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Book reviews

Hodgson, G., Screpanti, E. (eds.): Rethinking Economics. Markets, Technology and Economic Theory. Aldershot and Brookfield: Edward Elgar 1991. 206 pp. + xi. ISBN 1-85278 461-4

This book is a collection of selected papers presented at the second conference of the *European Association for Evolutionary Political Economy* held in Florence, November 1990. It contains, besides an introduction by the editors, nine contributions structured in two parts of the book. In the introduction, Hodgson and Screpanti, who are, respectively, the General Secretary of the Association and the local organizer of the meeting, call for new theoretical approaches to be gained from a combination of orthodox and heterodox economics, as well as from insights drawn from sociology, philosophy, political science, history, biology, systems theory and other disciplines. Eclecticism, however, "whilst initially necessary", is not seen as a solution (see p. 6). More than half of the contributions are dedicated to surveying recent literature, some of them more generally, others focusing on specific literature. One paper deals with methodological issues and four chapters present new approaches.

Two contributions, each introducing one of the two parts of the book, survey explicitly the "Contributions to an Institutional Theory of Price Determination" (Mark R. Tool) and the "Present State of Evolutionary Economic Theory" (Ulrich Witt). Both streams of thought are, according to the introduction, starting points for the Association's discussion of a further development of economic theorizing and both are corner stones for the theoretical discussions in the book. Witt's "reflections" on evolution are essentially based on individual behavior, stressing how creativity and cognitive structures influence the evolutionary process, while Tool, of course, stresses the influence of (societal) institutions on individual decisions. Nevertheless both authors resist the temptation to present their views as one-sided "truths". They rather complement one another; an important research program in the future could be the theoretical and empirical specification of these connections. Tool and Witt provide the basis for such discussion.

Frederick C. v. N. Fourie questions the "Nature of the Market". While, in traditional economics, the main features of the market are assumed rather than explained, Fourie tackles explicity the "typical or intrinsic nature of the market" as an economic phenomenon that is "inextricably interwoven with various societal organizations and communal structures" (p. 41-2). As a central concept he offers a "structural identity principle", which stresses qualified exchange as well as rivalry relations as important features of "the market" (see p. 48). Another typically institutionalist argument is raised in Mary K. Farmer's and Mark L. Matthews' "Cultural Difference and Subjective Rationality: Where Sociology Connects with Economics of Technological Choice". This paper provides a methodological foundation for dealing with rational choice arguments when choices within different (national) cultures are compared. This important point (we may think of its relevance for the contemporary European discussions on so-called economic integration and multi-cultural societies) is taken up by Maureen McKelvey, who critically analyzes four recent concepts dealing with similar problems of national institutions regarding technical innovation.

The explicit scope of all other articles is to provide – or at least to develop further – specific concepts of theoretical work towards evolutionary economics. Dominique Foray and Pierre Garrouste search for a unified framework to analyze firms and industries on the conceptual basis of Maturana and Varela's general theory of *autopoietic* systems. Here they generalize earlier approaches to explain partial phenomena in the same field and make an important contribution to scientific evolutionary economic theory. As the *autopoiesis* approach (Forray and Garrouste use Varela's term "eigenbehavior"; p. 67) is not uncontested, discussions on that topic may be induced by this paper. Mario Morroni, in the following chapter on "Production Flexibility", becomes more concrete as regards the economic "facts" to be examined. After some taxonomic remarks, he links the problems of uncertainty to the industries' attempts to make production processes more flexible. After this theoretical preparation, the concept is applied to an examination of the effects of computer-based technology on production flexibility.

"Rethinking economics" is not necessarily a verbal undertaking. The last two chapters deal with the two main possibilities to apply mathematical and statistical tools to (evolutionary) economics: simulation studies and empirical work. As they dare to be much more concrete, they offer the possibility of asking critical questions (which I see clearly as an advantage). Richard

Goodwin introduces a simple non-linear model, in which he claims to incorporate Marxian, Keynesian, and Schumpeterian ideas. The model formulates these ideas in terms of "Economic Evolution", and "Chaotic Dynamics" – with all the advantages and disadvantages of reducing "the economy" to a system of five equations. On the basis of these assumptions, Goodwin shows that a model, representing a chaotic attractor, is dynamically unstable. There is no need to fall back upon exogenous shocks to get irregular behavior. The more important – but also more difficult – question of whether such a model is structurally stable or not is vaguely answered: "I would say it is structurally unstable, which is why it is so fascinating". (p. 151) This is a point on which much further work awaits economists. (Goodwin himself, of course, did a lot of fundamental work in this field.)

Geoffrey M. Hodgson tackles the problem of describing and explaining fundamental changes in aggregate output in an institutional and evolutionary framework. Orthodox theory could only describe those cases by "mysterious shifts" in well-behaved production functions. Drawing a line from Veblen over Penrose and Polanyi to Nelson and Winter, he uses the concept of *autopoiesis*. In a concluding empirical section, Hodgson interprets the historical economic development of 16 industrialized countries on the background of these approaches. He shows that a historical view in the sense of evolutionary and institutional economics is not necessarily restricted to verbal description.

This volume provides a successful mixture of theory and empiricism, of various heterodox approaches, of criticism and alternatives. Pure criticism of neoclassical economics makes up only a minor part of the book, although it can be derived implicitly from the large variety of alternatives offered. *The* alternative to contemporary mainstream economics is, fortunately, not yet in sight. What we are experiencing is an evolution of heterodox approaches which promise to provide better results in important fields of economic research.

The whole issue is rounded off by an accompanying volume, which presents more papers of the same conference and is dedicated to more practical questions of economic policy: "Towards a New Europe?". The next volume(s) containing the results of the third annual conference, held 1991 in Vienna on the transformation processes in Eastern Europe, should be eagerly awaited.

