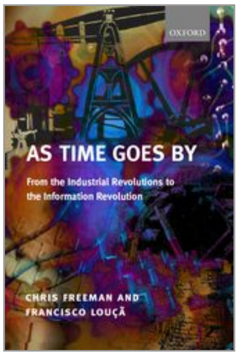

As Time Goes By: From the Industrial Revolutions to the Information Revolution
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The Strange Attraction of Tides and Waves

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Abstract and Keywords

A number of recent authors challenged Kondratiev's tentative conclusions, using modern statistical and econometric techniques, or indicating alternative processes of causation—the case of W. Rostow, arguing for the long-term evolution of relative prices, being one of the best known.

In an overview of the field, this chapter considers the available statistical and econometric techniques for the analysis of fluctuations, their theoretical underpinnings and the implicit structure they postulate: trend deviation techniques and other smoothing procedures, spectral analysis and other theory-free devices, and simulation models are considered and criticized.

The chapter concludes that the apparent lack of statistical evidence for these long periods of characteristic change is frequently imposed by the very methods used to demonstrate their non-existence, rather than by sensitive historical analysis of firms, markets, social institutions and their evolution.

Other authors who concentrated on the schedule of the rate of profit and discussed the social environment and processes of change that influence the lasting effects of clusters of radical innovations and the creation of new industries are reviewed.

Ernest Mandel, the forerunner of the revival of the Kondratiev hypothesis in the sixties, argued for a comprehensive approach to the phenomenon based on the dynamics of economic change and social conflict.

Keywords: causation, fluctuation, innovation, Nikolai Kondratiev, Long Wave, rate of profit, smoothing, statistical techniques, trends

4.1 A Broken Consensus

As we have seen, in the first part of the twentieth century there was widespread acceptance in the economics profession of the cyclical nature of capitalist development, including long periods of expansion and contraction. There were sound reasons for this periodization: the evidence from price statistics was strong, and the existence of a previous long period of expansion under relatively stable political and institutional conditions (British domination and the gold standard) was followed by the violent changes created by the technological revolution of electricity and steel. These changes had been witnessed by some of the early researchers and were still matters of recent memory. As a consequence, the evidence of each period of restructuring of social relations, of institutional settings, and of international relations was naturally theorized as a new regime of accumulation, in a longer perspective than that of the Juglar cycles.

These reasons were still valid for the following generations of researchers: the trough in the 1880s and early 1890s, known at that time as the ‘Great Depression’, was followed by a stormy period of expansion marked by the culture of the ‘Belle Epoque’ and then by the First World War and another Great Crisis in 1929–33; the thirty ‘golden years’ after the Second World War once more nourished confidence in long-term growth, and again the institutional arrangements were stable (US domination and the Bretton Woods order). After the international crisis of 1974–5, the cyclical depressions became deeper, and we are obviously now experiencing a technological and economic transition. In spite of that, much economic theory and most of the statistical methods failed to reflect these larger patterns, as identified by history.

The present chapter briefly surveys the research on the dynamics of modern capitalism following Kondratiev and Schumpeter. Although the problem of long-period fluctuations continued to interest many historians and economists, it is clear that the early consensus on the importance of these longer-term changes no longer held. The reason for this was partially given in the previous chapter. After the econometric revolution and the victory of the mainstream synthesis in eliminating the major alternatives to neoclassical economics, and in the framework of the postwar years of a seemingly indefinite exponential expansion, any claim about a long cyclical pattern of intense structural changes appeared to be theoretically unjustifiable and practically **(p.94)** nonsensical. New statistical and econometric studies appeared to disprove the previous existence of systematic long-term fluctuations in aggregate GDP. The long waves research programme was quickly considered obsolete and was abandoned; the dominance of mechanistic theories and models erased any concern about these mysteries of evolution.

The revival of the programme occurred only as a consequence of individual work in the framework of heterodox paradigms: Mandel (1964) was the first to reassess the question, followed by Hobsbawm (1968), both rightly announcing the coming end of the long expansion, and then by Boccara in the early 1970s, by Mensch (1975), Freeman (1977), and Duijn (1983), as well as by Forrester and his collaborators. In the 1980s, after the major economic turn of the previous decade, the research gathered momentum with major contributions by Pérez, Kleinknecht, Shaikh, Gordon, and Reijnders, among others, as well as with some new statistical (Maddison) and historical (Braudel, Wallerstein) work. The debate included the Social Structures of Accumulation (SSA) school, the Regulationist schools, some neo-Schumpeterians, some Marxists. In the 1990s, Tylecote, Bosserelle, and Fayolle among others contributed with major new developments.

This research on historical mutation in the economies has been criticized mainly from three perspectives. The first is the orthodox view, which ignores structural change and views the economies as cumulative and simple processes, driven by unexplained technological change and under artificial conditions (maximizing rationality, free competition, full availability of perfect information, etc.). In this framework, realism and the definition of distinctive periods of history are considered to be irrelevant or illogical. Of course, the long waves research programme strongly challenges these critiques, since it establishes an alternative evolutionary approach in which the abstraction of the representative agent is completely ignored, where diversity and the creation of novelty are considered the main factors in economic change, and where time matters and morpho-genetic processes are identified to describe the real economies.

The second view is that of some critics who study the *historical* processes of change but doubt the adequacy of, or simply reject, the available long wave models. Kuznets was, of course, the best known, since he was the first to criticize Schumpeter's concept of cycles driven by clusters of investment in major innovations. Kuznets's, or Lange's, or Rosenberg's criticisms are in fact addressed mainly to the general methodological conditions for the scientific demonstration of long waves rather than to some specific model, although they base their remarks on the Schumpeterian and some neo-Schumpeterian models and, as it will be argued, their main arguments only stand up against the most obvious weaknesses of such models.

Some researchers, such as Maddison or authors of the SSA or Regulation schools, take a third view. These authors recognize that there were different historical periods—and in fact concentrate most of their research on the study of the precise definition, the nature of the changes, and the evolution **(p.95)** of those sequences—but suspect adequacy of concepts like cycles or waves. However, given that they do not share the ancient and trivial account of history as a simple sequence of successive events, their debate about the rationale of social evolution is indeed close to our own research programme, since some of them explicitly wrote in such a context (Gordon and the initial SSA work) and others discuss the long-term fluctuations of capitalist economies, classified as ‘stages’ (Regulation schools) or ‘phases’ (Maddison). Boyer, among other leading French economists, contributed a great deal to the theory, with the concepts of regimes of accumulation and regulation, which include the institutional framework in economic research. If the new consensus is to emerge—and this book is a plea for it—it must synthesize various contributions on the historical evolution of the economies. Co-evolution, mutation and structural crises, and selective and adaptive behaviour should be the main conceptual references for that brand of economics brought back to life by evolutionary economics, in its constituent pluralism. In this perspective, a clarification of the past and present misunderstandings is needed.

In the sections that follow, three distinct groups of models, of proof, and of theorization will be briefly considered: (1) traditional statistical methods, (2) historical narratives, and (3) pure simulation from formal models. Although they are not completely alternative, these methods suggest different approaches to the analysis of long-term fluctuations and change in economic series (Table 4.1).

Long wave analysis began in the last decade of the nineteenth century and the first few decades of the twentieth century as an interpretation of crucial changes in the capitalist mode of production (van Gelderen, Parvus, Kondratiev) and as a tool for the integration of the socio-political determinations in long-term economic analysis (Bresciani-Turroni, Pietri-Tonelli, Pareto). In both cases, the research programme was defined as historical by nature. Nevertheless, the first methods to be used by these founding authors were limited either to a description of some indices or to the uncritical utilization of inappropriate tools taken from mainstream economics, itself under the influence of the analogy of the mechanics and thermodynamics of conservative systems. The obvious inadequacy of these procedures to demonstrate this scheme of historical succession of periods with distinctive social, political, and economic features consequently created strong doubts about the justification for the research itself.

The dominant methods in this analysis of long-term fluctuations can be traced back to the time of Kondratiev. By the time he began his systematic inquiry, the two current demonstrative methods were the historical and narrative account of oscillations and the early econometric techniques, and he used both. In its initial version, historical description is the method that includes the visual inspection of time series of data of aggregate production, consumption, prices, and trade, as well as the analysis of political events and social processes (including major ruptures such as wars and revolutions), **(p. 96)** of the role and evolution of institutions, and of the history and geography of the expansion of the world system. This method was important for the first Marxian authors, but was then relegated to oblivion by the dominance of econometric techniques until its resurrection in the 1970s, 1980s, and 1990s by Tylecote, by Perez, by new Marxist approaches, and by some of the Regulationist and neo-Schumpeterian work.

