UNIVERSITA' DEGLI STUDI DI TRENTO - DIPARTIMENTO DI ECONOMIA

# ECONOMICS AND THE COMPLEXITY VISION: CHIMERICAL PARTNERS OR ELYSIAN ADVENTURERS?

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# Economics and the Complexity Vision: Chimerical Partners or Elysian Adventurers? Part I[1]

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[1]This work began as a review article of: *Complexity and the History of Economic Thought*, edited by David Colander, Routledge, London,UK, 2000; & *The Complexity Vision and the Teaching of Economics*, edited by David Colander, Edward Elgar, Cheltenham, UK, 2000. It has, in the writing, developed into my own vision of complexity economics. I am deeply indebted to my friend, colleague and fellow Salta-Zambaist, Nico Garrido, for invaluable help in preparing this paper. Alas, he is not responsible for the inevitable infelicities that remain.

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## 1 Introduction

> 'If, then, it is true that the axiomatic basis of theoretical physics cannot be extracted from experience but must be freely invented, can we ever hope to find the right way? ... I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it. Our experience hitherto justifies us in believing that *nature is the realisation of the simplest conceivable mathematical ideas*. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena.'
>
> Einstein in his *Herbert Spencer Lecture: On the Methods of Theoretical Physics*, given at the University of Oxford on 10 June 1933, quoted in: [69], pp.136-7; italics added.

I begin with this apparently paradoxical statement by arguably the greatest natural scientist of the 20th century for two reasons. I interpret it, firstly, as a salutation to *Ockham's Razor*; and secondly as a paradox that has to be confronted by complexity theorists of any variety. A principle that unites every kind of complexity theorist,and they are a richly varied class (see §3 and §5, below), is that observable 'reality' pertaining to any field, physics, biology, chemistry, applied mathematics, economics, etc., is *complex* but this complexity emanates from *simple* building blocks - of concepts, methods and rules of interaction. Why, then, should this supreme scientist, of powerful intuitions, claim that *nature is the realisation of the simplest conceivable mathematical ideas*? Is it because even the 'simplest conceivable mathematical ideas', when realised in natural phenomena, become enveloped in complex manifestations and it is the task of the theorist to disentangle the apparent complexities and bare the hidden simplicities underpinned by, and in, simple laws and concepts? Such an interpretation would be welcomed by the complexity theorist who is in the habit of showing how even unbelievably simple mechanisms are sufficient to demonstrate and encapsulate the complexity of phenomena in the natural, physical, biological, social and other phenomenological worlds.

But, for me at least, a paradox remains: why should simple mathematical ideas be manifested as complex natural phenomena? Why is it not the case that the simplicity is not carried over, uniformly, to nature's manifestations, too? And if it does not, as the complexity theorists are wont to point out and, indeed, use as their starting point and as a justification for their various disciplines, at what point does the transformation from simplicity of the mathematical ideas to the complexity of natural phenomena take place and can it be determined? I will have my own answer to this question when I discuss computational complexity theory, a variety of complexity theory hardly touched in any of the papers of the two volumes being reviewed here.

So much is being written about complex systems that one tends to forget that one of the guiding principles of generating complex dynamics has been to use simple building blocks. In a sense the paradox of complex systems and complex dynamics is that they emerge, endogenously, from simple interactions between simple entities - and the simplicity in the last two senses are, usually, intuitively obvious, requiring almost no definitions of any sort. However, the more usual tenet of simplicity is, of course, some version of *Ockham's Razor*. It is well to bear in mind this tenet, or some equivalent version of it, at least tacitly, when reading this essay.

When the Review Editor, through the good offices of one of the Editors, approached me almost one year ago about writing a Review Article of these two books, I agreed almost with alacrity. I expected that my evaluation of the contents and structure of the books would be largely positive, given my own intellectual background and my current research agenda, and that I would, in the process of writing it, learn something about the frontiers of the subject. I did not expect the contents to present new breakthroughs in conceptual understanding of any aspect of complexity theory; nor did I expect the books to contain new techniques for analysing complex systems or new theories of complexity. I did expect, however, new perspectives on teaching some aspects of complex systems or complexity theory to economists and, perhaps, also new ways of incorporating complexity themes in standard or orthodox economic theories. I thought also that there may well be new perspectives and evaluations of past theories and theorists, especially when viewed from a complexity theoretic point of view (whatever that may turn out to be, which I also expected the books to clarify or, at least, classify in some useful way).

