorists ended up ignoring the issue completely especially that annoying voices were emerging on the "intellectual challenge" of integrating business cycle theory with equilibrium theory (Kuznets), the failure of "pure economic science" in explaining "dynamic phenomena"(Lowe), and of course, the challenge posed by Marxist economists. The economic profession seemed satisfied with the division of labor existing on this issue. On one side, the economic theorists working within the context of equilibrium economics, and on the other side of the fence, Wesley Mitchell's empirical work. One commentator during that period noted that "it is a notorious fact that the investigators of business cycles have made little or no use of economic theory. Working in the field in which lack of equilibrium is the prevailing condition rather than the exception, and finding that the principal treatise on economics center around the notion of static equilibrium, they have refused to make their own theoretical ascensions in the captive balloon of the received theories"(Shultz, 1927, p. 68).[9]

Despite the apparent theoretical impasse, the inter-war years generated a plethora of papers on the dynamics of business cycles. An important paper, from within this research activity that will prove later to be a benchmark to the emergence of modern equilibrium business cycle theory, was the paper of Ragnar Frisch (1933). Frisch provided the first mathematical model of the business cycle that implied sustained fluctuations in a capitalist economy. However, it was achieved at the theoretical expense of dividing the business cycle phenomena into an impulse and a propagation mechanism. Frisch's paper, by setting the stage for a particular modeling methodology, will come to have a lasting effect on modern business cycle theory.

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[7]Of course this view on the exogeneity of the cycle was not derived from purely empirical observations but was accompanied by the belief in "the famous theoretical impossibility of general overproduction developed with so much emphasis by J. B. Say" (Kuznets, 1930, p. 383). Moreover, for Waterman (1988), classical economists adhered to the post-Newtonian mind that abhorred disequilibrium phenomena. The social economy whether on issues of balance of trade or population should gravitate towards as equilibrium state "in the common course of nature." (p. 93).

[8]Kuznets (1930) reports that even by 1898, and despite the contributions of Bohm-Bawerk to cycle theory, the cleavage between economic theory and investigators of business cycles continued.

[9]Although Mitchell's work appeared to be void of theory, Sherman (1991) asserts that Mitchell did derive a de facto critique of Say's law "especially his historical finding that the business cycle is inherent in capitalist institutions and only in capitalist institutions" (p. 70).

This essay shows that the New Classical business cycle models of the Lucas and Real Business Cycle variants posit mathematical models that take the Frischian impulse and propagation mechanisms dichotomy and reduce it further into an impulse-only theory of the business cycle. The Frischian view of impulses as reinforcement mechanisms (Frisch, 1933), that prevent the cycle from dying down, is replaced by a system that generates cycles upon impulse only. This theoretical breakthrough, in mathematics and in content, provided finally the seemingly unachievable goal of incorporating the "business cycle" into equilibrium theory.

1.2. The Rise and Fall of Endogenous Models

Marx was the nineteenth century proponent of endogenous business cycle theory *par excellence*. Sherman (1991) says that "one may trace almost any endogenous theory back to Marx" (p. 70). Marx's theories of the structure and dynamics of capitalist economies showed their inherent instability, transient character, and susceptibility to endogenous cycles and crises--all at a time when the state of the art in economics was defined by the bizarre theories of Moore and Jevons.[10] An example of Marx's superb insight into the working of the capitalist economies is his analysis of the impact of the introduction of machinery into the production process as the dominant capitalist technique of production. Apart from what we can nowadays call micro-effects, the introduction of machinery, according to Marx, led to the following macro-level transformations: geographical allocation of investment, increase in the organic composition of capital, and more importantly to our purposes the industrial cycle. Marx says that "the factory system's tremendous capacity for expanding with sudden immense leaps......necessarily gives rise to the following cycle: feverish production, a consequent glut on the market, then a contraction of the market, which causes production to be crippled. The life of industry becomes a series of periods of moderate activity, prosperity, overproduction, crisis and stagnation. The uncertainty and instability to which machinery subjects the employment....becomes the normal state of affairs, owing to these periodical turns of the industrial cycle." (Marx, 1976, pp. 581-2). Other theorists during that period, apart from Marxists, attempted an endogenous explanation of the cycle. For example, Aftalion (1927) had an accelerator type theory of the cycle that incorporated time lags arising from the property of fixed capital. However, what defined the subsequent debate on the exogenous-endogenous front was not the competing empirically or methodically oriented theories but the introduction of a new tool of analysis into business cycle theories--namely mathematics.

The first dynamic mathematical models were cob-web type models that produced fluctuations in prices and outputs in micro markets. These cycles were produced due to the existence of time lags in either the demand for or the supply of commodities (Morgan, 1990). Although it was in the 1930's with the work of Kalecki (1935) and Frisch (1933) that time lags were introduced in business cycle research, nevertheless, the early micro cobweb-type mathematical models (Evans, 1931), (Roos, 1930), and Moore (1925) were already having detrimental consequences to economic theory. Evans (1931) in the conclusion to his paper said, "in particular, the fact of lack of equilibrium in economic systems continually, and practically, stares us in the face; yet the principal discussion from a theoretical point of view has been of equilibrium, and thus at one stroke has eliminated a major issue" (p. 61). The importance of the cobweb models comes from the fact that they are simple models of

[10]For more details on Marx's crisis theories, see essay 3.

commodity markets of the type $\dot{P} = \alpha(D - S)$. This model is of the stuff that Neoclassical theory of markets is made of. More precisely it is none other than the hypothetical auctioneer whose function is to raise and lower prices in response to excess supply and demand. Given the importance of reconciling the "dynamic" aspect of general equilibrium theory (the hypothetical auctioneer) with the fulfillment of the economic agents optimizing plans, it is obvious that the cobweb models would have had a devastating effect on the neoclassical claim of the superiority of the free market organization.[11] Recurrent cycles of the type that cobweb type models showed to exist in free market economies would eliminate the certainty of stability and existence of equilibrium price vectors, hence representing a failure of free markets to generate an optimal solution to the economic problem--the modus vivandi of neoclassical economics.