Unlike these descriptive and conceptual methods, the statistical methods originated some of the most intense early disputes about long waves. In the statistical and econometric methods, three main groups of techniques can be distinguished. First, there is the moving-average smoothing techniques and the trend-deviation computation (typically by Kondratiev, Oparin, Kuznets, Imbert, Zwan, Duijn, Nakicenovic, Sipos, Menshikov, Chizov, Craig and Watt, Glismann, Taylor, etc.). In this approach, if the average growth rate over a long period was computed as being above or below the long-term trend, this was taken as evidence for long wave fluctuations. The second group of techniques relates to the growth rate transformation and the analysis of long fluctuations from the behaviour of shorter ones (Mandel, Gordon, Kleinknecht, Duijn, Dupriez, Hartman, Ewijk, etc.), and the third, to spectral analysis (Ewijk, Metz, Kuczynski, Reijnders, etc.). Although the utility of each of these techniques is disputed, they were widely used, either as a complementary device in relation to the historical method or else, and essentially, as an attempt to demonstrate the existence of a cyclical structure by analogy with the accepted business cycle models, so that the inquiry might be legitimized by the comparison.

The general failure of such methods to provide absolutely conclusive evidence stimulated some researchers to look for new alternatives. The third set of methods to be considered in this chapter was born in the 1970s under the influence of Jay Forrester, from MIT. The use of pure simulation, unlike the previous two families of methods, does not present any direct claim for the interpretation of time series, given that it is simply based on a mathematical model with no direct empirical claim and, unlike traditional econometrics, its parameters are tuned according to the requirement of the demonstration but ignoring the available evidence on the real fluctuations. The model is considered to be explanatory simply if the simulation is able to mimic some of the features of the aggregate economic series. The closeness of the resemblance is therefore the desired proof for the causal implications represented by the abstract model.

These three large families of methods are summarized in Table 4.1. The listed authors are relevant examples and the list is not exhaustive; nor is it strictly chronological, since in some cases the theoretical vicinity is privileged. As the criterion for classification is the main method used by each author, the table simplifies some cases of use of a multiplicity of methodologies. Finally, not all the authors here considered identify themselves as part of the programme: this is certainly not the case of Kuznets, or of Maddison and the Regulationists. But it is legitimate to consider their work in this **(p.97)**

Table 4.1. The Main Constellations of Methodologies in the Research Programme

Model analysis	Statistical and econometric analysis		Historical analysis
	Kondratiev		Trotsky
	Oparin		
	Kuznets		
	Imbert		
	Dupriez		
	Duijn	Mandel	Maddison
Forrester	Kleinknecht	SSA	
Sterman	Menshikov	Gordon	Regulation schools
Mosekilde	Hartman	Aglietta	
	Metz	Boyer	Freeman
Mensch	Reijnders		Pérez
	Ewijk	Reati	Tylecote
	Zwan	Kuczynski	Fayolle
Silverberg		Shaikh	Bosserelle
		Entov	

Model analysis	Statistical and econometric analysis		Historical analysis
		Poletayev	
		Moseley	
	(others: Sipos, Chizov, Craig/Watt, Glismann, Taylor, Nakicenovic, Marchetti)		(others: Braudel, Wallerstein, Modelski)

context, since they are important parts of the debate and/or contributed in some significant way to the research.

Some intermediary cases are indicated between the three main and well identified constellations of methods, whenever it is impossible simply to classify the author according to a single criterion. The authors suggesting a specific theoretical problem are included in Table 4.1 in the shaded box, in a transition area between statistical analysis and pure historical description. They include Mandel, Gordon, Shaikh, Boyer, and others, who used some of the statistical and econometric methodology but were concerned mainly with the evolution of the rate of profit and therefore with concrete historical analysis. In the following pages these various methods and problems will be described and briefly discussed.

It will be argued that some of the dividing lines in past controversies are in fact avoidable consequences of the use and abuse of the wrong and mechanistic statistical methods that pervaded the research, under the influence of mainstream concepts. In particular, the methods defined by analogy to those applied to traditional business cycle and growth analysis, i.e. the standard decomposition procedures as applied to ‘trends’ and ‘cycles’, are criticized **(p.98)** and rejected in the sections that follow. Sections 4.2–4.6 contain some unavoidable technical detail in the discussion of the statistical problems. Readers who do not wish to follow this in depth may proceed to Section 4.7; that section sums up the conclusions of the technical discussion on methods in Sections 4.2–4.6 and is followed by proposals for a new type of research agenda in Sections 4.8 and 4.9.

4.2 Trend Deviation and Smoothing Techniques

The problems of decomposition of trend and cycle that confronted Kondratiev have already been discussed in Chapter 3. Many authors used the same type of detrending and smoothing procedure as Persons and Kondratiev.[1] Kuznets, in a book written during the same years as the main work by Kondratiev (1925–7), used the term 'trend-deviation analysis' to account for the distinction between 'primary' (trend) and 'secondary' movements (around 22 years) in the secular evolution of the economies (Kuznets 1930: 325). A moving-averages filter eliminated the primary movements, and logistic or other curves were fitted in some minor cases. (Kuznets did not use extensively the least-squares procedure, 1930: 61.) Not surprisingly, in a final note to the volume, he acknowledged that Kondratiev used what he called the 'modern methods of statistical analysis' (p. 263).

During the first revival of long wave research, after the Second World War, Imbert followed similar procedures: the trend was eliminated with logistic, parabola, or hyperbola curves, and Imbert fully endorsed Kondratiev's methodology, which was only criticized for not using logarithmic transformations (Imbert 1959: 20, 92 f.). This procedure was frequently followed, although, accepting the sensitivity of the results to the choice of the detrending function, van Duijn suggested a log-linear trend transformation of the index of industrial production, and tested the comparison between growth rates of successive Juglars (Duijn 1980: 224 ff.). Menshikov and Klimenko were also aware of the effects of the trend-deviation procedure, but they still used an exponential trend, and then a trigonometric regression, in order to eliminate fluctuations in the residuals if these were larger than fifty years and, finally, a nine-year moving-average filter on the residuals, although they argue that the use of a moving average is legitimate only when the fluctuations are systematic and quite deterministic (Menshikov and Klimenko 1985: 76, 77).

(p.99) The three essential methodological problems were not solved by any of these procedures: detrending supposes an economic distinctive reality for the trend and consequently implies its strict separability from the cycle; the choice of a specific curve for detrending is equivalent to the previous definition of a certain hypothesis about the economic behaviour of the long-term series; the smoothing techniques may create artefacts. No alternative was found to these problems, either in the domain of detrending or in that of linear filtering techniques.

Yet these methods have been repeatedly used, and uncritically abused. There are two substantial reasons for this and for recourse to the current detrending procedures. The first one is meta-theoretical: the trend is supposed to embody the equilibrium conditions and those should be identifiable; orthodox economics consequently favoured this assumption. But the second one is technical: the simpler available statistical methods require the data to be stationary, that is time-independent on average and on variance, and detrending is the simplest way to get a series of stationary residuals.

One of these stationarity transformations that has a somewhat intuitive theoretical justification is the computation of growth rates: it corresponds to common measures, to reference points in the usual analysis of economic conjunctures, and to standard descriptions of time series. Moreover, it is interpretable as an economic entity by itself: growth rates information configures the expectations and contributes to government's, firms', and individuals' decisions. Several authors, some of them well aware of the difficulties of the detrending procedure, used the growth rate transformation in order to proceed to different statistical tests. Yet, the transformation acts as a ‘high pass filter’, biasing the spectral properties towards higher frequencies or shorter cycles (Reijnders 1990: 219 f.; Fayolle 1994: 136), for instance biasing the results in the sense of indicating the presence of Kuznets cycles wherever Kondratiev waves eventually existed (Bieshaar and Kleinknecht 1986: 190–1).

In spite of this, the growth rate transformation has been widely used in statistics: Burns used ten-year periods and computed their growth rates at five-year intervals, in order to define the trend, and Abramovitz evaluated the annual growth rate from peak to peak and from trough to trough of predefined cycles, analysing the trend from these oscillations.

In the analysis of long waves, the methods led to contradictory results. Ewijk followed Imbert's criterion of comparison of growth rates of different Juglars, and did not find general evidence in favour of the long wave hypothesis: a longer secular cycle is detected in Great Britain (expanding from 1780 to 1860 and contracting from 1860 until 1920), but some evidence for Kondratiev waves is found in French industrial production and in German data (investment and imports) for 1830–1913; no such trace appears in US data (Ewijk 1981: 340, 355 f.). Ewijk acknowledges one of the crucial criticisms of this method, namely its dependence on the definition of the turning points, and suggests the computation of a 'normal' growth rate for the whole period (p. 347). Van Duijn follows the same approach, defining a **(p.100)** statistical test whose null is the hypothesized relationship between the growth rates of successive Juglars; but, contrary to Ewijk, he accepts the existence of long waves (Duijn 1980: 226; 1983: 149 f.).

Bieshaar and Kleinknecht defined a one-sided *t*-test for differences in the slopes of log-linear trend curves between predefined turning points (Bieshaar and Kleinknecht 1984: 282). Kleinknecht tested the differences of growth rates in the upswing and downswing of the long-term movements (Kleinknecht 1987*b*: 15 f.), and obtained some evidence for long waves after 1890, except for Britain, where a very long 'hegemonic life cycle' is defined (Kleinknecht 1987*a*: 219 f.).