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North, D.C.: Institutions, Institutional Change and Economic Performance. Cambridge: Cambridge University Press, 1990. pp. + viii.152 $32.50 (cloth); $10.95 (paper)

North has produced a succinct statement of how economists should deal with institutions while equipped with the anthropologist's sensitivity to the cultural framework. The concern with cultural inertia should not be surprising given that North is an economic historian. North's institutionalism does not overemphasize efficient rationality as the case with Oliver Williamson's transaction cost approach. In fact, North distinguishes his work from Williamson's by calling it the "University of Washington" view. The view includes others e.g., S. Cheung, Y. Barzel, K. Leffler, M. Hashimoto, and T. Eggertsson.

However, North does not altogether abandon Williamson's agenda. He reasons that institutions arise in order to capture greater benefits from trade. Thus, North's critique of the neoclassical agenda should not make old institutionalists (the followers of Veblen and Galbraith) totally delighted. In fact, he does not even mention them.

In this light, does North present a synthesis which supersedes the neoclassical emphasis on efficiency and the old institutionalist stress on culture? Or does he offer an eclectic feast? As he admits, he does not attempt a theoretical work of synthesis. But he does not even produce a tentative sketch towards such a synthesis. While this is a harsh assessment given that the book is rich in insights, I found the author constantly juggling between the two opposing poles: culture and rationality. Thus, the book is a taxing collection of terms, anthropological insights, and historical illustrations which are crying for a systematic organization. Before I substantiate my assessment in the fourth section, I offer a summary of the three parts of the book in the following three sections.

1. In the first part, North defines institutions as the "rules of the game," which are different from purposive organization. Organizations such as firms are defined by strategies, while institutions

are their constraints. He sets out to find why certain institutions are more successful than others in promoting prosperity.

North asks, why is impersonal trade in modern Western countries possible? How could cooperation arise in one-time trade, contrary to what is predicted by the axioms of rationality? Furthermore, neoclassical theory predicts that inefficient rules, as inefficient producers, would fail eventually because of the selection power of competition. However, the inefficient trading practices of the traditional *suq* (market), medieval manor, and the Champagne fairs persisted for millennia.

North's answer is that competition cannot engender maximization behavior because of the complexity of the environment. He borrows Ronald Heiner's explanation of the rise of institutions. According to Heiner, agents develop rules when the gap between competence and the cost of deciphering information widens. Such rules try to deal with the problem of uncertainty by limiting the choice set. According to North, rules arising from the gap, although inefficient, could persist because of transaction costs.

Rules, according to North, come in two flavors: informal and formal. He identifies informal rules with unwritten norms, while he equates formal constraints with laws. As I discuss below, this implies curiously that pre-literate societies are characterized only by informal constraints, while literate ones are typified by both. Given North's notion of the historical precedence of informal rules and their persistence despite political revolutions, it is curious why he asserts that formal rules underlie informal constraints. I think he does not make a careful distinction between cultural *norms* which precede formal rules and *habits* which spring up around such rules.

Formal rules, like contract enforcement, could reduce transaction costs. The high cost of informal enforcement limited the expansion of trade before the rise of the Western world. Needless to mention, trade and specialization are the pillars of economic prosperity which agents in the West have come to enjoy. The institution of formal enforcement as a public good explains the puzzle of the rise of cooperation in one-time deals. In the third world today, there is a great lack of such institutional frameworks to ensure private property, development of capital markets, and the distribution of rewards according to effort (not political patronage). Even when such frameworks exist, the third party (state) which is responsible for their enforcement often acts partially.

2. In the second part, North argues that institutional change arises from the incessant purposive drive of organizations to maximize wealth. Actors usually undermine inefficient rules by violating them at the margins. Thus, institutional change is an incremental affair of gradual change of norms and attitudes, as with regard to usury laws in Western Europe or the prerogatives of the Crown in England.

As organizations exchange goods and encourage specialization, they gain knowledge about reducing transformation and transaction costs. If institutions reward investment, rather than influencing rulers, we have at hand a self-reinforcing mechanism of growth. A society led by Schumpeterian entrepreneurs possesses an adaptive efficiency (not allocative efficiency) for coping with new circumstances. The consequent vitality is accomplished while the formal and informal framework is stable. Stability is a necessary but insufficient condition for efficiency. In fact, frameworks may be inefficient but do not change unless tastes/values are combined with economic incentives. The inefficient (but profitable) institution of slavery in the U.S. South, e.g., did not change until anti-slavery values were aided by the low cost of giving up slavery for most of the electorate.

Besides high relative costs, another reason for the persistence of inefficient frameworks, and the consequent failure for societies to converge, is the path-dependency character of the life of institutions. Borrowing from the ideas of W. Brian Arthur, it is very costly to set up new constitutions. Thus, once they are set up, they engender increasing returns as they are used because of learning-by-doing and the adaptive character of expectations. That is, expectations formed by the institutions become self-fulfilling because they are "historically derived subjective modeling". Thus, institutions usually become entrenched – even when they fail to promote social efficiency.

3. In the third part, North discusses the institutional framework of Spain in the 17th Century as an example of how inefficient institutions survive as a result of self-feeding mechanisms. The framework provided the king with great prerogatives to the detriment of property rights. This hindered a proper structure of incentives and hence stymied entrepreneurship. The underdevel-

opment of Latin America, as opposed to North America, is explained as the result of the underlying Spanish framework.

In contrast, the British-North American framework, which limited the arbitrary confiscation of property by the king, explains the flourishing of innovations and growth. Other examples of efficient path-dependent development is the British common law tradition and the 1787 U.S. Northwest Ordinance. The Ordinance dealt with the vast area of land in the West, unambiguously defining property rights and how the emerging territories could become states and join the U.S. While through time, the legal framework became more concrete through interpretations, it maintained a clear and proper incentive structure which encouraged expenditures on investments in technology.

Such efficient frameworks are contrasted to the *suq*. Although it has survived for thousands of years, the *suq* failed to operate within efficient formal rules. It persisted within face-to-face rules which are highly inefficient. This has prohibited the development of efficient capital markets such as those in the West. Of course, the printing press, uniform standards of measure, rise of insurance companies, and new accounting methods in the West have reduced the cost of transactions. However, the most distinguishing institution in the West is the rise of formal merchant laws which protect property.