Hence, mathematics was ideologically neutral in those days. It was used by the neoclassicals in the Walrasian tradition of n-equations and n-variables type static models. By the same token, there appeared an avalanche of models that questioned the stability and the smooth dynamics implied by the Walrasian systems. Although some of these models were confined to the explanation of cycles in a particular industry (e.g. Tinbergen and the shipbuilding industry), others used these models for explaining business cycles and crisis in capitalist economies. Proponents of the latter view were Evans (1931) and Roos (1930). Moreover, time lags in production, and in other facets of economic activity, were a major element in Kalecki's (1935) theory of the business cycle which was one of the first mathematical models of a macroeconomic business cycle. Kalecki's model produced cycles in investment, consumption, and output mimicking the stylized facts of the cycle that Mitchell's empirical work was observing in capitalist economies (Mitchell, 1941).

Within the context of the increased usage of mathematics in economics, the importance of developing a mathematical tool or proof for the endogeneity of the business cycle in capitalism cannot be overstated. Kalecki, as a Marxist-Keynesian, believed that capitalist economies generated business cycles because of their internal structure. Kalecki's model was derived from Marxian, and what became to be called Keynesian assumptions, about the structure and dynamics of the capitalist economy. The assumptions are: it is a two agent economy where the agents are workers and capitalists. Capitalist investment depends positively on profits and negatively on the capital stock. Workers do not save and hence spend all their income. In addition, there exists a time lag between investment orders and delivery of

[11]It is important that there exists a one-to-one correspondence between the vector of prices generated by supply and demand forces in the market (dynamic considerations) and the vector of prices generated by the economic agents optimization plans. If one fails then the other will fail too. According to Weintraub (1991), "in the Arrow-Debreu model, the coordination of agents' plans through optimization is necessary for equilibrium, and the clearing of markets as a balance is necessary for equilibrium, but they jointly are necessary and sufficient for equilibrium." (p. 107). Hence the Arrow-Debreu model linked "the equilibrium price vector in a general equilibrium model with a Nash equilibrium, which was really the fixed point of a mapping from prices "given" to prices "induced by the actions of agents." Thus the equilibrium metaphor shifted from a balance between market forces to a price that, once established by the desires of the agents, would not be modified as long as the desires of agents remained unchanged" (Weintraub, 1991, p. 108).

investment goods. Kalecki's (1935) model derives the following dynamic equation of capital accumulation

$$\dot{K}(t) = \frac{a}{\tau}K(t) - (\frac{a}{\tau} + n)K(t - \tau)$$

The solution to the above equation, which represents the evolution of capital stock through time, depends on the parameters a, τ, and n.[12] Kalecki chose the parameter values that produced constant oscillations in K because “that case is of particular importance as it appears to be nearest to actual conditions. Indeed, we do not observe any regular progression or degression in the intensity of the cyclical fluctuations”(Kalecki, 1935, p. 336). This choice generated a series of criticisms that paved the way for the adoption and institutionalization of Frisch's (1933) impulse-propagation method in economic modeling that prevails up till today. The criticism concentrated on the justification behind Kalecki's choice of an extremely small set of the parameters of his model. Mainly criticism came from Frisch and culminated in a mathematical rebuttal of Kalecki's solution (Frisch and Holme, 1935).

Writing about the incident at the 1933 Econometric Society meeting where both Kalecki's (1935) and Frisch's (1933) papers were presented, Richard Goodwin remarked “Alas, Frisch was there to point out that since the Greeks it has been accepted that one can never say an empirical quantity is exactly equal to a precise number....this was a deadly blow to Kalecki” (Goodwin, 1989, p. 250). What warranted the "deadly blow" was Kalecki's arbitrary choice of parameter values that insured constant oscillations solutions. Kalecki's appeal to reality as an excuse for fixing the parameters of his model was understandable, albeit indefensible, if mathematical models in economics were to be taken too seriously.[13] From this fundamentalist point of view, all models of the business cycle are supposed to generate empirically observed fluctuations without excessive restrictions or assumptions such as parameter fixing. What happened later will shed a light on the seriousness of this incident in the history, and subsequent development, of business cycle theory right down to our day.

As I stated earlier, Kalecki, as a Marxist, believed that the institutions and internal structure of capitalism produced the business cycle. His first reaction was to defend his position but, from the beginning, it was obvious that the methodological terrain that he was drawn into was unyielding. Responding to early criticisms of his model, he would confess to the weakness of his method by saying "I must admit that there is one very important gap in this part of my argument. One should not be satisfied with merely *stating* that no clear, regular progression or damping in the amplitude of business fluctuations can be observed in reality; one should explain this constancy in the intensity of fluctuations"(Kalecki, 1990, p.

[12]For more details of the model and its solutions, see essay 2.