Since the method introduces obvious bias, all these results are subject to caution. The growth rate transformation suffers from the same problems as the general methods to obtain stationarity, but has a somewhat better control system, since it may use preliminary historical knowledge of the turning points in the business cycle and the pre-definition of the periods—that is an advantage and not a limitation of this procedure, given that it openly presents the historical hypotheses followed by the modeller. In other words, the assumptions of the model are arguable and the choice of turning points may determine the conclusion of the test, but at least they are clearly stated and controllable, unlike the hidden but theoretically relevant assumptions of the choices of other techniques of curve fitting.

4.3 The Enigma of the Sphinx: The Decomposition Problem Reassessed

The previous section discussed some of the problems generated by the use of standard statistical techniques. Here we continue to pursue this question, considering the impact of such an analysis of historical periods on capitalist evolution. In traditional statistical analysis, if decomposition is accepted, then the identification and theoretical characterization of the various elements becomes crucial. One of the simplest ways to proceed is to assume an additive (or log-additive) relation between trend, cycle, seasonal variation, and random shocks. This simplifies the issue to the extreme, since any other type of interdependence is excluded. Each of the elements can then be described in a specific causal context; they can be separately analysed; and it is accepted that the series is the result of their summation. This atomistic hypothesis is coherent with mainstream statistics, and, since stationarity is needed to proceed to the typical tests, researchers are pressed to abandon their objections and to use the decomposition in order to achieve some publishable results quickly enough. But this implies that time and history are consequently put into parentheses, and the inquiry is centred on a new statistical entity, deprived of historical tendencies but hopefully gifted with equilibrium properties. Keynes already had warned of these dangers.

In spite of the investment of treasuries of imagination and of skilful technique in this paradigm, the adoption by so many of the long wave researchers **(p.101)** of this perspective was highly damaging, given that the programme was directed to study long-term evolution of the economies and could not therefore dispense with history as an inseparable part of the object to be studied.

In this framework, four types of solution to the problem of decomposition have been put forward: (1) some of the researchers fully endorse the decomposition procedure and use it without any restriction; (2) some indicate that utilization is ‘purely artificial’ and merely argumentative; (3) others oppose the standard methods but try to find alternative procedures of decomposition; and (4) finally, some reject any solution of this kind. These alternative views will be briefly described.

The first group consists of some researchers who are apparently unaware of the difficulties and biases of decomposition procedures, namely its critical issues: the theoretical assumptions implicit in the choice of function to represent the trend; acceptance of the structural stability of that representation; and the postulated additive (or log-additive) relationship between trend and cycle, that is, a strong but unrealistic assumption that there is no interdependence between the trend and the cycle. A particular form of the decomposition procedure, widely used in past works on the topic, is the 'binary split' method, which uses price series to fix the dating scheme and then proceeds to tests on volume series in order to check the explanatory power of those periods. But this technique requires the price and volume series to be closely cointegrated, otherwise the tests are irrelevant—either the proof is already implicit, or no further clue is provided by the experiment.

The second stance was favoured by several researchers who discuss the pitfalls of decomposition procedures, but nevertheless use them for illustration or for some 'purely artificial' purpose, as Garvy put it (Garvy 1943: 210).

The third group of researchers criticized the decomposition methods, tried to find some alternative, but still used them in some very particular ways. Unlike the previous group of scientists, who were also aware of and commented on some of the pitfalls of the method, this category of researchers generally tried to avoid the procedure. That was the case of Kuznets, who in his 1930 major work asserted that no detrending procedure was atheoretical and technically objective as claimed, and none could distinguish between alleged independent processes:

> Secular movements are continuous, irreversible changes that underlie the cyclical fluctuations of a time series. . . . The precision of our knowledge of these movements depends to a large extent upon the definiteness of our conception of cyclical variations. Nor is this dependence [of secular movements and business cycles] dissolved by any refined mathematical methods of curve fitting or smoothing. (Kuznets 1930: 60)

Kuznets's alternative was to incorporate some clear theoretical assumptions into the choice of the detrending curve: 'And if a hypothesis is to be verified, the secular movements should be described by a curve that incorporates the assumptions to be tested' (1930: 61). Of course, he assumed that there are always elements of arbitrariness in this choice of the function, and **(p.102)** that 'we must bear in mind the essential uncertainty of the whole process of separation, or we shall be unduly influenced by the mechanical methods of fitting' (pp. 62, 67). And finally: 'They [the fitted curves] are not the only mathematical expressions possible, but tentative equations to be used in attempting a uniform, analytic description of secular movements' (p. 68).

Least-square methods were presented as the paradigm of those 'mechanical methods' to be avoided, and researchers were advised to look elsewhere for new methods. But, as no concrete alternatives were available, the main recommendation was still to consider carefully the theoretical assumptions and to accept that the results of 'mechanical' fitting were not conclusive tests for those hypotheses. In fact, Kuznets used logistic or Gompertz curves to detrend his series, and furthermore used moving averages to compute his 'secondary movements'. Like Kondratiev, he used different functions according to the structure of each series. Yet his criticism influenced younger researchers.[2]

The fourth group of researchers includes some more radical opponents of any decomposition procedure. These include early opinions such as that of Dupriez, who stated that long waves cannot be meaningfully represented as trend deviations, since arbitrariness in the choice of the trend curve could not be avoided (Dupriez 1959: 243), or Fellner, who argued that the long fluctuations were merely intermediate trends of more or less steepness (Fellner 1956: 49), or later views such as that of Silverberg, who rightly points out that the assumption about nonlinearity abolishes any possibility of decomposition, since both cycle and trend are generated by the same constellation of processes, or interact in crucial forms (Silverberg 1985: 274).

A recent debate in the *Journal of Monetary Economics* (41: 1998) discussed some of these questions. Fabio Canova compared different methods of detrending and concluded rather sceptically that they were unable to discriminate robust stylized facts, independent of the theoretical definition of the cycle. Although Craig Burnside answered that the complex form of historical series is imposed by the superimposition of frequencies of distinct cycles, but that we can establish adequate filters and analyse this process in the frequency domain, the debate is open. Anyway, the definition of a broad **(p.103)** range of frequencies corresponding to each theoretical cycle does not satisfactorily explain historical mutation and therefore maintains the difficulty of the decomposition, since it indeed tries to address a moving target.

The decomposition enigma can be solved only if three interrelated problems are satisfactorily addressed. The first is the assumption of structural stability of the trend, which reintroduces the concept of equilibrium, and the series is supposed to be generated by the same function with the same parameters over a long period of time. Some authors explicitly accept this assumption, although most of them only consider it implicitly. However, whatever technical sophistication is invested in it, the assumption is not convincing, given that it amounts to the extraordinary claims that the trend is structurally the same for an era, but there are distinct structures for several of the subperiods considered. Moreover, it is obvious that this subvariety is defined according to the particular interests of the researcher, some looking for fifty-year cycles, others for twenty-two-year cycles, and still others merely for ten-, seven-, or four-year cycles, those periods defining the alleged changes of substructures. This leads to a paradox: the scheme is incoherent, since the equilibrating structure of the decomposition between cycles and shocks is part of the disturbances of the previous decomposition between trend and cycle—in this case, equilibrium must have both the face of Mr Hyde and that of Dr Jekyll.

The second problem is the definition of a criterion for the choice of the function representing the trend. This was discussed by most of the authors mentioned above, and no shared conclusion was reached. But if there is no criterion to discipline the research, then there is no room for a demonstrative logic and proof is reduced to a tautology: the researcher is merely supposed to verify what they already know, since the evidence was created by their own convenient method of verification. If this is the case, any hope of solving the first problem must be abandoned.

The third problem is the assumption of the strict independence of the trend and the cycle and of the connected assumption about the linearity of the generating system, so that the series can be defined as a summation of both processes. Some of the authors also discussed this problem, but most of them accepted that the assumed independence was merely a stratagem in order to make the statistical work possible.

In other words, the answers to all three questions indicate that the decomposition problem cannot be solved within this framework. The decomposition problem is in fact tailored by the availability of methods: as an econometric analysis is considered to be the authoritative element for the demonstration of a theory, and as stationarity is currently required for that analysis, detrending becomes a strategic issue. But, given that all its assumptions—equilibrium, linearity, decomposability, independence of the components—are unacceptable in historical analysis, the conclusion that follows is that their lasting influence in the discipline simply proves that the right solutions were found for the wrong problems.

(p.104) 4.4 Simulation Models

All the methods discussed so far have an implicit method of proof, which is based on the analysis of real time series: the statistical test and its ability to confirm the hypotheses to establish the legitimacy of the demonstration. But the deficiencies of the decomposition methods and the danger of inconclusive or spurious results led some researchers to look for alternatives. Forrester and his collaborators, in particular, rejected the econometric procedures and supported a different system of proof, considering that, if a model can generate data comparable to the historical series, that is a relevant test and provides sufficient proof. This method is designated a ‘structural change approach’ as an alternative to the ‘episodic events approach’ and the ‘coherent wave approach’ (Mosekilde *et al.* 1987: 257–8), and it is supposed to make possible historical analysis, unlike the traditional statistical method, which cannot be applied to long wave analysis (Forrester *et al.* 1985*a*: 224). On the other hand, these authors argue that, since some a priori theory is needed to develop any empirical research, and no statistical test proves a causal assertion, a theory encapsulated in a model, based on acceptable behavioural assumptions, and generating data similar to reality is necessary for overcoming the limitations of the inconclusive econometric analysis (p. 236).