Finally, North muses on how economic historians should construct their stories in light of the pervasiveness of institutions. It would be sterile to build growth models which focus exclusively on technology and investment. It is important to bring up front the institution of incentive structure, which is implied in all neoclassical models of growth. The incentive structure does not have to be endogenous. A case in point is the formal institution imposed on the British Crown in the 1688 Glorious Revolution. Such a formal framework, following the work of Alan Macfarlane, could be traced to informal cultural norms of individual rights starting with the Magna Carta in the 13th Century. In this manner, North resorts to *ad hoc* cultural reasons when efficient rationality fails to explain why proper rules arise in some countries and not in others.

4. North's narrative fails to give us a sketch of when we should appeal to rationality as opposed to culture. Orthodox economists would find it wanting for its failure to give an endogenous account of institutions, while old institutionalists would judge it too much concerned with rationality. I concur with both assessments: North's book is a hodgepodge of arbitrary cultural explanations and individualist cost/benefit analyses.

To be accurate, North never intended to provide us with a theoretical synthesis which avoids the inconsistent playing on both sides of the fence. Nonetheless, he fails to satisfy a more primary question, namely, what does he mean exactly by the word "institution"? North and many others employ the term loosely to denote incentives, standards of weights, uniform money, ideology, cultural norms, informal constraints, formal rules, family structure, constitutions, rules of politics, habits, property rights, enforcement tactics, contracts, legal framework, trust, reputation, and moral constraints.

When a distinction is finally made, between informal and formal rules, it amounts to the difference between written and unwritten constraints. However, the difference lies elsewhere. Namely, it is between an obligatory (formal) constraint such as not favoring one's relative in promotion decisions (nepotism) vs. voluntary (informal) constraint such as helping a relative in times of financial stress. While the sands between what is formal and what is informal somewhat shift historically and comparatively, *all* societies, not only literate ones, possess both. There are formal constraints in hunting/gathering bands in the form of ostracism and in kinship horticultural societies in the form of blood feuds.

North briefly makes another distinction, viz., between institutions and organizations. Unfortunately, he does not carry it far. To wit, he confuses the two when he treats the structure of incentives, values, and principles – which motivate organizations – as part of the institutions of efficiency. The distinction between institutions of efficiency and principles of organization has, broadly speaking, not been adequately distinguished in economics (Khalil, 1990). A sports game, e.g., includes rules or institutions on how players should not toss the ball. But such rules do not specify the principle or goal of the game.

The rules/principles contrast has great ramifications with regard to path-dependency analysis highlighted by North. Institutions, as rules of efficiency, experience a different *kind* of non-ergodic feedback than do organizations embodied by frameworks or regimes of principles. A recent example of the former genre is Paul Krugman's (1991) trade theory. Ironically, he also

uses Arthur's non-ergodic technique, but shows how increasing returns induce geographical polarization, i.e., the core/periphery dichotomy. Such a dichotomy is the result of unintended consequences of transportation and fixed costs. Thus, poverty of the third world is not a product, à la North, of the lack of clear property rights. Rather, the path-dependency of regional development, according to rules of efficiency, lends credence to the argument that poverty is the result of attracting physical and human resources to the center, away from the periphery.

This implies that there are no "correct" legal frameworks. Although North is at pains to distance himself from 19th-Century orthogenetic views of history, à la Marx and Weber, as ending up with European institutions, he cannot repress his enthusiasm for the superiority of Western democracy and the British-North American path. While he muses why the *suq*, tribal trade, and Caravans have not adopted the "correct" Western institutions, he pays little attention to how competition in advanced markets has given rise to unproductive expenditures on position goods (see Hirsch, 1976). To wit, the transaction cost of price discrimination and other marketing devices are only more sophisticated versions of the wasteful haggling of the *suq*. In addition, he neglects the literature on rent seeking which shows how strict property rights in the West, in comparison to Japan, has engendered a huge, unproductive legal industry. Moreover, the celebrated structure of incentives in the West could be shown equally to be responsible for luxury consumption, complacency at work, and low saving rates.

It is illusive to think that success is the result of a "correct" regime of rules and principles. Such an illusion amounts to what I call elsewhere the "reification of institutions". For North, the divergence of the British-North American and the Spanish-Latin American developments in the 17th Century is the result of the greater security of property rights in the former. This neglects, however, the fact that the Spanish framework was very successful in driving out the Moors in the 15th Century, economic expansion in the 16th Century, and continuation of the empire in Latin America in the following two centuries. Also, the "correct" British-North American regime has not secured, as we witness today, a lasting superiority for the U.S. economy.

Of course, North denies that he promotes a *telos* of institutional change towards a "correct" one, and admits that Western democracy is riddled with problems – e.g., the agency/principle dilemma. However, such denials are *ad hoc* qualifications of his basic view of rectilinear progression towards Western-style efficient institutions. In fact, North's qualifications show that he is caught, without being able to synthesize, between the two opposing paradigms, viz., the culturalist agenda and efficient rationality.

The argument that there is no "correct" institutional scheme need not lead to cultural relativism à la German Historical School and old institutionalism. To wit, cultural relativism concurs with the rectilinear view that each culture is radically particular. While one camp concludes that it is not possible to compare the presumably different frameworks, the other camp arranges them along a progressive tendency towards an ideal convergence. One major task of evolutionary theory is the elucidation of the development of socio-cultural frameworks without falling into cultural relativism or rectilinearism (see Khalil, 1993). Certainly, this is not the place to start such a task.

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Tylecote, A.: The long wave in the world economy. Routledge, London, 1992. 338 pp. ISBN 0-415-03690-9.

Tylecote's book deals with the 45–60 year Kondratieff long wave, a construct which implies that long-run economic growth occurs in alternating periods of slower and faster growth. These waves should not be confused with the (Kuznets) "long swings" which have approximately half the length of a Kondratieff wave. Theorizing on long waves in economic growth has developed somewhat outside of mainstream economics, and the long wave literature itself seems to come about in waves. Generally speaking, prosperous periods are bad times for theories on long waves, and periods of slower growth stimulate them. A first literature wave occurred between the two World Wars. Tylecote's book belongs to the second wave of the last 15 years. The latter is characterized by two lines of research.