[13]J. C. Andvig notes that the subsequent neglect of Kalecki's model by economic historians, such as Schumpeter, was due to his "mathematical" error. (quoted in Osiatynski, 1990, p. 476). In another paper, Andvig states that Kalecki's method of specifying the parameter values that generated constant cycles in his model was a "clumsy method ....(that) explains why Kalecki's early papers did not make any greater impact on the group of mathematical economists who, like Frisch, were preoccupied with the problem of mechanical characteristics of the cycle."(Andvig, 1981, p. 717).

119). Later, he would make one more attempt to rescue his linear model by assuming that one of the parameters depends on the elasticity of credit which in turn depends on fluctuations (Kalecki, 1990). However, he soon abandons these efforts and develops shock-driven cycle models and becomes an advocate of the importance of exogenous shocks in the maintenance of fluctuations. Such was the power of mathematics and its economic interpretation by Frisch at that time.

However, there is a technical reason to the rise of this debate in business cycle modeling. Kalecki's linear model, as a model of fluctuations, needed such parameter fixing because mathematically all linear models generate periodic solutions only for a very limited set of the parameters. The basic flaw with linear models, such as Kalecki's, is their sensitivity to parameter variations--solutions of linear systems change from explosive, to constant, to damped, as we span the parameter space. While linear models were common in economic modeling, apparently for their simplicity and the widespread belief in the intrinsic linearity of economic phenomena, different mathematical tools had to be used in business cycle modeling if economists wished to solve the problem of persistent fluctuations. The above discussions on Kalecki's model and linearity tells us that the role played by Ragnar Frisch in that determining period was very important to the future development of the models of the economy. Let us, then, look at what Frisch had in mind.

Frisch's most important paper that spelled out in exact terms the impulse-propagation method in analyzing business cycles was published in 1933. In the paper, Frisch models a hypothetical economy by considering the following mixed difference-differential system

$$y = mx + \mu \dot{x}$$

$$\dot{x} = c - \lambda(rx + sz)$$

$$z_t = \int_{\tau=0}^{\infty} D_\tau y_{t-\tau} d\tau$$

The solution to the above model, as shown by Frisch, for the most part, displayed damped oscillations (a result similar to Kalecki's model of 1935). This prompted Frisch to state "but in reality the cycles we have the occasion to observe are generally not damped. How can the maintenance of the swings be explained? Have theses dynamic laws deduced from theory and showing damped oscillations no value in explaining the real phenomena, or in what respect do the dynamic laws need to be completed in order to explain the real happenings? They (dynamic laws) only form one element of the explanation: they solve the propagation problem. But the impulse problem remains" (Frisch, 1933, p. 197). Hence, starts the propagation-impulse dichotomy in business cycle theory. The impulse problem is solved by considering that random shocks maintain the cycles in an otherwise damped system. What Frisch has done was to formulate mathematically Wicksell's analogy of the rocking horse and the stick; the rocking horse being the cycle, and the stick the exogenous shocks.

However, Frisch was very much interested in an endogenous explanation of the cycle and in no way started his work with an a priori assumption about the methodological necessity of exogenous explanation to the cycle. Frisch in one of his untranslated articles asserts that economists should not be satisfied with theories that explain cycles with forced oscillations. He says, "the bundle of phenomena we call business cycles is...a complex we have to attack as composed of free oscillations if we as economists are ever able to understand it. The explanation of the *cyclical character of oscillations must be sought in the inner structure of the system*" (as quoted in Andvig, 1988, p. 709, italics mine).[14] Moreover, Gordon (1985) observes that the pre-Keynesian theorists concerned themselves with the propagation mechanism because they wanted an endogenous explanation of the cycle.

However, Frisch's method gained the upper hand in the methodology of economic modeling for various reasons. First, the incident and debate with Kalecki set a precedent in methodology for subsequent mathematical models. Second, the interpretation given to Frisch's dichotomy would rigidify the impulse-propagation dichotomy. As we will see later, the Real Buisness Cycle theorists and Robert Lucas would interpret literally the "propagation mechanisms" as mechanisms that transmit exogenous shocks through an otherwise stable economy. This propagation leads to the transformation of "shocks" into "cycles". While, for Frisch, the propagation-impulse dichotomy was developed to explain how impulse sources outside the economy can maintain a cycle generated from within the system. Finally, the method Frisch had a theoretical appeal to economic theory. As Andvig (1988) states that by adopting the impulse-propagation dichotomy, Frisch not only solved his model, but "he thereby also solved the use of linear functional equation systems as a method of theoretical study of business cycles" (p. 717). He then adds that "Frisch's solution also had some appeal from the theoretician's point of view. Frisch's economy fluctuated around some stationary state or equilibrium growth path in a damped way. Left to itself the economy would approach some kind of equilibrium state, which made static reasoning methods relevant. The fluctuations were only kept going by external shocks and this agreed with most of the theoreticians intuition" (p. 717). Morgan (1990) also held a similar view that "the particular attraction of Frisch's rocking horse model was that it allowed for the free and damped oscillations desired by economic theoreticians and yet was compatible with observed business cycle data which were irregular and undamped." (p. 92).[15]

Thus, we can see that a certain methodology was developed after the 1933 paper by Frisch. This methodology was later applied to the macroeconometric models developed by Keynesian economists such as Lawrence Klein and others. An important paper in this respect was the one by Adelman and Adelman (1959). In this paper, the authors simulated the Klein-Goldberger model of the US economy and showed that the model, in the absence of outside shocks, produced damped oscillations. Gordon (1985) situates this paper within the history of business cycle theory as the "evidence (that) naturally helped to shift the attention of economists from propagation mechanisms to the sources of impulses, and soon the profession lost interest in business cycle theory per se as it became caught up in the emerging

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[14]It is ironic that Frisch, in one of his early model sketches that appeared in Norwegian, constructed a model that Andvig (1988) said it implied "a system of 37 to 38 mixed non-linear difference-differential-integral equations" (p. 710). This system which now could be solved using modern techniques, would generate endogenous business cycles with no need for erratic shocks.