The National Model established by Forrester and his collaborators represented a major effort in that direction, and had a large impact in the early 1980s following the discussion about the ‘Limits to Growth’. It generates three types of cycle from various causes: (1) business cycles are created by inventory and employment interactions; (2) Kuznets cycles are created by the evolution of capital and labour; and (3) Kondratiev cycles are created by five distinct mechanisms: (*a*) self-ordering of capital, that is, the positive feedback from the fact that capital is an input for itself; (*b*) the evolution of real interest rates and prices, another positive feedback loop; (*c*) the inflation spiral; (*d*) the introduction of innovations, namely a fifty-year cycle in basic innovation and the bunching process induced by the model; and (*e*) the effect of political and social values (Forrester *et al.* 1985*b*: 204 f.).

As far as the shorter cycles are concerned, they require the presence of exogenous random impulses; in contrast, the long wave is a ‘self-generating process’ in the model (Forrester *et al.* 1985*b*: 209; Sterman 1987: 132). The main factors are the positive feedback loops, particularly the capital self-ordering ones, although capital and labour interactions or real interest rate dynamics are also considered (Mosekilde *et al.* 1992: 198); in that case, technology is a secondary and even dispensable factor for the longer oscillations: ‘The National Model has demonstrated that the long wave can arise even when the level of technology is constant and the rate of innovation is zero’ (Forrester *et al.* 1985*b*: 208). One of the stated causes for the long waves process, technological evolution, and change, is exogenous to the model, unlike the main cause (capital self-ordering) and like the evolution of population, **(p.105)** government activity, and the small random shocks (Sterman 1987: 130 f.; Mosekilde *et al.* 1992: 192). This creates a particular contradiction between the functioning of the model and its ability to describe reality—and, according to the selected criterion of the proof, this is a major problem—since it is accepted that each long wave is really related to a specific technological pattern: ‘Although innovation is not necessary to explain the long wave, there is little doubt that each long wave is built around a particular ensemble of basic technologies. . . . These ensembles evolve synergistically and, like species in an eco-system, compete against other candidates for a limited number of available niches’ (Sterman 1987: 152).

Now, this points to one severe difficulty in the logic of the demonstration, since the status of the difference between the model and reality itself is not clear: the authors first define the model (which dispenses with innovation) but then acknowledge that in real economic processes the long wave is generated by the impact of systemic innovations and its ‘echo’ effect. In fact, if Sterman and his colleagues are correct about the importance of technological innovation, then the model is only able to generate a simulation whose proximity to real time series is more apparent than real, given that each ‘particular ensemble of basic technologies’ is ignored in the model. The example shows that this proof by similitude is not a satisfactory criterion and may be as arbitrary as the econometric procedures it is intended to reject.

Later, Forrester's collaborators provided a new generation of models. They pointed out that socio-economic systems must be described with positive feedback reactions (Mosekilde *et al.* 1992: 212), which imply auto-catalytic (Sterman 1988: 395, Rasmussen *et al.* 1989: 281) and mode- or frequency-locking processes, that is, the coherent entrainment of distinct oscillations (Sterman 1988; Mosekilde and Sterman n.d.: 1, 8), generated by the coupling of different modes of periodic behaviour, a quite general phenomenon in nonlinear systems. These important insights were obtained by persistent work on models; they do not prove that reality is identical to the model simulations, but nevertheless suggest important new conjectures. In other words, this work confirmed the role and importance of metaphorical innovation in science, although it does not make possible new direct evidence about the existence of long waves. The creation of conjectures, although decisive for the development of new knowledge, is nothing but the first step in science.

But there is also a second order of reasons to impose some discipline on the use of purely abstract simulation from models in the research about long processes in economic history, since models themselves have a history of their own. In fact, in economics much more than conjectures have been demanded of the models prevailing in our science, and this is why an uncritical use of simulation is not welcome. Furthermore, the recent generation of the dominant models in economics is by itself a tentative solution to the paradoxical failure of the macroeconometric models supporting the neoclassical synthesis of the 1950s and 1960s, as the current discussion on their value is highlighting. Indeed, given the difficulty of structural estimation, the new **(p.106)** Real Business Cycle models presented an alternative that questioned the cognitive value of statistical inference. Consequently, Lucas championed a sceptical critique of the previous experience and suggested the use of ‘toy-models’, based on artificial economies, whose behaviour should mimic the real series. In that sense, calibration replaced estimation, and the neo-classical school implicitly recognized the intrinsic limits of its investigation: instead of producing assertions on the real economies, an economic theory should just map the construction of a metaphorical entity, a purely ‘mechanical imitation’ (Lucas 1980: 697).

This absolute confidence in the heuristic value of the mechanical life of the artefact emphasizes how wise Ragnar Frisch was when he attacked, in his speech at the ceremony of the award of his Nobel Prize—the first to be given for economics, shared with Jan Tinbergen—the danger and practice of ‘playo-metrics’. And this is also a good reason to refine the methods of historical inquiry in economics.

4.5 Endogeneity: The Dispute Over the Adequacy of Models

Yet, none of these methods means the end of trouble. A complementary question concerns the obvious contradiction between a century-old proclamation in favour of unified societal theories and the profound difficulty of economics in considering concrete history. This section develops the argument that long wave theories are useful to address the critical issue of a general economic vision of historical processes, and that the ignorance of these insights results from the pressure originated by the reverence for neoclassical standards.

Kuznets's review of Schumpeter's *Business Cycles* took up the issue in 1940. According to Kuznets, there are two conditions necessary to establish the credibility of the research programme: it must prove (1) that the oscillations are general, and (2) that there are external factors or internal peculiarities of the economic system that create the recurrence (Kuznets 1940: 267). Otherwise no satisfactory proof exists, and in that regard Schumpeter provided a theory that was not even ‘tolerably valid’ (p. 269).

Now, recurrence is conceptually a very imprecise requirement. In a literal sense, it simply implies that upswings must follow downswings and, conversely, that there is an always repeated order of phases of the cycle, without any necessary constraint about the timing or nature of the causality. In other words, the cycle is the consequence of the previous sequence—this may be called the *weak version* of the recurrence requirement, which we endorse and exemplify in Part II, especially in the Conclusions to that Part, where we argue that this type of recurrence is to be theoretically explained by a defined causal system. A stronger version, the one that is in fact implied by Kuznets and most of the above authors, means that recurrence must conform to further definitions: it must be a time variation in certain precise limits and **(p.107)** with well defined and stable causal relations, to be repeated over and over again. In short, the requirement for the *strong* version of the recurrence turns out to be that the analysis must closely follow the analogy with the business cycles traditionally analysed as the summation of irregular impulses generating regular cycles.

In a clear-cut way, Kuznets asked the research on Kondratiev waves to conform to this stronger version: recurrence could be legitimately explained only by either the impulse (exogenously) or the propagation mechanism (endogenously). As no linear model with strong and exogenous shocks can generate such a cyclicity without implying some sort of historical supranatural design, the burden of proof lies in the characteristics of the endogenous mechanism and an assumption about the distribution of the shocks. These requirements were presented as the scientific condition for the demonstration.

Oskar Lange reviewed the same book by Schumpeter in 1941 and, although denying the Kuznets cycles and accepting that the Kondratiev waves were 'better established empirically', doubted that they could be called 'cycles', since they were exogenously driven by 'historical "accidents" due to discoveries in technologies [rather] than regular fluctuations in the rate of innovation connected with fluctuations in the risk of failure' (Lange 1941: 192). Criticizing the Schumpeterian, 'rather mechanic' extension from the business cycle analysis to the longer movements, Lange dismissed the cyclical nature since recurrence could not be proved and the exogenous factors dominated. Nevertheless, the alternative solution—recurrence based on a cyclical explanation of endogenous factors—became the favourite strategy.

These two possible implications—recurrence requiring the causality to reside in the propagation and equilibrating mechanism and dominance of exogenous factors implying the possibility of no recurrence in the strong version—dominated the debate on the question for some decades.

It is well known, after Frisch's model of cycles, that no linear deterministic system can by itself generate sustained fluctuations—without exploding or damping away—except under a very precise and unchangeable choice of parameter values, and such a structural and parametric rigidity is not acceptable. That is why the following step in the development of cycle theory was the incorporation of random shocks. But these introduce a new set of problems: originally defined as residuals, that is as measures of ignorance, and thus as depending on the modelling devices and not being sufficient to explain the origin of the fluctuation, the shocks came to be integrated in the models as a new concept of causality. This type of determination was introduced either because the theory implies a strong influence of some non-economic factor, or simply because a large part of the economic factors cannot be modelled as endogenous variables since they are not known or hypothesized, or finally, just because the model is restricted to a limited and manageable dimension.