But now, almost one difficult year later, the final product of my painful and sad struggle through these two books might appear in a largely negative form. It is not that I am, in principle, opposed to or sceptical about a complexity vision for economic theory; nor am I against a complexity vision to reinterpret aspects of the history of economic thought and theory. For example Duncan Foley's recent book, [27], is an excellent example of what I find encouraging and interesting from this latter genre of work. It is simply that the contents of the books under review here are less than careful in their doctrine-historical research, their mathematical underpinnings and conceptual clarifications. Even when they venture out of these narrow domains and explore or invoke images and metaphors from, for example, the worlds of art, mathematical logic and applied mathematics, the claims are so preposterous that my initial enthusiasms and natural empathy were gradually eroded and I felt that these works do a disservice to those of us who would like to promote a complexity vision for, and of, economic theory, applied economics and the history of economic thought.

*Emergence*, *order*, *self-organisation*, *turbulence*, *induction*, *evolution*, *criticality*, *adaptive*, *non-linear*, *non-equilibrium* are some of the words that characterise the conceptual underpinnings of the 'new' *sciences of complexity* that seem to pervade some of the frontiers in the natural, social and even the human sciences. Not since the heyday of *Cybernetics* and the more recent brief-lived ebullience of *chaos* applied to a theory of everything and by all and sundry, has a concept become so prevalent and pervasive in almost all fields, from Physics to Economics, from Biology to Sociology, from Computer Science to Philosophy as *Complexity* seems to have become. An entire Institution, with high-powered scientists in many of the above fields, including several Nobel Laureates from across the disciplinary boundaries as key permanent or visiting members, has come into existence with the specific purpose of promoting the *Sciences of Complexity*[1]

I have found Duncan Foley's excellent characterisation of the *objects* of study by the 'sciences of complexity' in [27], p.2, extremely helpful in providing a base from which to approach the study of a subject that is technically

[1] I am referring, of course, to the *Santa Fe Institute*, which, refreshingly, has thought it prudent to have a permanent Economics division from the outset. In some senses the books being reviewed can almost claim to be manifestos of the Santa Fe approach to the study of complexity and its application to economic theory and applied economics. But, on a sceptical note, the almost untrammelled enthusiasm for a unified vision for all of the disciplines has the danger, in my opinion, of making essentially moral, human and social sciences like economics handmaidens to the concepts and methods of physics and, in this sense, we seem to be travelling along well trodden paths of the past. Vico's famous dictum keeps coming back to haunt my mind: *'Corsi e ricorsi ...*! I think the reader will detect, in this essay, not only between lines, an exposition of this scepticism. However, there are also refreshingly critical notes on this 'Santa Fe enthusiasm' by several of the authors in these volumes. I have in mind, in particular, the chapter by Montogomery in [17] and those by Foley and Pryor in [18]

demanding, conceptually multi-faceted and philosophically and epistemologically highly inhomogeneous:

> Complexity theory represents an ambitious effort to analyse the functioning of highly organized but decentralized systems composed of very large numbers of individual components. The basic processes of life, involving the chemical interactions of thousands of proteins, the living cell, which localizes and organizes these processes, the human brain in which thousands of cells interact to maintain consciousness, ecological systems arising from the interaction of thousands of species, the processes of biological evolution from which *new species emerges*, and *the capitalist economy, which arises from the interaction of millions of human individuals, each of them already a complex entity, are leading examples.* ([27], p.2; italics added.)