[15]In this respect, it is important to remember the importance of static reasoning in economics. Starting with Samuelson's *Foundations of Economic Analysis*, comparative statics became the main tool in mainstream economics for studying the effect of a change in a certain parameter on the behavior of the economy. Policy analysis would be almost inconceivable without the comparative statics method which, incidentally, presupposes the existence of equilibrium point in the first place.

debate regarding the relative role of monetary and fiscal shocks. Since that time the business cycle has been viewed as resulting from irregular impulses whose effect on economic activity is transmitted by a complex propagation mechanism." (pp. 6-7). After the Adelman and Adelman paper the business cycle then will be declared "dead", for one reason or another, and theoretical interest dwindles until Robert Lucas (1975) revives the "business cycle", albeit in different form.

Now the question remains: could things have developed differently? To give an answer would be an exercise in post facto historical simulation. However, some questions could be raised concerning the possibility and/or desirability of some alternative methodological paths that could have been taken once it was obvious that the mathematization of business cycle theory was leading the pure endogenous explanations astray.[16] Some questions come to mind. First, are all relationships in the economy between variables (e.g., between profits and wages, between investment and profits,...) linear in nature? Second, even if we assume that linear models represent exactly economic reality (something that cannot be proved, see Blatt (1979, 1983)), can economists, as Tinbergen (1940) tried to convey, pick the solutions that they think represent economic reality from the myriad of possible solutions. More precisely, can the damped oscillations solutions of the linear models represent the business cycle if the damping coefficient is small, i.e., for all practical purposes the cycle takes a very long time to die out? Since we know that the dampening coefficient depends on the parameters of the model (Tinbergen, 1939) and in turn these parameters--the marginal propensity to consume, the coefficient measuring the response of investment to profits etc.--can change form within the system (Koopmans, 1940), then the concept of superposition of solutions becomes an attractive solution. This brings us to the third question; if these parameters are changing, and hence time dependent, then their evolution through time can produce a myriad of possible movements--damped, explosive, and constant. Hence, the true motion of the economy can be seen as a superposition of these movements.

If any of the above modeling methodologies was adopted, it could have dampened the effect that Frisch's method had on the subsequent demise of endogenous cycle theory. Subsequently, many interesting observations and theories of the crisis and the business cycle would have gained more audience and further analysis. In conclusion, we can say that the initial rise in endogenous theories of the cycle of both the literary and mathematical types represented a serious challenge to equilibrium theory prompting Hayek to take notice and start a serious project of incorporating business cycles into general equilibrium theory (Kim, 1988), and (Andvig, 1988). The initial rise of the endogenous models of the business cycle was followed by their immediate demise because of their linear formulations and the wide spread adoption of the Frischian impulse-propagation dichotomy. Althusser said "as Hobbes put it, geometry unite men, social science divides them. 'Economic Science' is the arena and the prize of history's great battles." However, mathematical economics, in the final analysis, is economic science in its most pure form. The irony remains, that with what Frisch came to rescue the endogenous models of the cycle, will be later used against its very foundations.

[16]It should be noted that I make no claim that exogenous shocks play no role in the business cycle phenomena of capitalist economies. The stress on endogenous explanations of the cycle means only that the business cycle rise and persistence should be understood from within the institutional framework of capitalist production, exchange, and distribution. Once that is established then external shocks can be incorporated.

1.3. Critique of Lucas and RBC theories

We have seen in the previous sections how the Frischian methodology provided the window, from which the exogenous school, entered the scene of modeling mathematically the business cycle. However, this process, started by Frisch's differentiation between impulse and propagation mechanisms, was swept away when Keynesianism became the dominant macroeconomic theory especially in its static IS-LM variant. Shifts in the IS schedule became the mainstream view on the cause of the business cycle. In addition, historical events helped in the fading away of the earlier debate on business cycle theory. During the post-W.W.II period and until the late sixties, the business cycle was mild relative to earlier periods in the history of capitalism prompting many to declare the business cycle as "dead."

All was well until the onset of economic crisis in the seventies in the major advanced insustrial societies. Consequently, there was a revival of interest in the business cycle. This revival often coincided with a devastating critique of Keynesianism. Moreover, the new business cycle theory initiated by Lucas (1975) attempted to revive the old issue of embedding the business cycle into equilibrium theory. It is no coincidence that the name "equilibrium business cycle theory" encompasses the work of Lucas and Real Business Cycle theories (Kydland, Plosser,....). Lucas and Sargent (1978) go to the crux of the matter saying that, "increased attention and respect are accorded to the theoretical casualties of the Keynesian revolution, to the ideas of Keynes's contemporaries and of earlier economists whose thinking for years has been regarded as outmoded" (p. 57). In this section, I argue that the Lucas and RBC theorists take the Frisch impulse-propagation dichotomy and use it to derive an impulse-only theory of the cycle. In this process, Frisch's original project of explaining the business cycle as a phenomenon of "free oscillations", was lost. The models of Lucas and RBC produce cycles only in the case of exogenous shocks--otherwise the economic systems are monotonically stable.