Of course, none of these motivations justifies the causal attributions of the shocks. This did not stop some authors from considering a purely exogenous **(p.108)** causality. Such was the case of the pendulum analogy initially used by Frisch to represent Schumpeterian innovations. But the model indicates the nature of the difficulties in obtaining an unequivocal definition of the variables, given that Frisch defined innovation as exogenous, while Schumpeter fiercely resisted and argued that the impulses had an endogenous character (Louçã 2000). As Goodwin put it, his formulation of the fountain metaphor—which is equivalent to Frisch's pendulum—'means accepting the idea that the exogenous (to economics, narrowly formulated) events of social history play an essential role' (Goodwin 1987: 28; also 1985: 12).[3]

It is difficult to define recurrence on the basis of pure exogenous causality. This is why Kleinknecht, among many others, emphatically argued for the alternative strategy and claimed that that credible theory depends on a causal endogenous explanation: 'The question whether or not such fluctuations are true cycles depends on whether theoretical explanations can convincingly demonstrate their endogenous character' (Kleinknecht 1992: 5; also 1987*a*: 222; 1987*b*: 13, 33). So did Poletayev (1992: 166), Ewijk (1981: 325), Metz (1992: 82) and others.[4]

This definition creates great methodological difficulties, since the formalization of the model, by analogy to business cycle analysis, imposes a strict and exhaustive definition of the nature of the variables, which is rather incompatible with an inquiry into real historical processes. In fact, it makes possible exogenous causality, which may prevent or create regularity or even recurrence, requiring some heroic assumptions or, alternatively, some endogenous transformation when the system is submitted to exogenous shocks and propagates them according to an internally defined mechanism. As a consequence, the linear impulse propagation system defines two precise families of extreme causal assertions: either a strong version of complete exogeneity, or a strong version of complete endogeneity (requiring the impulses to be random, small, and economically non-significant, although being decisive for the maintenance of the oscillation). In the latter case, non-correlated random shocks create correlated fluctuations, and simplicity and order emerge from complexity.

Some of the researchers went so far as to identify a completely endogenous explanation as the condition for a demarcation between scientific and **(p.109)** non-scientific propositions. Of course, such completeness is purely rhetorical, since the object cannot be the whole world. One of the most relevant examples was that of Kondratiev, to whom endogeneity meant that all the relevant variables could be defined as having been generated by the economic system itself:

> This just means that these episodic and external causes ['in particular the variations in the extraction of gold'] are themselves included in the whole process of the socioeconomic dynamics and for that reason cannot be considered as external causes accounting for the cycles. From our point of view, the explanation of the long cycles and in particular of those of prices must be looked for in the character of the mechanism and the internal laws of the general process of socio-economic development. (Kondratiev 1928*b*: 425)

Consequently, these very general laws should account simultaneously for the discovery of gold in California and Australia, for the two world wars, for the decline of the British Empire, and for the electricity revolution at the end of the nineteenth century: they should explain the co-variation of all these variables and do so from the standpoint of a non-defined 'general process of socio-economic development'. But the claim is obviously excessive, and in some places Kondratiev indicated that he understood this; while repeating that the generally considered exogenous causes were really consequences of the economic system, and therefore of endogenously determined factors, he still accepted that there was in society 'an element of creation' (Kondratiev 1928*b* : 149, 150–1). He furthermore wrote that the cycles corresponded to an 'internal logic', which was a sequence of cumulative causal effects, but that new historical conditions could precipitate the cyclical fluctuations (p. 164).

Kondratiev's tradition amounts to (1) that only 'endogenous models' are valid, and (2) that all relevant factors of all kinds must therefore be modelled as endogenous variables. As a consequence, the scope of the model is defined in such a way that it must include all social, economic, political, and institutional realities. Paradoxically, most of the authors referred to above suggested models that cannot, of course, correspond to this theoretical requirement of universality, since they are limited to a small number of variables and are by this sole fact forced to ignore most of the relevant factors, which are finally condensed under the form of some exogenous residual random term.

Clearly, the very definition of endogeneity depends on the scope of the operational model that is used. Of course, the problem is also one of terminology, and several authors use the concept in a rather loose way. Endogeneity can be rigorously addressed only in the framework of a formal model, since it is non-definable in the context of a general social theory, in which it refers to any possible ingredient of the explanation. It is obvious that there is a dramatic trade-off between on the one hand simple and formal models, which can be tested against empirical evidence and whose definition of exogenous and endogenous variables is exhaustive, and on the other **(p.110)** hand broader theoretical frameworks, where that definition is not so precise. Whoever opts for formal and testable models must accept their standard limitations, such as a small number of dimensions, generally a linear configuration, and decomposability. Alternatively, the other option implies the acceptance of the notion that the boundaries of the economic sphere are not objectively defined and that it is not possible to endogenize artificially such factors as state intervention, social institutions, or cultural features, which are obviously so relevant for economics since a formal model cannot completely determine these variables. In other words, either the models are testable but do not explain reality, or they explain reality but are not necessarily operational. Consequently, the useful role of simple formal models is not in the precise explanation of socially complex processes, but in the creation of conjectures for their study—it is unwise to ask metaphors to deliver what they do not know.

On the other hand, complete endogeneity refers to an ideal, all-comprehensive model that is unreachable. It should represent the whole universe, and then the distinction between the endogenous and exogenous variables becomes useless since everything is endogenous by definition in that case. But this cosmic vision, useful as it may be, does not provide the foundations for an operational model and is essentially a literary device to bridge over contradictions between general theories and concrete modelling.

Alternatively, one may define capitalism as a concrete historical process, some of whose contradictions may be captured by the concrete economic analysis of production and distribution, while others remain outside the scope of each model. As the economic subsystem is partially and conceptually autonomous from the other social and political spheres, where independent processes may develop, the theory must address both their relative autonomy and their interconnections.

If researchers on long series take this interpretation, then they must consider not only the economic forces, but also the social and institutional environment and the historical events—each wave is one 'historical individual', wrote Schumpeter—in order to formulate a theory.

So it is not just the extreme versions of exogenous and endogenous determination that are at stake: it is the very purpose of the antinomy that is rejected. In fact, the standard distinction between endogenous and exogenous variables implies no less than: (1) the linear and additive representation of the trend and the cycle; (2) the fact that therefore decomposition is possible and meaningful; (3) that the relevant object of the study is the cycle and not the trend; (4) that the cycle is the reversible process to be explained by the endogenous mechanism; and finally (5) that the trend represents equilibrium and that the reversible movement is organized around equilibrium. But this paradigm defines models with no time, with no change, and with equilibrium properties that are the more absurd if extended to a period of history covering nearly two hundred years, and indeed are clearly contradictory with the observable reality of contemporaneous capitalism.

(p.111) Moreover, no theory about history can ignore the evolution of the socioeconomic environment and of historical contingent events; neither can it fully integrate those factors as mere consequences of the economic endeavour. In fact, in the historical perspective a strict endogeneity is either trivial or wrong, and merely constitutes a self-confident *vue d'esprit*. This is why Mandel defined an asymmetric and integrated explanation for the long wave (Mandel 1979: 14–15), just as Reati did (‘Comment’ in Poletayev 1992: 169), or Shaikh (1992: 175), or Pérez (1985: 453).

The final question concerns the nature of the difference between endogenous and exogenous variables. This identification is somewhat arbitrary, even within the dominant paradigm which favours that formalism: investment may be modelled either as ‘autonomous’ (exogenous) or as induced by the evolution of the system (for instance, by the aggregate product), or as both simultaneously, and then causality may not be unequivocally determined. On the other hand, the candid belief that economies can be exhaustively represented by a formal model with a finite and normally very reduced number of variables whose legitimate relations are calculable, which is the rationale for the instrumentalist epistemology, is incompatible with the analysis of real evolutionary processes.[5] That was the sense of Schumpeter's and Keynes's arguments about ‘hybrid variables’.

Given that most research on long-term economic movements stuck to the first generation of mechanistic models, it is understandable that the postulated legitimacy or intelligibility of the theory has been associated with endogeneity. Thus, the acceptance of Kuznets's or Lange's requirements amounted to an open invitation to search for the Grail: a perfectly all-comprehensive endogenous model capable of generating the cycles, or a Laplacean demon knowing everything.[6]

(p.112) The whole quest amounted to a tremendous and self-defeating loss of energies. The Grail cannot be found, simply because it does not exist: a complete endogenous explanation whose scope is the complete universe itself is either a literary device and an aesthetic vindication leading to resignation, or a meaningless methodological criterion, since causality is not self-sufficient except in theology.

Split between these two contradictory requirements—the standard formalism necessary in order to legitimate the inquiry in relation to the discipline as a whole, and the inclusion of the whole universe in the operational model—the modelling procedures in the long wave programme have been generally inconclusive.

4.6 Spectral Analysis as an Example of the Decomposition Procedures

The critique of the traditional econometric methods for cycle analysis led to the defence of non-parametric methods for the statistical inspection of time series, and of historical methods to establish the hypotheses for that inquiry. Spectral analysis, one of the most powerful statistical alternative methods, corresponds to the first requirement, although not necessarily to the second one. Its applications in long-term economic analysis are reviewed in this section.

Spectral analysis, which is based on the decomposition of the total variance of a series into the contributions of individual frequencies, has been commonly presented as a 'theoretically free' device, allegedly independent from any theory about the nature of the trend. It is, but although this quality is certainly adequate for experiences and for laboratory replications, it is not necessarily so for the analysis of social evolution in the long term, given the irreversibility of historical mutations.