The first line of research includes statisticians addressing the question of whether long waves can be discerned in aggregate production or GNP time-series of major industrial countries. Often without engaging in theoretical discussions, they simply examine whether actual fluctuations in those time-series do or do not fit in the time-frame of the 45–60 year Kondratieff wave. The second line of research deals with theoretical explanations of long waves (assuming they exist). These researchers can be divided into three major groups. First, Marxists, who analyze aggregate profit rates in a long wave context; second, evolutionary economists, who examine historical patterns of innovation in a long wave context; and third, adherents of a "regulationist" or "social structure of accumulation" approach emphasizing the role of social and institutional change for long-run economic growth. Tylecote's book belongs to the last category. The author makes it quite clear that his line of reasoning is mainly qualitative, yet ambitious: "The argument is presented ... without a single mathematical equation, and the issues discussed extend across and beyond the social sciences and over the history – not only *economic* history by any means – of the world since the late eighteenth century" (p. 2).

The book has a "theoretical" and an "empirical" part. The former starts with a (selective) review of long-wave literature, followed by a discussion of "technological styles" in chapter 2. Chapter 3 deals with the author's view on the interaction of political, social and economic structures. This is followed by a treatment of monetary and demographic factors in chapters 4 and 5. Part one concludes with the author's propositions about the relationship between income inequality and economic growth which he himself considers "highly controversial" (p. 2).

Chapters 8–11 provide a qualitative account of the history of the World economy, subdivided into periods which roughly correspond with the "classical" dating of Kondratieff long waves (1780–1850, 1850–1896, 1896–1945, and 1945 to the present). The final chapter ends with proposals "for resolving ... a very deep-seated and acute crisis, so that we may release the economic and social potential of recent developments in technology, and launch a new world upswing without parallel in history" (p. 3).

There have been major conferences on long waves at Siena (1983), Paris (1983), Weimar (1985), Siena (1986), and Brussels (1989). The author acknowledges that he participated in some of them; however, he missed the Brussels conference, and the conference volume appeared too late for inclusion in his book. This implies that Tylecote missed the pathbreaking work on time-series by authors like Gerster, Metz, Stier and Reijnders[1]; also the earlier work by Reijnders (1988) and by Rasmussen and Mosekilde (1989) has not been taken into account (Tylecote's book seems to have had quite a long production time). This has serious consequences. Tylecote concludes from elementary computing of average growth rates that the existence of long waves is doubtful in the 19th century, and he then invests much effort into explaining such long wave "anomalies". However, more sophisticated methods have meanwhile rehabilitated the long wave hypothesis for both the 19th and 20th century. In fact, the Kondratieff long wave pattern appears to be surprisingly regular. This makes much of Tylecote's reasoning obsolete.

Another problem is his treatment of technological innovation, borrowing from Carlotta Perez's hypotheses on new "technological styles". While this may appear at first glance as something really innovative, a closer examination of Tylecote's chapter 2 reveals that his description

[1] Besides the references listed below, some of the work by these authors is published in: Kleinknecht A, Mandel E, Wallerstein I (eds) New findings in long-wave research, Macmillan, London, and St. Martin's Press, New York, 1992.

of "new technological styles" is not much more than a modern re-phrasing of Schumpeter's original (and, in some aspects, richer) theory of innovation clusters. Having myself been involved in a debate as to whether Schumpeter's hypothesis of clusters of major innovations is realistic (which it is, see Kleinknecht 1987), I wonder how the author could ignore almost all of this work and then try to re-invent the wheel.

A similar problem holds with respect to modern Marxist studies on long run profit rates. Tylecote is not aware of work by authors such as Fontvieille, Menshikov, Poletayev, Reati, or Shaikh who established evidence of a long wave pattern in aggregate profit rates (see footnote 1, and the references below). Instead he tries to prove (with shaky data) that Mandel's earlier proposition with respect to a long wave pattern in profit rates is unconvincing (chapter 6). Obviously, Tylecote's preference for a Kaleckian under-consumption crisis theory brings him in conflict with (supply side) Marxist research.

Besides the limited use of quantitative methods, the book shows a major weakness with respect to the appropriate level of analysis. The author seems to confuse arguments relevant at the level of the World market with arguments relevant for individual (groups of) countries. While (in accordance with much of the literature) he seems to consider long waves as a World market phenomenon, much of his historically descriptive argument is concerned with development patterns of specific countries whose relevance for the World economy remains obscure. The problem with his assumption that institutional change drives technical innovation (and hence economic growth) is that institutions and patterns of institutional change are likely to vary considerably across different countries and from culture to culture. Therefore, it becomes difficult to demonstrate how institutional change in certain countries can account for the economic growth of the World economy.

This is not the place to go into detail, but I would like to suggest there be a clear distinction between, on the one hand, factors which explain long wave variations in average growth rates of the World economy, and, on the other, factors which explain the deviation of the growth of individual countries from World average growth. While Schumpeterian and Marxist research is predominantly concerned with explanations of the former, many of the "regulationist" (and of Tylecote's) arguments are likely to be more suited to explain why certain countries (like Japan or England) systematically realize above or below average growth rates with respect to the World economy. The debate between the three currents in the long wave literature seems to suffer from the confusion of these two levels of analysis.

Clearly, the book has the merit of trying to combine actual history with economic and social science. It is well written and covers a number of interesting details. Some passages reminded me of Schumpeter's *Business Cycles*. However, mainstream economists who tend to be a-historic and who could, in principle, learn a great deal from such an approach, will probably ignore it. Due to the author's obvious aversion to quantitative methods, the book lags behind the present standards in long wave research. This makes it unsatisfactory for the specialist scholar. On the other hand, because of its extremely selective perception of the relevant literature, I hesitate to recommend the book as an introductory text to the more general reader.