(i) The Lucas Model

Lucas's formal model of the equilibrium business cycle is presented in Lucas (1975). However, this technical paper was followed by a series of papers on methodology. These papers expounded on the methodological aspects of embedding business cycle theory into equilibrium theory (Lucas, 1977, 1978, 1980). The building blocks of the basic Lucas (1975) model consists of rational optimizing agents that are geographically separated in an island-type economy. These individuals lack the ability to obtain perfect information on prices. Hence, if a certain general price increase (inflation) is perceived by the agents as an increase in the relative price of the commodity they are producing, then their immediate rational reaction would be to increase the production and supply of the commodity. Once the agents discover their error --the error being that they confused a general price level for a relative price increase--which should have no effects on their production plans, they scale back production. Hence, the business cycle is a result of misperception on the part of rational optimizing agents. In addition, since the equilibrium state is maintained throughout time, then the cycle is perfectly compatible with economic theory. In technical terms, the model consists of Lucas Supply Function

$$y_c^t = b(1-\beta)(p_t - \bar{p}_t) \tag{1.4}$$

$b = \text{constant}$

$\beta = \text{measures the reaction of agents to price changes.}$

$y_c^t = \text{cyclical level of output}$

$\bar{p}_t = \text{mean price level}$

If β =1, then there is no response of agents to price changes. Hence, agents do not make mistakes in perceiving the increase in price level as an increase in the relative price => they do not change their production plans => $y_t^c = 0$. However, if β=1, then the agents will "perfectly" believe that the increase in price is an increase in the relative price level, and hence will alter their production plans accordingly => $y_t^c > 0$. The business cycle is a resultant of agents confusion about general and relative price level changes. Furthermore, because the change in the price level could be the result of an unanticipated increase in the money supply, the model provides a completely exogenous explanation of the cycle.

One of the criticisms levelled against this model (Tobin) is it's inability to explain the persistence of the cycle. If agents misperceive the price changes at time t then that may produce a cycle lasting only for one period, up till t+1. Since consistent serially correlated errors or persistent errors are not compatible with rational behavior, then the model according to Tobin "can scarcely explain more than transient disequilibrium in the labor market" (as quoted in Lucas and Sargent, 1978, p. 65).

Lucas and Sargent responded to such criticism by invoking the impulse-propagation mechanism dichotomy. They say,

> "This criticism is fallacious because it fails to distinguish properly between "sources of impulses" and "propagation mechanisms" stressed by Ragnar Frisch in a classic 1933 paper that provided many of the technical foundations for Keynesian macroeconomic models. Even though the new classical theory implies that the forecast errors which are the aggregate demand "impulses" are serially uncorrellated, it is certainly possible that "propagation mechanisms" are at work that convert these impulses into serially correlated movements in real variables like output and employment." (Lucas and Sargent, 1978, pp. 65-6).

This criticism of the Lucas-Sargent model. on the question of persistence of cycles in their model. prompted them to invoke the Frischian impulse-propagation dichotomy. Several propagation mechanisms were proposed by Lucas and Sargent (1978) such as costs of adjustments and search. However, an important mechanism (that will be also important in RBC models) is according to Lucas and Sargent "already present in the most classical growth models" which makes demand for assets in one period dependent on the initial stock of capital; a relationship of the type $K_t = f(K_{t-1})$. Hence, the Lucas supply function can be augmented by a lagged value of the cyclical part of output to become

$$y_t^c = \lambda y_{t-1}^c + b(1-\beta)(p_t - \bar{p}_t)$$

where 0 < λ < 1.

Given this assumption, an iterative solution (see Woodford (1990) for method) of the model will give

$$y_t^c = y_t^c = \sum_{j=0}^{\infty} \lambda^j b(1-\beta)(p_{t-1} - \bar{p})$$

Thus, the cyclical component of output, at any time t, is a function of all past shocks to the economy as they appear in the misperceptions, $0 \le \beta < 1$, of rational economic agents. Henceforth, the cause of the cyclicality of the economy lies outside the economic system. Endogenous factors play only the role of "propagators" of the cycle, and by no means, the initiators of the cycle. In addition, since the economic agents are "on" their supply functions then the economy is in equilibrium at all times. Notice that if $\beta = 1$ then agents do not make misperception mistakes and consequently the economy does not fluctuate. In addition, if $\lambda = 0$, no fluctuations occur. Hence, we see the importance of the incorporation of past values of the cyclical output in the Lucas supply equation. For this ad hoc inclusion of past values, Lucas and Sargent had to endure severe criticism. Blinder (1985) says that there was no theoretical justification for adding the lagged value of the GNP in the Lucas equation. Blinder adds, "while criticizing the Keynesian answers to everything, the new macroeconomic paradigm created an intellectual puzzle where none existed before: why do economic fluctuations persist?" (p. 107). He notes that the various persistence mechanisms made the new classical models appear more like Keynesian ones. This, according, to Blinder led to the revival of Keynesian economics by default. He further says "it has been victorious by default, I think, because the new classical economists failed miserably to meet the criterion for a Khunian paradigm change. Not only did it fail to explain any empirical phenomenon that baffled Keynesian analysis, but it created *anomalies (like persistence)* where Keynesian economics offered coherent explanations" (Blinder, 1985, p. 107, emphasis mine).