The first applications of spectral analysis for the study of long-term fluctuations, before revival of the long wave programme, were naturally concerned with long swings, as suggested by Kuznets. Adelman, in 1965, reconsidered a previous experiment with a randomly shocked Klein–Golderberger model, which could generate long swings and mimic real series quite well. Adelman used a log-linear trend and least squares to stationarize the series and compute the residuals. The conclusion was that the Kuznets swings 'are due in part to the introduction of spurious long cycles by the smoothing process, and in part to the necessity for averaging over a statistically small number of random shocks' (Adelman 1965: 459). In 1968 **(p.113)** Howrey came to the same conclusion: he used a growth rate transformation in order to make possible the spectral analysis, and concluded that no evidence could be found of the presence of long swings in the US 1869–1955 and 1860–1961 series of GNP, population, GNP per capita, industrial production, and others (Howrey 1968: 250).

The applications of spectral analysis to the inquiry into Kondratiev waves came much later: Ewijk claims to be the first to have applied the method to data from the United States, Britain, France, and West Germany. He introduced two different transformations for stationarity—first differences and first differences in logs—and confirmed evidence of long waves in the price series but not in the production series, except for France, after the interpolation for the Second World War years (Ewijk 1982: 478, 486, 489). Other authors followed.[7]

In this framework, three main methodological problems persist in spectral analysis. The first one is common to other econometric procedures: conclusive results require longer series than the available ones. As early as 1965, Adelman commented that the seven Kuznets periods available were not enough for a conclusive test (Adelman 1965: 451), and Duijn justified the need to look for qualitative data, given the insufficiencies of the statistical information for spectral analysis (Duijn 1977: 567). There was general lack of agreement on the number of cycles necessary to test the long wave hypothesis—10 cycles (Ewijk 1981: 336–7), 8–10 cycles (Beenstock 1983: 139), 7–10 cycles (Duijn 1983: 169), 7 cycles (Solomou 1988: 16), and longer series (Bieshaar and Kleinknecht 1984: 281; Glismann 1985*a*: 231). This disagreement is rather general, and not easy to solve, at least in the short term.

The second major problem is the sensitivity to the decomposition procedure, for the trend elimination. Bieshaar and Kleinknecht note the problem (1984: 281), and several authors address the core issue of stationarization: Duijn argues that the process can affect the turning points (Duijn 1983: 169), and is supported by Metz (1987: 392); Gerster notes that the trend-decomposition assumption implies that the series under scrutiny must be free from any evolutionary movement (Gerster 1992: 124). Indeed, the series must be conceived as a realization of a purely random, a stationary Gaussian process (Gerster 1992: 128; Howrey 1968: 229; Adelman 1965: 448).

The third problem is that the results can be affected if the amplitude and period of the cycles change abruptly (Glismann 1985*a*: 231; Chizov 1987: 5; **(p.114)** Taylor 1988: 427), that is, if the assumption about structural stability is challenged. The trend representation is essential and it is essential that it captures the whole process generating the series for a very long period without any possible interruption: this is implicitly assumed by many of the statisticians.

One of the efforts to address these problems was developed by Jan Reijnders, who attempted to incorporate spectral analysis in the framework of long wave research and to avoid the purely mechanistic procedure of trend decomposition. His choice is to make explicit the assumptions about the economic description of the trend: he develops a very sophisticated discussion about previous methods in econometric analysis, and suggests a new technique, considering the trend as the summation of a ‘standard trend’ and ‘systematic deviations’. The standard trend is computed from the average growth rates for a very long period, given that Reijnders points out the danger of the ‘perspectivistic distortion’ whenever the size of the window is not enough to account for complete cycles (Reijnders 1990: 190, 133, 135 f.). The standardization procedure thus adopted—or the elimination of the standard trend—is based on the values of the longest available series, the 690–year long Phelps-Brown and Hopkins index for a set of prices. The spectral analysis on residuals from the trend (conceived as underlying all the series) is the test for the presence of long waves (Reijnders 1990: 218), and Reijnders concludes that there is positive evidence in price series after 1700, and in production series, provided that the war years are interpolated (pp. 227, 230).

In this case some of the major problems with spectral analysis are avoided, namely the purely atheoretical application which is correctly rejected. Reijnders also provides a powerful critique of the dominant techniques: he shows that the growth rate transformation implies predetermined windows and an increased danger of the ‘perspectivistic distortion’ (1992: 25); that is, it hides the longest waves. Finally, he argues that the problems of heteroscedasticity and auto-correlation, which are obviously to be expected in historical series, can be only partially and tentatively solved (1990: 155–6).

But this effort is still neither conclusive nor complete. For the sake of the available data, Reijnders assumes heroically that there is an implicit economic trend influence for the last 690 years in all his British series, which can be represented by the evolution of the prices of a basket of consumables in the southern part of Britain for seven centuries. (The original series is 1264–1954.) He further assumes that this trend, embodied in an increasing monotonic function, an exponential function, can be meaningfully separated from the cyclical influences in all other series (Reijnders 1990: 132). As a consequence of both options, he obtains non-explained results, such as very long cycles of 376 and 242 years, which are not supported by any theory.

(p.115) The difficulty seems to be insurmountable: spectral analysis still assumes the superimposition of frequencies as a specific form of the trend–cycle relation, and consequently is submitted to the general critique of decomposition.

But this is not all. Precisely because they opted for econometric procedures requiring the stationarization of the series in order to test the hypotheses, many of these researchers were confronted with the concrete difficulty of dealing with the most extreme economic situations, such as the war periods with their extraordinary economic turbulence. In general, the application of these statistical methods required the a priori elimination of the impact of those war periods on the mean and variance of the series, in order to make a proper analysis possible.

Without exception, and this is hardly surprising, those engaged in this type of statistical inquiry accepted the necessity for those punitive methods.[8] According to Metz, only a linear interpolation for the war periods may rehabilitate the Kondratiev hypothesis; otherwise the test indicates the sole presence of Kuznets swings in the twentieth century (Metz 1992: 89). Metz goes as far as saying: ‘From a statistical point of view, most of the World War values can be regarded as outliers which disturb the “normal” structure of the time series’ (p. 110, also p. 89).

Kleinknecht strongly supports that view. He gives three reasons for this: (1) war impacts are very severe and thus distort the data; (2) the statistics may otherwise give implausible results (namely, a Kuznets downswing in the

1950s); and (3) there is no theoretical explanation for the Kuznets cycles, which would survive if no elimination of the ‘outliers’ were decided (Kleinknecht 1992: 4). Of course, it is difficult to see the relevance of the first reason for a realist epistemology, and the second and third are merely a restatement of the correction of the theory, in spite of, and in contradistinction to, the statistical results. The paradox is that the fascination for these methods was based on their capacity to challenge a priori interpretations and to reveal new evidence. But in this case, and contrary to what has been proclaimed, the evidence is tailored to the existing theory in a very frank and open way, and no independent test is accepted: if the evidence does not obey the theory, the evidence is changed by the statistician. The epistemology of confirmation never went so far, and never was so little confirmatory.

(p.116) There is no possible causal implication from this kind of test. If a series that excludes the war time periods is tested and confirms the existence of long waves or of long swings, this statement may be interesting, but surely is completely irrelevant to describe or analyse reality, given that history stubbornly but unfortunately did include those hidden wars. Erasing parts of history is not a method to study history: the creation and manipulation of an historical artefact does not allow for any reasonable claim about real evolution, unless one could proclaim the absolute separability of international militarism in relation to the economic movements.

4.7 Problems and Perspectives

Two main families of methods have been reviewed in the last sections: what could seem to be a technical detour from the essential theoretical problems was instrumental in addressing some of the current limits of a research pervaded by *ad hoc* concepts, mechanistic models, and equilibrium theories. A summary of these families of methods is now in order, so that new alternatives can be explored.

The first constellation of methods, the standard statistical and econometric approach, did not solve the decomposition problem. This was to be expected, since the simple transposition of the dominant methods in business cycle analysis, where the assumption of no major economic change holds, to the study of long-term processes of growth and change, where it is obviously inconsistent, ought to fail. Indeed, the metaphor is incoherent, given the implausibility of structural constancy for fifty or one hundred years or longer. It is worth remembering that Keynes's discussion with Tinbergen was motivated by his opposition to correlations established for *ten* years. The presumption of causal stability, the exact same causes acting in exactly the same fashion in such different periods, furthermore added to the requirement of endogeneity, i.e. the same causes explaining the whole time process are not compatible with real history.

Finally, in order to preserve the method, some supplementary assumptions were made, such as the declaration of irrelevance of the inconvenient part of history (wars and major economic crises) which should not be considered in order to make possible the study of the relevant part. But this amounts to a confession of failure, since structural changes in economic history cannot be explained in ignorance of the concrete historic ruptures.

Otherwise, the standard econometric methods could not solve the decomposition enigma, since it is probably not possible to solve it. Some promising avenues were presented, namely non-parametric methods such as spectral analysis, but, given the irreducible irregularity of historical series, the wisest use of those new types of method is to establish conjectures rather than to claim to have the last word in the testing of hypotheses. Indeed, the most **(p.117)** sophisticated statistics require the most developed theoretical and historical explanations.