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Chiarella, C.: The Elements of a Nonlinear Theory of Economic Dynamics. Lecture Notes in Economics and Mathematical Systems. Springer, Berlin, Heidelberg, New York, 1990. 129 p, DM 39.00 (paperback). ISBN 3-540-52622-6.

The mathematician and economist Carl Chiarella describes in his book "The Elements of a Nonlinear Theory of Economic Dynamics" a complex of themes which since the beginning of the eighties has moved increasingly into the field of vision of economists and which has for this reason led to numerous publications. For various reasons, however, this book is not just another new book about nonlinear economic dynamics.

To start with, methods and formal tools are introduced and applied which, though standard tools in the natural sciences, have so far been used only very rarely in economics.

Moreover, the reader will be pleased with the limitation to a few "elements" of the very varied theoretical structure since he will never lose sight of the basic statements which the author wishes to transmit. Everyone with an open mind toward this kind of analysis will receive many suggestions and hints about how to continue his own work with this theory within a more complex framework.

Before examining in detail the formal tools, Chiarella asks why there is any need for a nonlinear dynamic economic theory. In a brief historical sketch of the development of both economic theory and the theory of dynamic systems, he points out that the former development is not independent of the development of the latter. The predominant linear models are not appropriate for the theoretical starting points of Hayek, Harrod, Kaldor, the Stockholm School and, by no means last, of Schumpeter. Hence the analysis by means of nonlinear dynamic theories becomes essential. Thus he points at the end of the first chapter to problems in economics which require a nonlinear theory, such as business cycle theory or the dynamics of foreign exchange and financial markets.

Chapter 2 deals in a general manner with techniques developed by the theory of dynamic systems which are necessary for the understanding of the subsequent chapters. To make it somewhat easier for economists the author here foregoes strict formal analysis and gives preference to verbal-geometric explanations. At the beginning he deals only with two-dimensional nonlinear systems. Starting with the various stability concepts of linear systems he then explains the theorems which are important for the existence of limit cycles in nonlinear systems. He emphasizes in particular the Hopf-bifurcation and Poincaré-Bendixson theorems.

The subject matter of the beginning of the second chapter must, however, be understood only as the minimum necessary background to allow the reader to understand the subsequent discussion. Readers without the necessary background are recommended to consult the voluminous literature cited. Next some of the basic techniques of a qualitative analysis of nonlinear systems are presented. The method of averaging, which is considered an approximation technique for the amplitudes of limit cycles, is discussed theoretically, described and applied to the example of the Lienard equation.

This basic analytic tool box, which has thus far not found much application in economic analysis, becomes of central importance in the subsequent chapters because it permits comparative dynamic investigations of the amplitudes of limit cycles.

The method of relaxation oscillations is of equal importance for the rest of the book. Simply put, this method investigates effects of different speeds of adaptation on the dynamic system and it finds the limiting limit cycle. Neither method has so far been widely used in economic analysis and for this reason is described in detail at this place in the book.

Up to this point the discussion referred only to second-order nonlinear systems. There follows, therefore, a view of higher-order nonlinear systems. At present, the mathematical analysis of such systems is still not available. The author presents the centre manifold theorem to discover cycles in higher-order dynamic systems. The remainder of the chapter deals with the present "hot topic" of chaos theory by means of the example of the Lorenz attractor, and the major characteristics of so-called chaotic systems are worked out in easily understandable language. The second chapter is purely theoretical in nature and it is the only chapter without economic background. The rest of the book captivates by its numerous economic applications.

Thus, chapter 3 undertakes to present new vistas of the old endogenous business cycle theories. This is done primarily with the techniques which were presented in the preceding chapter. The author first discusses briefly the present state of the business cycle discussion. He then presents the multiplier-accelerator connection by means of an investment function which consists of a weighted average of preceding investment decisions. This connection consists of a

two-dimensional nonlinear system of differential equations which follows from the particular form of the investment function. The nonlinear part of the function is separated out for the subsequent analyses.

The first step is to introduce into the general model a Goodwin-type investment function. In the detailed explanation it is investigated how the limit cycle which is possible in the general model reacts to particular changes in the model's parameters. These parameters, e.g., the acceleration coefficient or the marginal propensity to save, affect the amplitude of the cycle. The limiting limit cycle is worked out by means of the "method of relaxation oscillations" applied to changes in the time lags built into the model. And this limiting limit cycle is in turn subjected to a comparative dynamic analysis.

Thus a model variation in the direction of anti-cyclical Government expenditures leads in this, as in a pure Keynesian model, to a diminution of the amplitude of the limit cycle and with it to a dampening of the cyclical amplitude. The last analysis uses a different investment function whose acceleration coefficient shows certain saturation tendencies at a certain distance from the equilibrium output. In principle, results similar to those of the preceding model can be derived, but the effects of changes in parameters are not as strong. The analysis of this chapter demonstrates impressively how business cycle models can offer new insights by the introduction of small nonlinear elements.

The economic content of chapter 4 consists of a macro-economic model with an explicit Government sector. The Government sector is modeled by a Governmental budget restriction which allows the Government to issue money or bonds to finance a deficit. In contrast to the analyses of a similar complex of problems which prove the existence of possible limit cycles by the application of the bifurcation theorem of Hopf or the Poincaré-Bendixson theorem, Chiarella employs the method of averaging which in his opinion is more suitable for economic applications. In this example, the non-linearity, which is based on Kaldor's approximation to a non-linear investment mechanism, is approximated in the model by a piecewise linear investment function. This trick allows an easier entrance to the dynamic behavior of systems and with it the proof of the existence of the limit cycle. Next, once again a comparative dynamic analysis is worked through. The heart of this chapter is, however, not the results but the alternative description of the technique of the application of the averaging method.

The preceding two chapters dealt with two-dimensional non-linear economic systems. In chapter 5 the analysis of a three-dimensional non-linear system is undertaken by means of the centre manifold theorem. The basic economic model is based on the Goodwin model of cyclical growth of the sixties. Goodwin had used a system of differential equations which was equivalent to the predator-prey model of Lotka-Volterra in the biological application. The major characteristic of this model, but also its central weakness, is that it produces cycles whose amplitudes depend on the initial conditions of the system.