While it is not true that the question of persistence of oscillations was never an "intellectual puzzle"--it only seized to be so since the advent of the Keynesian static thinking--Blinder is suggesting, without saying it explicitly, that the new classical economists have undone their revolution the moment they introduced persistence into their models. Market friction is the stuff that Keynesian economics is made of, especially the neoclassical variant of it. Hence, macroeconomics had made a complete circle.

However, the new classical models are revolutionary.They model the economy as a stable system where oscillations are imposed on the system by exogenous shocks. This methodology has a resemblance to the Frischian method. However, a closer look reveals otherwise. If we look at the lagged GNP-augmented Lucas supply function, and assume, for example, that $\beta = 1$ (either agents have complete information or we have an inflationary environment), then the supply function becomes

$$y_t^c = \lambda y_{t-1}^c \quad \text{where } 0 < \lambda < 1$$

The solution of the above equation given that 0 < λ < 1 is monotonically stable. Hence in the absence of unanticipated shocks and information lags (both exogenous factors to the economic system at hand) the economy is stable. This charcteristic of the Lucas model defines it as an impulse-only business cycle theory. In contrast to the initial Frischian insight into exogenous shocks and propagation mechanisms, the shocks in the Lucas model are not a source for the maintenance of the cycle but are the initiators of cycles.[17]

Some commentators on modern business cycle theory tend to miss this point. Ferri and Greenberg (1989), for example, situate equilibrium business cycle theory within the tradition of multiplier-accelerator models. However, we know that the multiplier-accelerator models were an attempt at an endogenous modeling of the cycle (Gordon, 1986). They represented a sort of a dynamic Keynesian cycle (Laidler, 1992) which incorporated the Keynesian multiplier C= mpc. Y and the accelerator mechanism I = v (Y(t) - Y(t-1)). In contrast, the new classical models are low dimensional models that generate cycle solutions only by outside shocks. Thomas Sargent says "if the initial conditions of low-order deterministic linear difference equations are subjected to repeated random shocks of a certain kind, there emerges the possibility of recurring, somewhat irregular cycles of the kind seemingly infesting economic data. This is an important idea that is really the foundation of macroeconometric models, an idea that was introduced into economics by Slutsky (1927) and Frisch (1933)" (as quoted in Kim, 1988, p. 62). This mathematical method gives a perfect combination: a linear deterministic system of difference equations that guarantee stability and the existence of equilibrium, plus outside shocks, guarntee the emergence of fluctuations in the economic variables similar to those observed in empirical data. Voila! in one strike, new classical macroeconomics have explained the business cycle, and in addition, in the presence of rational expectations, the policy ineffectiveness hypothesis.[18].

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[17]This question of differentiating between "initiator" and "propagator" could be a semantic one but albeit a very powerful one in forming opinions about the source of the cycle in capitalist economies. There are those who argue that the whole New classical counterrevolution was a revolution in discourse, method, and the use of persuasive language (Laidler, 1992) and (Rosetti, 1992). Due to the Lucas and Sargent "discourse" on the business cycle, the belief in the complete exogeneity of the cycle spread like fire in a haystack amongst the economic profession prompting Woodford (1990) to comment "such models are quite familiar--so familiar indeed that many readers may wonder how an economic explanation of fluctuations could be of any other kind." (p. 2). Blinder (1992) also says "how and why the market clearing approach caught on in this environment will, I venture to guess, be a source of consternation and amazement to economists of the twenty-first century--much as modern physicians marvel at the eighteenth century belief in the curative effects of bleeding" (p. 105).

[18] The rational expectations hypothesis is a necessary condition for the validity of the policy ineffectiveness hypothesis which depends upon the assumed existence of equilibrium. The equilibrium price, for example, in a supply-demand system exists independent of the expectations of economic

(2) The Real Business Cycle Model (RBC)

Although many variants of the RBC model have appeared in the literature (Kydland, 1982) and (King, Plosser, and Rebelo, 1988), I will take Plosser's (1989) model as a starting point because it is the simplest model that includes all the fundamental building blocks common to all RBC models. Plosser (1989) motivates his model by providing a methodological introduction to RBC modeling which criticizes Keynesian economics on two grounds: (1) the absence of a consistent foundation based on the choice theoretic framework of economics; and (2) the static nature of the Keynesian model that contains none of the dynamic elements found in earlier business cycle research of Mitchell and Von Hayek. According to Plosser, the introduction of dynamics into the Keynesian system was at best done in an ad hoc fashion such as the accelerator mechanism and the wage and price adjustment equations. The building blocks of the RBC model (Plosser, 1989) are

(i) Preferences: the economy is populated by identical agents that live forever. The agents utility function is of the form

$$U_t = \sum^{\infty}{}_{s=0} \beta_u^{t+s} u(C_{t+s}, L_{t+s})$$

(ii) Production: A single final good, Y_t, produced by a CRS technology given by

$Y_t = \Theta_t F(K_t, N_t)$

K_t = predetermined capital stock at time t – 1

N_t = labor input at time t

Θ_t = a temporary shift factor that alters total factor productivitity

(iii) Capital Accumulation: the produced commodity Y can be either consumed or invested. The invested commodity becomes part of the capital stock that is available for production in the next period. The capital stock evolves according to the following dynamic equation