The second constellation of methods is the model approach. This creates a much larger degree of freedom in the search for causal systems, and may even contribute to overcome another major limitation of the dominant traditional econometric methods, which is the presumption of linearity. In fact, nonlinear systems and models question the traditional definition of endogenous and exogenous variables, differentiate the impact of external perturbations according to the state of the system, produce mode-locking behaviours, model structural instability and dynamic stability in the same context, and interpret complexity. But models alone cannot sustain a demonstrative logic or an adequate interpretation of reality, since simulation is not demonstrative proof. In this sense, models are useful metaphors for the creation of hypotheses in order to analyse reality, but they are not the reality itself, nor can they reproduce it.

A third constellation, the approach based on the complexity of reasoned history, has to be developed in the intersection between the historical, analytical, and descriptive statistical methods for hypothesizing causal relations and the modern and infant methods of nonlinear quantitative and qualitative research. This agenda includes:

1. the rejection of the claim for a complete quantitative description of the universe, as made by classical positivism and empiricism;
2. priority for the ‘reverse problem’ in quantitative analysis, i.e. for identification of features of the real time series, instead of the fabrication and simulation from an abstract model;
3. the acceptance of complex determination and the importance of social, institutional, and political factors, represented by semi-autonomous or ‘hybrid’ variables. Those factors are part of social and economic coordination, that is, of the cohesive processes such as those defining the rules of social conflict, economic determination, and political decisions contributing to the reproduction of the mode of production. They underlie the evolution of institutions and explain the economic behaviour dubbed as ‘equilibrium’, i.e. the

> dynamic local stability of the system, in spite of, and together with, its constant drive to change.

For these reasons, the reasoned history approach denies the extreme assumption about self-contained models and methods, and looks for integrated theories that will be incomplete and not definitive, explanatory and not predictive, historical rather than simply economicist, and evolutionary rather than mechanistic. A long time ago, Kuznets himself argued for such an interdisciplinary approach, although this plea had little effect on the profession:

> If we are to deal adequately with processes of economic growth, processes of long term change in which the very technological, demographic, and social frameworks are also changing—and in ways that decidedly affect the operation of economic forces proper—it is inevitable that we venture into fields beyond . . . economics **(p.118)** proper. . . . It is imperative that we become familiar with findings in those related social disciplines that can help us understand population growth patterns, the nature and forces in technological changes, the factors that determine the characteristics and trends in political institutions . . . (Kuznets 1955: 28)

As Chapters 2 and 3 have tried to show, Kondratiev and Schumpeter also argued for this, even though inconsistently. Our own approach takes up this challenge and ventures into those fields where economics is to be reconstructed as a social science.

4.8 A New Research Agenda

Is the metaphor of the wave the most appropriate to describe the long-term evolution of structures of accumulation, of employment, of production or trade? It is certainly reminiscent of the equilibrium paradigm, since waves and tides alter the level of the water, but there is still a reference situation that would occur if no attraction of the moon, no winds, and no disturbances existed around which the real levels gravitate. Indeed, Walras, argued for the unimportance of crises and the primacy of the static analysis based on the metaphor of the surface of the lake being agitated by transient waves. Consequently, one can ask if this new metaphor does help to master the nature of irregular growth and to create some pertinent conjectures for its discussion.

The metaphor of the wave is deficient—and it will be used here only because it is the established reference—since in real economic series nonstationarity and time dependence do matter, and indeed are the central features to be explained by the theory: the economic variables really do evolve and, with the absolute exceptions of entropic death of societies, they do not stabilize around some imaginary permanent level or constant rate of growth. In other words, persistent processes with long memory typically drive the macroeconomic variables. But still, this metaphor implies that only intermittent equilibrium exists, under the form of two attractors (the maximum and the minimum levels of the tide). Of course, even in these conditions, the analogy is limited—society is not constrained by this type of permanent boundary, given the persistence of material development.

Moreover, evolution is nothing more than the creation of variety and novelty, and *a fortiori* no fixed attractor or strictly unchangeable mechanism can represent that process. Irregular waves do exist, and they cannot be studied under the diktat of the *ceteris paribus* conditions: time is turbulence. The theories of cycles tried to encapsulate these phenomena under deterministic equilibrating representations, and then some exogenous noise was added for the sake of the quality of the simulation; naturally, they failed to produce either logical explanations or coherent descriptions.

Alternatively, we suggest that nonlinear complex models are necessary to address the duality of dynamic stability (around the attractors, in a region **(p.119)** bounded by the availability of material resources, labour, or technological capacities) of systems that are nevertheless structurally unstable (inducing switches of regime from changes in the structure). These evolutionary models must address the central features of real economies: capitalism is unstable and contradictory, but it controls its process of accumulation and reproduction. Even more, critical instability generates new developments and new phases of dynamic stability: this morphogenetic feature is the peculiar strength of capitalism that fascinated Schumpeter and was so vividly described in Marx's and Engels's

Communist Manifesto as the programme of modernization, ‘all that is solid melts into air. . . ’.

In long wave research, some authors addressed these criteria. The SSA and Regulationist schools discussed historical determinations, and so did the historians of the economic stages and several Marxists or neo-Schumpeterians. Some of their models explored nonlinear relations and established new hypotheses, and many authors claimed, and rightly so, that the general equilibrium paradigm was inappropriate.

But the state of the art in this research, as argued before, clearly indicates the difficulties and contradictions that dominated so far, the pervasiveness of inadequate methods, and the dependence on mainstream epistemological requirements for the construction of theories and the definition of models. Neoclassical doctrines were frequently rejected, but the general recourse to linear econometric methods reintroduced the equilibrium concept and imposed drastic restrictions on the historical nature of the series. Positivist standards were often rejected, but many authors accepted the atomistic and deterministic implication of the decomposition of time processes. The implication was self-defeat.

A new agenda implies a return to the organic and evolutionary metaphor; in particular, it requires the incorporation of two concepts: those of morpho-genesis, or the study of structural crises in economic history, and those of co-evolution.

Morphogenesis implies two essential features: change and control, or rupture and continuity. Both coexist and are interdependent and inseparable: the one-sidedness of the analysis of a single term of the social process is indeed responsible for most of the relativist trends in economics, the extreme examples of theories of continuity being those defined by an assumption of perfect rationality and the general equilibrium paradigm.[9]

(p.120) Coordination is the appropriate concept by which to interpret and analyse control systems and cohesive functions in historical development. Coordination, as a social process subjected to complex interactions—and not equilibrium, which is a state—explains the existence of attractors in growth patterns, the weight of social institutions, and the relation between the economic system and other parts of society. It establishes, from that point of view, the condition for the viability of morphogenesis, originally defined at the organismic level but then metaphorized to the general evolutionary process of society. In other words, coordination explains why disequilibrium processes exist but are constrained, why different rhythms are mode-locked, and why structural instability persists but does not drive the system towards explosion.

Turing, celebrated as a forerunner of computer science but frequently ignored as one of the first discoverers of complexity, modelled morphogenesis with a very simple chemical system of two components diffusing at different rates under random shocks: the system attained the ‘onset of instability’, given its auto-catalytic properties. In the case of that model, the emergence of a pattern of organization shows that it is instability that entails the development of the structure (Turing 1952: 37). This is order emerging out of complexity, and we may conjecture that this provides the example of a general case.

Of course, there are major differences between those simple chemical and organic systems and the social ones. The balance of positive and negative feedbacks in ecological niches, the channelization [in other literature, the term used is ‘canalization’] of development, the selection of the spaces of viability and of stability are mainly driven by naturally coordinated processes, while in economies and in societies there is a combination of natural processes and of conscious choice and purposeful action. In this sense, social coordination may be defined as the working of two related sets of variables: (1) the technological, scientific, economic, political, institutional and cultural subsystems, and (2) the semi-autonomous variables connecting those subsystems. So far, an important part of the discussion on long wave movements in economic history was concerned with precisely these distinct sets of variables. Nevertheless, it has been limited through the assumption of strictly independent, separable, and additive subsystems and of a narrow atomistic hypothesis to interpret each of them. A new research agenda requires emancipation from such constraints.

4.9 Conclusions: Evolution and Mutation, The Epitomes for a New Consensus

In his reference to the Kondratiev cycles, Mandelbrot suggested that the superimposition of different orders of cycles should be assessed by hierarchical descriptions (Mandelbrot 1987: 126). Such is the approach of this **(p.121)** book: emergence, complexity, and open systems are central features of the evolutionary theory as applied to social historical processes. In that sense, the basic hypotheses can thus be stated under the following form. (1) The social subsystems (science, technology, economy, politics, culture) generate a large number of irregular fluctuations, namely cyclical and wave-like movements with different approximate periodicities, caused either by specific subsystem cycles (political business cycles, technological trajectories, cultural movements, life-cycles of products or industries, etc.) or by the lags and feedbacks in the inter-subsystem connections. (2) Those streams are combined in some bands of fluctuation by specific coordination processes emerging after structural crises. These coordination processes are therefore the crucial causal determination for the business cycles and the long wave movements in real historical development, and that is the focus of Part II of this book.