Chiarella has added to the original Goodwin model a time-lag equation for real wage adaptation to past employment ratios, which interacts with the non-linearities of the two other equations. This procedure has the purpose to get a stable limit cycle instead of many closed orbits whose amplitude varies with the initial conditions. The resulting non-linear three-dimensional system of differential equations is for a small time lag reduced to a two-dimensional system by means of the centre manifold theorem, the Eigenvalue analysis of which suggests a limit cycle. The method of averaging applied to it differs from the applications in the preceding chapters. In the present chapter the standard form is applied as briefly described in chapter 2.

Chiarella arrives at the result that both the employment and the wage-share cycles depend on the time lag of the wage adaptation. This means that the greater is the time lag of the adaptation of wages to the different employment situations, the smaller are the amplitudes of economic activity. At the end of the chapter the effects of changes in the other model parameters are also discussed. Since we deal originally with a three-dimensional system of differential equations a chaotic behavior might theoretically result with an increasing time lag, as Chiarella remarks as transition to the next chapter.

Chapter 6 discusses chaotic behavior within the framework of the instability of the cobweb model. The author limits himself here to a simple discrete supply and demand model, with both an expectation and a transactions time lag. As long as both lags are equal the known results of the cobweb model are confirmed. If, however, the time lags differ, i.e. if the time periods in which producers form their price expectations are longer than the time periods between the transactions, chaotic behavior may result through the process of period doubling.

Formally, the resulting system of differential equations will be approximated by diverse parameter transformations of the known discrete form of the logistic equation, with the aid of which the period doubling transition to chaos is vividly explained. This equation, which is also known as the Verhulst-dynamics, is the equation among the so-called chaotic systems which has been most extensively studied. Hence the author can derive a completely described dynamic behavior of his initial problem by a reverse transformation of the parameters from the known behavior of the logistic equation. Accordingly, differences in the two time lags may induce periodic and later chaotic behavior.

Chiarella uses in his model an adaptive scheme of the formation of expectations and at the end of the chapter he deals briefly with adaptive vs. rational expectations in the economic model, which will be more thoroughly discussed in the next chapter.

The problem of so-called dynamic instability which arises in particular in rational expectations models is the content of the next-to-the-last and probably the most interesting chapter. All techniques and concepts discussed in the preceding chapters are here applied and the attempt is made to overcome the instability problem by the inclusion of a non-linearity within the framework of the model. Briefly, the problem of dynamic instability consists in the fact that in models in which the economic actors anticipate correctly the rates of change of the economic variables, only a limited number of trajectories depending on the particular initial conditions converge towards the equilibrium. This results from the saddle point property of the equilibrium. The majority of the trajectories, however, move more and more away from equilibrium. The literature has tried to overcome this phenomenon by means of a number of tricks, among them, for example, a discontinuous jump of the economic variables towards the equilibrium. None of them was economically very convincing.

Chiarella shows first the major characteristics of the dynamic instability problem in a simple monetary model with adaptive expectations in which the demand function for money may, among others, depend non-linearly on the rate of inflation. This model was, incidentally, expanded by Chiarella stochastically in one of his more recent articles.[1] He defends his use of adaptive expectations with the argument that systematic errors of estimation are not caused by the mechanism of forming expectations but by the use of low-dimensional models to which economic theorists are forced because they can be mathematically treated. In higher-dimensional models the regular fluctuations which are observed on a lower-dimensional plane become under certain assumptions apparently irregular oscillations in which economic actors can no longer make systematic forecast errors regardless of which mechanism of forming expectations they use.

At the beginning of the chapter the author proves that in his model and with perfect foresight, i.e. if there is no time lag in the formation of expectations, the cause of the dynamic instability lies in the instantaneous adaptation of the money market which many rational expectation theorists assume. He then tries, by varying the lengths of the time lags in the formation of expectations, to analyze in greater depth the region in which the instability occurs. Thus with a positive time lag there arises a limit cycle for the rate of inflation in place of the previous instability which depends on various economic parameters of the model. This cycle is analyzed by means of the method of averaging.

In the appendix alternative theorems to prove the existence of limit cycles are presented and applied, all of confirm this result but do not permit a comparative dynamic analysis. Finally, the dynamic adaptive processes underlying the limit cycle are economically interpreted.

In order to allow a better analysis of the transition of adaptive expectations towards perfect anticipations, i.e. as the time lag moves from positive towards zero, Chiarella uses for clarity's sake the phase-diagram technique as a more heuristic approach. The result is that the behavior of the system with marginally small time lags corresponds to the behavior with perfect foresight. But on the basis of this analytic technique the dynamic instability problem turns out, in the non-linear model approach, to be a limiting limit cycle which has its cause in the transition from adaptive expectations to perfect foresight. Needless to note that this limiting limit cycle occurs whatever initial values are used for the relevant economic variable.

In the model this variable is the rate of inflation and thus also the price level. It is subject to slow and fast movements which within certain limits exhibit discontinuous jumps. Hence this

[1] Chiarella C (1990) The Bifurcation of Probability Distributions in a Nonlinear Rational Expectations Model of a Monetary Economy. Eur J Political Econ 7:65–78.

approach permits consideration of higher dimensional problems in which the manifold characterizing the dynamic movement is embedded. The discontinuous jump in the economic variables appears thus no longer arbitrary and specified ad hoc. as is the case in the lower-dimensional linear models for reason of convergence.

In order to show that chaotic behavior can also appear in monetary models, a time-discrete version of the preceding model is analyzed at the end of the chapter. It turns out that this analysis is more complicated than the corresponding system of differential equations. Chiarella refers therefore to the possibility of computer simulation and to models with similar behavior which have already been analyzed.

The main part of the book ends with the graphic representation of the Lyapunov exponent found by simulation, whose positive appearances, for particular values of the parameters, suggest a chaotic model behavior. The formal conclusion is presented in the final chapter which gives a summary and the prospect of future possibilities of research. The latter is larded with a multitude of interesting suggestions for a research program within the framework of this form of analysis.