$K_{t+1} = (1- \delta)K_t + I_t$

(iv) Resource Constraints: the resource in each period are

$L_t + N_t \leq 1$

$C_t + I_t \leq Y_t$

The computation of equilibrium prices and quantities is simplified by considering a representative agent: Robinson Crusoe. His optimal choice of consumption, work effort and

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agents. Given this equilibrium price, then rational expectations hypothesis says that the economic agents know this equilibrium price and expected to prevail in the future periods (Ferri and Greenberg, 1989). It follows that if the solution to the underlying dynamical system of the economy gives rise to disequilibrium phenomena, or to indeterminacy of solutions, then the rational expectations assumption cannot be applied by the economic agents. For example, the policy-effectiveness hypothesis rests upon the existence of equilibrium price p_t^*

output over time will represent the per capita outcome of a competitive market economy. The Lagrangian associated with the problem is

$$L = \sum_{t=0}^{\infty} \beta^t [u(C_t, 1 - N_t)] + \sum_{t=0}^{\infty} \lambda_t [\Theta_t F(K_t, N_t) - C_t - K_{t+1} + (1-\delta) K_t]$$

The first order conditions will give us time paths for the variables based on the following time-invariant decision rules

$$C_t = C(K_t, \{\Theta_{t+s}\}_{s=0}^{\infty})$$
$$N_t = N(K_t, \{\Theta_{t+s}\}_{s=0}^{\infty})$$
$$K_{t+1} = K(K_t, \{\Theta_{t+s}\}_{s=0}^{\infty})$$

Plosser (1989) says that an "important feature of this economy is that in the absence of changes in technology (i.e., $\Theta_t = 0$, for all t), and given some initial capital stock, per capita values of consumption, hours, capital and output, converge to constants, referred to as the steady state." pp. (73-4). After establishing (or asserting) that the dynamics of the decision rules are monotonically stable, Plosser proceeds to approximate the nonlinear decision rules by linear equations. In addition, this assumption allows King et. al (1988) to isolate the exogenous shocks as the origin of the business cycle. The procedure involves approximating around the steady state which gives us, for example, for the capital stock equation

$$\hat{K}_{t+1} = \mu_1 \hat{K}_t + \Psi_t \Theta_t + \Psi_2 \sum_{j=0}^{\infty} \mu_2^{-j} \Theta_{t+j+1} \qquad (1.5)$$

where $\mu_1, \mu_2, \Psi_1,$ and Ψ_2

are complicated functions of the underlying parameters of tastes and technology. Hence, the next period's capital stock depends on the current capital stock and the current level of productivity and subsequent discounted productivity shifts. Plosser adds that the condition on the problem "pretty much" guarantee that $\mu_1 < 1$ and $\mu_2 > 1$. This, in turn, guarantees the stability of the solution. Given eqaution (1.5) for capital accumulation then through an iterative method, the cyclical component of the stock, $\hat{K}$, can be written as a function of past shocks i.e., $K_t = f(\sum_{j=0}^{\infty} \Theta_{t-j-1})$ which means that the cycle is completely exogenous.[19]

[19] Long and Plosser (1983) construct a multisectoral RBC model that due to the existence of multi-sectors produces a dynamical system that is capable of generating damped oscillations irrespective of external shocks. Although this is a special case of RBC models, Long and Plosser say that since the spectrum of tome series do not show a pronounced peak at business cycle frequencies, "it is probably correct to say that such a condition is not a key feature of the business cycle." p. 56. They say that a more important characteristic of the cycle is the tendency of outputs in the different

Again, if exogenous shocks do not exist then the model reduces to $\hat{K}_{t+1} = \mu_1 \hat{K}_t$ which has a stable solution.

The assumption of linearity is fundamental in the above results. In King et al (1988), the authors explicitly acknowledge in a footnote that the "approximation method rules out certain phenomena that may arise in the basic neoclassical model, such as a humped shape transition path for investment." (p. 204). This of course rules out the possibility of the rise of endogenous chaotic fluctuations. Given that the starting point of all RBC models is Solow's growth theory, then it is apparent that the RBC models are built around a model that has a stable solution--a well known fact of the Solow model even in discrete form (Boldrin, 1988).

Hence, in the RBC model, the representative agent responds to productivity shocks by varying his work effort, output, consumption and investment over many periods. In this sense, the optimal decisions of Robinson Crusoe constitute the business cycle which renders the business cycle Pareto efficient. Plosser (1989) concludes "therefore the business characteristics exhibited by the economy are chosen in preference to outcomes that exhibit no business cycles." (p. 56). Who said that business cycles and equilibrium economics are irreconcilable? Quoting Plosser again, "the appeal of this line of research is the apparent power of some very simple economic principles to generate dynamic behavior that was heretofore thought to be incompatible with any notion of equilibrium" (Plosser, 1989, p. 71.

The RBC model actually is a step ahead of the Lucas model. The model does not have to respond to the critique by Blinder, stated earlier, with regards to the Lucas model. It represents the final model in the long and arduous journey of neoclassical economics to incorporate business cycles into equilibrium economics. However, what was accomplished was the merging of the two phenomena into an indistinguishable whole.[20]

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sectors to move together--something they show that their model generates irrespective of shocks. Although the ability of the economy to generate BC phenomena endogenously (although damped) in this RBC model depends on a rather non-fundamental aspect of the economy--the mere existence of similar sectors--it is apparent that the authors do downplay this fact although they say that "Nevertheless, we feel that the ability of simple rational expectations model such as ours to generate damped oscillations in the expected output path is an important and little appreciated fact!!". The question is little appreciated by who if not by the authors themselves who in the preceding sentence to the above quotation deny the relevance of even damped oscillations to the study of business cycles. My hunch is that this downplay of the simple non-exogenous dynamics in the only RBC model that generates such a dynamic (due to the existence of different sectors) is an attempt to construct a methodology among economists that avoids opening a Pandora's box of more complex dynamics that would invalidate all the results of the RBC models.