It is one thing to observe similarities at a phenomenological and structural level. It is quite another to claim that one 'science', with its own characteristic set of methods, can encapsulate their study in a uniform way, thus providing rationale for an interdisciplinary approach to all of them. Here again, I believe the elegant attempt to go just below the surface similarities of phenomena and structure, and define the conceptual and methodological underpinnings of this new 'science', in [27], is most illuminating to anyone trying to make sense of 31 different chapters by 23 different authors in [17] and [18], the books being reviewed here:

> What these [highly organized but decentralized] systems share are a **potential** to configure their component parts in an astronomically large number of ways (they are *complex*), constant change in response to environmental stimulus and their own development (they are *adaptive*), a strong **tendency** to achieve recognizable, stable patterns in their configuration (they are *self-organising*), and an avoidance of stable, **self-reproducing** states (they are *non-equilibrium systems*). The task complexity science sets itself is the **exploration of the general properties of complex, adaptive, self-organizing, non-equilibrium systems**.
>
> The *methods* of complex systems theory are highly empirical and inductive. ... A characteristic of these ... complex systems is that their components and rules of interactions are *non-linear* ....
> **The computer plays a critical role in this research**,

> because it becomes impossible to say much directly about the dynamics of non-linear systems with a large number of degrees of freedom using **classical mathematical analytical methods**. ([27], p.2; bold emphasis added.)

Note, however, that the discourse is about *potentials* and *tendencies* and, therefore, in an economic context, but not only in it, there could be scope for *design* or *policies*. Moreover, the 'avoidance of stable, self-reproducing states' is an indictment against mechanical growth theories, of a macroeconomic sort, with their uninteresting, stable, attractors.

Thus, it is almost natural that economists are enthusiastic about, and take almost like a duck to water, to the methods and conceptual schemes of the 'sciences of complexity', particularly to its *Santa Fe versions*, although not uncritically. Some words of caution about the general contents of the books, against the backdrop provided by the 'Foley characterisations', as I shall call my understanding of the subject, objects and methods of the complexity sciences, are probably appropriate at this point. None of the methods in any of the essays, in either of the volumes, go beyond the use of the analytical methods of the classical mathematics of real analysis, except for a brief discussion of non-standard analysis in Levy's chapter[2] in [17]. In addition, although much is made of simulations and their fertility, no non-formal or non-Bourbakian[3] methods are invoked in any serious way in any of the exercises. Indeed, I suspect, and I think I can easily demonstrate, that all of the serious mathematical results implicitly invoked belong to the class of 'classical analytical mathematical methods'. This applies, in particular to the proof of the results reported, for example, in [6], which are liberally invoked whenever appeal is made to 'urn models'. Now, these methods are devoid of numerical content and their use in simulation models must involve some kind of black magic - i.e., leap of non-analytical faith - at some point in the exercise. I am not

[2]A perplexing and somewhat unsatisfactory discussion, given that the author lists himself as an 'Erdös-4' (the umlaut on the 'o' is missing in the book version! How Erdös would writhe at such an imprecision!). For the uninitiated in such symbolisms I may add that, for example, an author with an 'Erdös-1' signifies a *mathematician* who has published a paper with Paul Erdös; for an 'Erdös-2', a mathematician must have published with someone who has published with Paul Erdös; and so on. So, I presume, David Levy is a mathematician who has published with a mathematician who has published with a mathematician who has published with . . . .

[3]There is one, gratuitous, mention of 'Bourbakism' in Hoover's chapter in [18], p.191. But this is an uninformed and dangerous mention. I suspect that Hoover does not know the difference between Bourbakism and Formalism for, if not, he would not have made the remark he makes. von Neumann was no Bourbakian and, late in life, even began to doubt his formalistic credentials.

against black magic, alchemy and such things, when practised in the privacy of individual laboratories; but when such things are invoked to substantiate arguments for and against policy and the like, then I reach my limits of tolerance. Moreover, although much is made of induction and simulation, there is not a single serious mention, let alone a discussion, of either recursion theory or algorithmic complexity theory. This means a whole branch of constructive, non-classical, theory of induction is totally neglected.

It is not surprising that the bug has spread far into the economist's domain, both in pure theory and in its several applied wings. The attempt, therefore, by an Editor sympathetic to new currents of thought, to bring together specialists, competent researchers and, probably, enthusiastic amateurs to evaluate and propagate a 'Complexity Vision' in the teaching of economics and a retrospective evaluation of the contribution of our great predecessors is to be welcomed by all open-minded economists. I am certainly one of those who welcome this effort.