Chiarella shows in this book that a revitalization and new formulation of old research approaches with techniques from the theory of nonlinear dynamic systems may lead to results which only a few years ago would have been unthinkable and which permit a better insight into the dynamic behavior of economic systems. It is not grand, spectacular, new approaches and discoveries which make this book interesting and well worth reading, but small changes which, though used sparingly, yet lead to remarkable insights. A radical step away from traditional economics is consciously avoided. On the contrary, it is here employed as a toolbox to be godfather, as it were, to a new point of view of the complicated economic process. An enlarged second edition in the near future which would include stochastic elements to the necessity of which the author refers in the last chapter and which he has already considered in his latest publications, would be desirable. In this welcome new edition more care should, however, be taken to avoid printing errors which frequently make the life of the reader unnecessarily difficult.

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Nakicenovic, N., Grübler, A. (Eds.): The diffusion of technologies and social behavior. Springer and IIASA, Berlin Heidelberg New York 1991. 605 pages, 107 figures, DM 220.00 (hardcover). ISBN 3-540-53846-I.

The diffusion of innovations is a mature research subject and so I did not expect to discover much of a novelty in this work. I must admit that I changed my opinion quite radically as I went on reading. The economic emphasis of the volume is in fact embedded in contributions that make the whole an integrated contribution to the social sciences, something not so common. It is by no means an easy promenade because it is very diversified, probably as a function of the degree of maturity of the subject; shifting attention from one dimension to another while keep trying to make sense of the whole is always a problem. The book contains nearly 600 pages and is made of 24 individual and joint contributions. It is a conference volume presenting a large heterogeneity of views, opinions, methods and whatnot. And yet progressively a very beautiful impressionistic picture is built up; diversity does fulfil the constructive and the evolutionary role in the design of the diffusion perspective. Diffusion is basically a learning process and this book exemplifies very nicely this fact. It is also a research book: you do need to be acquainted with the basic features of the literature if you wish to classify and integrate efficiently the acquisitions of the reading. However, if the reader is a graduate student with no previous background in the subject it can serve as an excellent introduction to the components and morphology of the subject. Access is actually made easier through the introduction and the first chapter. The introduction describes simply and effectively the pattern that the editors decided to follow in organizing the book. The essence of each contribution is summarized and related to the precedent or the following ones. The first chapter offers another survey through a lateral organization of topics. I find however that this second trial is less successful than the first in bringing out the issues, arguments and facts in a meaningful way. The real content-transmitting device is the introduction. Order is not in itself a guarantee of consistent structure. I have decided nevertheless

to try and summarize the volume from both perspectives. I shall introduce the volume as a whole on the basis of the topics raised in it, but I will try to do so through a simple and natural rationale in the area of diffusion, i.e., a micro-macro ordering. I shall not try therefore to report briefly on each of the contributions, but rather focus on the overall consistency, mentioning individual contributions. I shall conclude briefly afterwards.

Diffusion of innovations is basically a socio-economic subject, not because it is more mature or fashionable to be multi-disciplinary or open minded but because the innovation adoption decision depends on the perceptions of others. The simplest analytical form, or model, is already multi-disciplinary as it involves from the beginning both a traditional economic cost-benefit argument and a representation of agents' interactivity and its influence on individual decision-making. It is from this perspective that the research in the field is both rich and complicated. On this fundamentally hybrid ground one must add the fact that diffusion as such occurs within the larger scope of social, political and societal phenomena, and hence stimulates the interests of most research groups in social sciences. The issue actually goes far beyond this; natural scientists are often involved, as they were at the conference to which this book relates, in the representation and analysis of diffusion combining quantitative magnitudes and qualitative change. It is certainly not to readers of this journal that I need to recall the basic biological-rooted evolutionary mechanisms that sustain our overall grasp of this field of research. From this standpoint I find the book truly instructive; the multidimensional analysis it offers from many distinct perspectives melts into a consistent broader picture.

Analytically speaking, the volume is very comprehensive. It incorporates practically every aspect of diffusion the social researcher can think of. However, being a collection of contributions, it does so in particular intersections. I find it more useful to describe in a different way the analytical dimensions it contains rather than to explain the particular intersections that the reader will easily locate.

Diffusion theories are a typical example of a resarch field that adopted naturally a micro-foundations-to-macroeconomics approach (or to the macro-system as a whole), and it is convenient to follow its analytical trajectory in the same logic. The first diffusion models (not presented as such in this research volume) deal with innovation on a quasi-static basis, that is, following the adoption behavior of potential adopters without taking any real account of the qualitative changes that the innovation itself, as well as its milieu, undergo. Although cost-benefit considerations were roughly integrated in early modelling, and an epidemiological interaction dynamics was presented to explain imitation behavior in the aggregate of potential users, no real learning took place. Homogeneous users simply reacted to the competitive stimuli of early adopters. However, the industry is almost never made of similar agents, so that agent heterogeneity in terms of industrial structure and behavioral rules is an important characteristic of the diffusion analysis. It is also a strong feature of some of the contributions here (see *Maryellen Kelley*'s and *Harvey Brooks'* chapter on the influence of size in adopting programmable automation as well as the chapters by *Giovanni Dosi* and by *Charlie Karlsson* on behavioral diversity).

The next point involves the learning mechanisms employed. From a pure decision-making perspective, learning is first of all an uncertainty-reducing device. A particularly interesting point is made by *Sergei Glaziev* and *Yuri Kaniovski*, who focus on the uncertainty treatment during the early phase of diffusion before stabilization or standardization. This whole issue of the diversity, uncertainty and stabilization process of learning is one of the most difficult problems in the analysis of diffusion. More generally, the issue of learning, whether technological or informational, is central to the basic mechanism of diffusion. We have various treatments of information flaws, screening and decision principles in this volume. Learning processes have also very concrete and practical dimensions. *Diederen et al.* show the gradual process of skill adaptation in banking services in Denmark, while *Jonny Hjelm* studies an analogous adoption of innovation in Swedish forestry. Very often skill-building and professional education determine diffusion rates in as much as they reflect the ability of the social infrastructure to face and integrate novelties.