[20]Given that the stable growth models of Solow took over form the Harrod-Domar model as the centerpiece of growth theory (Laidler, 1992), it becomes tempting to

1.4. Toward a Nonlinear Theory of the Business Cycle

Now the question remains: could things have developed differently? To give an answer would be an exercise in ex-post counter-factual simulation. However, there are three methodological paths that could have been taken to continue the research program of endogenous business cycle theory.[21] First, relationship between economic variables (e.g., between profits and wages, between investment and profits,...) can be modeled in a nonlinear fashion. Second, even if we assume that linear models represent economic reality (something that cannot be proved, see Blatt (1979, 1983)), economists, as Tinbergen (1940) tried to convey, can pick the solutions that they think represent economic reality from the myriad of possible solutions. More precisely, the damped oscillations solutions of the models (such as Frisch's and Kalecki's) may represent the business cycle if the damping coefficient is small, i.e., for all practical purposes the cycle takes a very long time to die out. Third, the business cycle can be represented by a superposition of different solutions if parameters are time-dependent--this is the case if the dampening coefficient depends on the parameters of the model (Tinbergen, 1939) and in turn these parameters--the marginal propensity to consume, the coefficient measuring the response of investment to profits etc.--can change from within the system (Koopmans, 1940). The time evolution of these models can produce a myriad of possible solutions--damped, explosive, and constant. The true motion of the economy can be seen as a superposition of these movements.

The first solution remains the most attractive. An important aspect of the capitalist economy is the prevalent nonlinearities in economic dynamics. Some nonlinear relationships in economics are, according to Zarnowitz (1993): investment, interest rates, total profits, final sales, profit-sales margins, and finally the important relationship between the cost of labor and finished output prices. Nonlinearities may arise from the dependence of model parameters on economic variables. An important nonlinearity in economic dynamics rises from the dual nature of the wage: as a cost for producers and as a source of demand for the commodities produced. The dual nature of the wage induces the existence of a two-regime macroeconomic structure: one where profits and wages are compatible and another where profits and wages are contradictory.[22] The Post-Keynesian critique of Say's law takes this duality as its starting point of analysis--any excess supply of labor leading to a lower wage will create a cumulative causation effect that leads to reinforcement of the initial excess supply (Sherman, 1991). According to Minsky (1986), in monetary economies the excess supply in the labor market will persist even if the money wage decreases as a result of the excess supply. In all of these views, the dual nature of the wage, as cost and as source of demand, manifests itself in the impossibility of the achievement of full employment equilibrium.

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view the history of growth and business cycles models as a grand movement from unstable models towards stable ones.

[21]It should be noted that I make no claim that exogenous shocks play no role in the business cycle phenomena of capitalist economies. The stress on endogenous explanations of the cycle means only that the business cycle rise and persistence should be understood from within the institutional framework of capitalist economies. Once that is established then external shocks can be incorporated.

[22] For a theoretical analysis of this nonlinear regime structure in the economy, see Bowles and Boyer (1991) and Marglin and Bhaduri (1991). For a mathematical model of the resultant business cycle, see Dibeh (1995).

In technical terms, nonlinear models generate fluctuations that are persistent. The complex dynamics generated from these models range from limit cycles (representing periodic cycles) to chaotic motion (representing irregular fluctuations). Stable limit cycles are the best representation of a business cycle. The range of parameters of the model allowing limit cycle solutions is usually wide and hence eliminates the theoretical appeal of the Frischian critique. The nonlinear formulation eliminates the necessity for an outside shock to explain the persistence of fluctuations in a capitalist economy. The nonlinear system generates and maintains the cycles from within, hence collapsing impulse-propagation dichotomy into a unified system.

1.5. Conclusion

In this essay the recent surge in interest in business cycles, which reopened the question posed earlier in the century on the incorporation of business cycles into economic theory (Kuznets, 1931), has been critiqued on methodological grounds. It was shown that the modern exogenous theories take the Frischian impulse and propagation mechanisms dichotomy and reduces it further into an impulse-only theory of the business cycle. The Frischian view of impulses as reinforcement mechanisms (Frisch, 1933) that prevent the cycle from dying down is replaced by a system that generates cycles upon impulse only. We have shown that this result depends crucially on the mathematical technique chosen. In section 1.3, an alternative modeling strategy was proposed that calls for the inclusion of time delays and nonlinearities into the models of the business cycle. Moreover, these time delays and nonlinearities were shown to depend on the specificity of capitalist production and relations. It is ironic that the Lucas and RBC business cycle models have realized the predictions of Simon Kuznets, formulated sixty years ago, who predicted that there were two ways to incorporate business cycles into equilibrium theory: (1) as "deviation from a preconceived picture of reality" (p. 399); and (2) as "a consequence of cycles in *outside* factors and that this variation in what might be called the economic constants does not essentially disturb the determinate fundamental relations between economic factors" (p. 396). It is not so surprising then that Kuznets (1931) paper has not been a major methodological milestone as Frisch's (1933) was in bench marking the new classical counterrevolution.

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