It is particularly to be welcomed given that our pedigrees, as economists, for the conceptual underpinnings of the so-called ' sciences of complexity' are impeccable. The stem *nom*, in eco*nom*y, from the Greek $\nu\o\mu\o\varsigma$, sugggests not simply *order*, but its realisation as well[4]. Hence, the idea of *self-organized order* sits well - or at least *should* fit like the proverbial glove - in the economist's repertoire of conceptual *hilfenkonstruktions*. Some would immediately recall the idea of the *invisible hand* as a device for the *realisation of an order*, perhaps even replacing the indefinite article with the definite one. But others, brought up on Hayekian writings, would feel entirely comfortable with this as a starting point for economic analysis rather than *catallactics* and other fancy or less fancy words, even those stemming from Xenophon's or Aristotelian writings.

As for the more evocative term *emergence*, perhaps also stemming from ideas of the pre-Socratic, Vedic and Mayan speculative philosophers on the idea of 'wholes being greater than the constituent parts', its modern connotations, especially as used in the sciences of complexity, stems from *Problems of Life and Mind* by G.H.Lewes, where he distinguished between *resultants* and *emergents*. But I would go slightly further back, to Mill's *Logic*, severely maligned by Jevons in his anti-Millian and anti-inductive phases, where he discusses

[4]Hayek's interpretation of $\nu\o\mu\o\varsigma$ may also be pertinent in this context:'By *nomos* we shall describe a universal rule of just conduct applying to an unknown number of future instances and equally to all persons in the objective circumstances described by the rule, irrespective of the effects which observance of the rule will produce in a particular situation. ... Such rules are generally described as 'abstract' and are independent of individual ends. **They lead to the formation of an equally abstract and end-independent spontaneous order** or *cosmos*'([35], p.15; bold emphasis added)

so-called *heteropathic laws* in causality (System of Logic, Bk.III, Ch. VI, §2). This is particularly relevant in G.H.Lewes's distinction between *resultants* and *emergents*, where the former signifies processes whose individual or singular steps, say in an algorithmic sense, are clearly identifiable and reproducible in the production of a *resultant* phenomenon; contrariwise, in the production of an *emergent phenomenon*, such algorithmic identification is impossible. But these ideas, until resurrected by C.Loyd Morgon, in the 1920s, in his *Emergent Evolution*, which may well merit the honour of being the *the* classic of the sciences of complexity, remained buried in arcane philosophical and logical circles.

It is to Osborne Reynolds that the honour of conceptualising and quantifying - experimentally and numerically - the idea and phenomenon of *turbulence* in fluid motion, belongs. But in recent lectures Benoit Mandelbrot has suggested that the *idea* of *turbulence* to encapsulate non-laminar fluid flow was borrowed by Reynolds from the economic writings and phenomena of the period. I have myself not been able to trace any reference to economic writings in the more classic works by Reynolds but Mandelbrot's suggestion is, of course, highly plausible given that Macauly had made the term an object of current discourse in educated circles with his references to 'turbulence in the London markets'.

The rest of this essay is organised as follows. In the next section, titled 'A Cautionary Tale - or Two', I begin the main business of reviewing the books with two unfortunate examples of inappropriate and unenlightening metaphors for 'complexifying' economic theory. I would not have begun in this negative fashion if not for the fact that the two examples are both dangerously misleading and, in a certain precise sense, symptomatic of the careless scholarship that, unfortunately, pervades the contents of many - even most - of the 31 essays in the two books. If the reader survives a reading of the next section, then in §3 I present the first half of a synopsis of 'Varieties of Complexity Theory'. This section is presented with two purposes in mind: one, to indicate the nature of the world of theoretical complexity sciences; two, the nature of the restricted 'complexity visions' invoked in the essays in these two books. Perhaps I reveal an ulterior motive in presenting this kind of synopsis, but it is, in fact, not so; I simply want to point out, retrospectively, that the authors are *underselling* the potentialities and possibilities of a richer and more measured variety of the science of complexity than that purveyed by the 'Santa Fe enthusiasts.