An interesting analysis is offered in this volume in terms of the impact of relative prices on diffusion. A new good does not enter in a vacuum; one often faces a quite rich competitive dynamics of substitution and complementarity through which the innovation becomes a part of industrial life. This is the case with *John Tilton's* chapter on material substitution in the beverage industry, and with *Robert Ayres'* and *Ike Ezekoye*'s analysis of antiknock additives. This type

of relative prices argument is best differentiated then by *Arnulf Grübler* both as a function of the stage in diffusion and from an inter-generational view of diffusion clusters.

A further point is to recognize that the diffusion process is not only rational; it is basically an evolutionary process where the innovation itself is being transformed during the diffusion (as do the absorbing firm and the specific industry and environment). The evolutionary nature of innovation diffusion is at the heart of *Jerry Silverberg's* chapter. Here we move towards a view that learning becomes more and more a collective process whose features are determined in a more organic fashion through self organization of individual components and the impact of historical (path dependency) determination. Such a view also underlines Dosi's contribution, although emphasis is much stronger on equilibrium/disequilibrium perspectives and technology/institutional interaction as a source of different innovation dynamics. I find that these two contributions are, analytically speaking, the backbone of the volume. As far as the structural transformation of the environment is concerned, two of the contributions offer complementary examples. On the one hand, *Dominique Foray* and *Arnulf Grübler* describe very well the "locus" of innovation diffusion in the morphology of the absorbing sector through a France-Germany comparison in ferrous casting. On the other hand, *Dirk-Jan Kamann* and *Peter Nijkamp* find a more macro-system structuralist view in which diversity is really multi-dimensional and encompasses, in addition to agents behaviour, an industrial sector and traditional performance perspective, a tentative integration of spatial economics with urban and regional aspects, together with hierarchies and network considerations.

The network structure is something we should consider very seriously in the diffusion literature. It is an industrial structure whose primary function is to ensure innovation diffusion through social sharing of know-how capital and through internalization of externalities. This is done by sharing R&D costs or by exchanging information. Networks are based on partnerships, joint ventures and other forms of cooperative agreements. As information technologies progress, this form of partly competitive and partly cooperative structure may become dominant far beyond high-tech sectors. Cooperative agreements are analyzed from different perspectives in this volume. User-producer interaction (see in particular the supply-demand interdependence in *Ove Gransstrand's* chapter which rejoins the evolutionary trend mentioned before), collective learning and information exchange figure throughout the book in various settings. Yet network dynamics as such is not tackled except in *Gerhard Rosegger's* chapter, where inter-firm cooperation appears as a key factor for successful technology transfer, and in the *Kelley* and *Brooks* chapter, where the intensity of network linkages is shown to positively influence adoption rates among small firms. In my view, at least, network dynamics is the new way to deal with interactiveness, at the heart of innovation diffusion. More generally, diversity fulfills an important evolutionary role: if indeed the product life cycles are becoming increasingly shorter (as in the case with computers in *Modis'* and *Debecker's* discussion of the service sector) and the pre-paradigmatic stage of technology development becomes longer, it means that a larger proportion of the innovative activities is spent in search and learning activities rather than in scale-related activities and the diffusion of standardized goods. Thus interaction analysis is something that acquires a growing importance, and for which the scope of progress is still largely open.

The next step in the gradual development from reduced-form micro-diffusion analysis to a more complex patterns lies in the recognition that diffusion processes maintain relationships with one another and are certainly not an isolated reality. Techno-economic systems and paradigms are articulated around major configurations of key technologies. Infrastructural technologies often drive the overall development process, shifting from the train and electricity to the automobile and modern information technologies. This driving force of infrastructure is particularly well analyzed in *Sergei Glaziev's* contribution and that of *Nebojsa Nakicenovic*. In the former, the emphasis is put on the interrelatedness of different types of innovation (infrastructural, technological and organizational); in the latter some application to the international pattern of development is applied and huge clusters of diffusion processes representing the Kondratieff cycles are used as tools for the comparative analysis of lagging, catching-up and the like. I find that the international dimension should have received more emphasis, especially in the light of the recent integration of the new growth theories and international trade.

The contributions in this book go beyond the structural transformation context. European integration perspective is discussed by *William Pierce* from a technology policy standpoint. Diffusion policies as such are more problematic than the traditional non-interventionist tradi-

tion, but other countries in the EEC certainly push towards stronger positions in terms of industrial and technology policy. This is naturally beyond the traditional support for R&D where government intervention is viewed as a palliative to market failure in the allocation of resources to innovative activities. Pierce analyzes both traditional procurement policies as well as regulatory powers in matters such as harmonization of standards in matters of safety, the environment etc. and I appreciated his straightforward approach. He did not tackle strategic coordination of the Japanese type, but perhaps this is a bit too far for a volume which is focused on diffusion. I also enjoyed *George Modelski*'s and *Gardner Perry*'s analysis of democracy as a diffusion process. This very long process with 100 year spans confirms our intuition that democracy is also a learning process. It is difficult to appreciate how much can be said accurately in such a long-run political realm, but the view that "Democracy is one of a family of global, collective evolutionary process" should give us some hope that values have some irreversibilities associated with their social learning.

I did not survey the other topics in the book such as the contribution on marketing by *Vijay Mahajan et al.* or the one on the AIDS epidemics by *Emilio Cassetti* and *Cindy Fan*, that send us back to the epidemiological foundations of the discipline. My micro-to-macro survey was necessarily non-exhaustive and partial as I am afraid one necessarily has to be with a volume of contributions of such breadth and depth. Again, it is not always easy reading when the foucs is constantly shifting, but I certainly enjoyed my investment in time spent reading, and whole-heartedly recommend it to every researcher in the area of innovation in the social sciences and beyond. I should add that for advanced students and researchers I find it also well equipped in terms of resources, directions and instructive reference lists. We should all congratulate *Nebojsa Nakicenovic* and *Arnulf Grübler* for the fine collection of contributions in the area of the economic and social analysis of diffusion.

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