In various ways, several authors anticipated these hypotheses. Van Gelderen was the first author in long wave research to have formulated a related interpretation, when he indicated that periodicity was connected not simply to the technological subsystem but also to the working of the capitalist system as a whole, that is to the construction of social order (van Gelderen 1913: 45). Of course, fifty years earlier Marx had anticipated the importance of the coordination process of 'capitalism as a whole', and had explained it as the outcome of profound tendencies and counter-tendencies, i.e. of conflict. For both Marx and van Gelderen, coordination was the rationale of the organic metaphor, and they were followed by modern evolutionary economists.

Some of the main consequences of this approach are no other than the previously stated epistemological conclusions: given that each subsystem is defined as the heuristics for some social relation, their interrelations cannot be deterministically discriminated by an exhaustive account of a simple model or by endogeneity or exogeneity of variables. On the other hand, semi-autonomy therefore means that the variable most relevant to the understanding of historical dynamics is the coordination process itself. In summary, the hierarchical explanation considers the specificity of each of the five subsystems and of their types of determination, as well as their organic totality.

This five-subsystems approach has three central innovative features. (1) It is a description based on the overlapping of subsystems, since their relationship is more adequate to explain reality than the artificially isolated description of each of the subsystems. (2) It analyses the crises and phase transitions from the viewpoint of the lack of synchronicity and maladjustment between subsystems, which defines the time band of major fluctuations. (3) The social conflicts of all types are generated and articulated by the coordination process, that is by power under all its forms, from the production of legitimacy to strict coercion. This coordination process proceeds at several simultaneous levels. The first level is that of the actions **(p.122)** embodied in the social working of the economic system, the tension to integrate the conflicts, the conventions, and the institutions, and the second level is that of power, strategy, and domination.

In this sense, the fact that there is coordination does not imply that there is harmony or equilibrium, either in the ideological sense of a general feature of the capitalist economies or in the precise sense of a permanent dynamic stability property prevailing in the markets—which would require some hyper-centralized architecture of decision. In other words, complexity and structural change can be explained only as historical developments, as co-evolutionary processes. This is a building block for a new consensus: as we have shown in previous chapters, several economists—from heterodox traditions, from the original econometric movement, from the NBER—have been arguing for an effective change of paradigm. The Nobel prizewinner in physics, Philip Andersen, one of the founders of the Santa Fe Institute, put it in this way: ‘Theoretical economics will have virtually abandoned classical equilibrium theory in the next decade; the metaphor in the short term future will be evolution, not equilibrium’ (*Science*, 17 March 1995: 1617).

Notes:

(1) Persons's method of detrending through curve fitting and moving averages became the standard form of decomposition: ‘His [Persons'] methods (or closely related ones) for detrending data, for removing seasonal variations and for smoothing out erratic fluctuations are now such a standard part of data-preparation for econometric work that they are barely given a thought’ (Morgan 1990: 63).

(2) Some years after Kuznets, Frickey strongly criticized Warren Persons's decomposition methods, which had inspired Kondratiev and so many others: ‘we expressly reject the notion of commencing the investigation by application of the conventional procedures for trend–cycle separation—fitting secular trends and computing cyclical deviations therefrom . . . ’ (Frickey 1942: 9). In particular, Frickey discussed the arbitrariness of curve fitting choice. Considering a long series of pig iron production, Frickey fitted 29 different curves for the secular trend, obtaining cycles in the residuals that varied from 2–3 years up to 40–45 years (p. 10). As a consequence, he rejected the mechanistic method and suggested a search for an alternative ‘genetic’ method, based on theory and history and using some statistical devices (e.g. checking the patterns of dispersion of various series, in order to make comparisons: pp. 9–10). Nevertheless, Frickey used the log-linear trend and the inspection of its residuals as a ‘first approximation’ for the detection of long movements (pp. 21, 250, 260 f.); he claimed that his results were consistent with the long wave hypothesis (pp. 231 n., 232 n., 255 n., 340).

(3) In a more general way, Reijnders formally endorses the rocking-horse analogy of the equilibrium apparatus moved by exogenous shocks: ‘Like in the impulse and propagation theory of the “familiar” business cycle, the long run movements are interpreted here as a particular manifestation of the working of a mechanism through which the economy counterbalances and absorbs the shocks to which it is subjected’ (Reijnders 1990: 52).

(4) In the same direction, Escudier defined endogeneity as the scientific condition for the success of the programme (Escudier 1990: 128) and Glismann called for an ‘endogenous theory’ (Glismann 1985*b*: 221), as did Kuczynski (1992: 264), Delbeke (1985: 11), and Duijn (1983: 129). Altvater argued that factors that are exogenous from the strict economic point of view could be considered endogenous to the dynamics of capitalist crisis (Altvater 1983: 13); so did Frank and Fuentes (1992: 1, 5) and Rosier and Dockès (1983: 125, 183), for whom the class struggle is endogenous since generated by capitalism. Recently, Bosserelle argued that recurrence and endogeneity were the conditions for the success of the investigation on long-term fluctuations (Bosserelle 1994: 47).

(5) More recently, these constitutional dogmas of decomposition have been challenged by the abandonment of the assumption of linearity. Some nonlinear dissipative models, such as Goodwin's 1990 model, require a forcing factor to mimic reality. This is not a random shock procedure, since it is economically defined and parameterized. It represents simultaneously endogenous and exogenous causality in a model whose nonlinear functioning generates evolution and change, structure and intrinsic randomness, so that the changes in basic technologies impact on the economy, which acts as a ‘frequency converter’ (Goodwin 1985: 271–2), changing both the dynamics and the structure of the society. As a consequence, the strict separability between endogenous and exogenous variables is dissolved: as far as this model is concerned, the forcing factor is both endogenously determined and influenced by the initial conditions, and then is exogenously reparameterized every half century. And, second, the endogenous variables create oscillations that are deterministically irregular although apparently random (Goodwin 1990).

(6) This quest for the Grail tradition is indeed responsible for the most extravagant characteristics of some work, both in the desired scope for the research (it should include series on criminality, suicides, divorces, school attendance, and strikes, according to Imbert 1959: 151 f.; on the adoption and diffusion of stamps, the book printing industry since 1470, England's war fleet since 1485, US murder rates and even killing techniques, or the building of the first Metro line in major cities, according to Marchetti 1993: 6, 10; 1986: 377, 383; on women's clothing fashion, Arizona's trees growth or advertising strategies, following Beckman, Shearlock, or Lorenz; quoted by Marshall 1987: 3) and in its predictive capacity. (Marchetti predicted, in the same vein as Mensch, that a major innovation wave was to be expected in 1984–2002, (Marchetti 1988: 2); Hall and Preston that the IV Kondratiev would end by 2000 or 2003 (Hall and Preston 1988: 21); and Islam that 1990–2000 will constitute the recovery period (Islam 1985: 66).) In the same vein, and in a bold generalization of Jevons's position, Abalkin suggests that the future of the long-wave research may be connected to the understanding of cosmic processes (Abalkin 1992: 14).

(7) At the same time, Haustein and Neuwirth used the same type of procedure to compute the spectrum of deviations from a long-term exponential growth in world industrial production. Other series, such as energy consumption, inventions, innovations, and patents, were treated in the same way, and the authors found evidence for 50–year cycles, but also for 40-, 32-, 20-, 13-, and 7-year cycles (Haustein and Neuwirth 1982: 53, 66, 69 ff.). These cycles were interpreted as lag cycles and not as life cycles; no dominant auto-correlation cycle was found, and as a consequence the hypothesis could not be supported. Beenstock differenced the data and concluded from very long price series (Beveridge series on European wheat prices from 1500 to 1869 and Phelps-Brown and Hopkins historical series of prices) that there was some evidence for 157-, 40-, 27-, and 14-year cycles, and still no support for the Kondratiev hypothesis (Beenstock 1983: 139 ff.).

(8) Ewijk was able to find some evidence for long waves in French industrial production, provided the Second World War years are linearly interpolated (Ewijk 1982: 486). Bieshaar and Kleinknecht also used the GLS technique in order to smooth the differences in variance resulting from the presence of war impacts (Bieshaar and Kleinknecht 1986: 185). Klimenko and Menshikov simply suggest excluding the war years and their presumable influence in the period 1933–57 (Klimenko and Menshikov 1987: 351). Solomou, in his research on British data, uses dummy variables and linear interpolation for war years, and finds no evidence of Kuznets long swings in the period after 1913 (Solomou 1987: 25, 34–5). Rasmussen and his colleagues justify the elimination of war influences through interpolation, since those periods can account for most of the variance of series and since wars are supposed not to be 'directly involved in the basic mechanisms of the economic long wave' (Rasmussen *et al.* 1989: 288).

(9) See this new example of the contradictions of the assumption of rationality: 'Why is it that human subjects in the laboratory violate the canons of rational choice when tested as isolated individuals, but in the social context of exchange institutions serve up decisions that are consistent (as though by magic) with predictive models based on individual rationality?' (V. Smith 1991: 894). Vernon Smith, one of the founders of the programme of experimental economics designed to repair the averages of the rationality postulate, is right to call for the spell of magic to save the general equilibrium. A somewhat more secular explanation would indicate that institutional or social processes operate in order to coordinate decisions and to avoid extreme tensions and ruptures in the social process.