In §3 I present the elementary background to two traditions of complexity analysis. I call them the von Neumann and Turing Mark I traditions, respectively. The former is the *cellular automata* tradition; the latter is a *linear, bifurcation, dynamical systems* tradition. Neither of these traditions are explicitly dealt with in any of the papers in the books, although there are implicit and marginal references to them in a few places. But no serious discussion or analysis is possible without an understanding of these two - and two other - traditions. The other two are presented in the first section, $5, of Part II.

In $4 I discuss, critically - but, hopefully in a constructive spirit, given that I am myself an enthusiast of the 'complexity vision' for and in economic theory - the second of the two books, [18]. In Part II, $6, a similar exercise for the first of the two books, [17], is presented.

In §7, I try to evaluate, on the basis of the results and critiques of the previous sections, whether the case for the prosecution, so to speak, or for the defence, respectively, are substantiated or not. I think I come down in favour of the case for the prosecution, in spite of having begun as a counsel for the defence. My reasons are detailed and discussed in §6.

Finally, I return to the grand themes of methodology, epistemology and philosophy, in the concluding, §8. In spite of coming down in favour of the case for the prosecution, I am certain that there is a sound case to be made for the defence. It is just that such a case was not successfully defended by the counsel for the defence, as represented in these two particular books. How, had I been a counsel for the defence, such a case would have been presented and defended is the main subject matter for the concluding section. But it is also about how a future can be imagined, for a complexity vision for economic theory, based on theoretical technologies, conceptual innovations and the work of our masters and the giants on whose shoulders we stand - economic, philosophical and mathematical - that may provide an alternative to the arid orthodoxies of the present.

The mathematical appendices go into greater detail on several of the concepts and tools liberally mentioned in the main texts of Part II & II.

# 2 A Cautionary Tale - or Two

> I'm trying to understand what's happening here [Mondrian to Peggy Guggenheim, ca. April, 1943, on Jackson Pollock's *Stenographic Figure*]. I think this is the most interesting work work I've seen so far in America ... You must watch this man. ...
> Everybody assumes that I am interested only in what I do in my work,... [but] there are so many things in life and in art that can and should be respected. [65], pp.444-5.

I begin this section in doubly unconventional ways, as if it is an amateur note on modern art and to make a confession of sorts, declaring - with a warning to any potential reader - that a largely negative evaluation will be presented of the unguarded metaphorical adventures in many parts of these two books. I should like to substantiate this particular point at the very outset, with a couple of unusual examples of infelicitous and even dangerous metaphors - especially since I embarked upon the task of reviewing the books with considerable enthusiasm and empathy. The first of the 'dangerous' and highly misleading analogies will be on Mondrian and Klee as providing metaphors to be shunned and emulated, respectively, by economists. The second is the use of the fertile and elegant, but artificially constructed, Rössler system as a basis for discussing chaos in complex (economic) dynamical systems.

## 2.1 Mondrian and Klee - Pitfalls of Superficial Analogies

At a very superficial level and viewing, in particular of the originals of *Stenographic Figure* by Pollock (completed in the last three months of 1942) and, say, *Broadway Boogie Woogie* by Mondrian from about the same period - the one at the beginning of a revolutionary life in art and the other at the end of one (Mondrian died in 1944) - have little in common except, perhaps, some of the colours. But Mondrian and Pollack had much more in common than at this superficial level, particularly of a deeper, philosophical underpinning in that they had both been deeply influenced by various aspects of *theosophy*. In Mondrian's case, among the meagre possessions at the time of his death, was the membership card of the Theosophical Society, obtained in 1909. The concept of *evolution* that his readings of Helena Petrovna Blavatsky had given him, a variation on Hindu themes of a similar sort, remained influential in his own evolution towards pure abstractionism. As Carel Blotcamp says in a majestically detailed work on Mondrian, appropriately sub-titled *The Art of Destruction*, [11], p.15 (italics added):

> What he did borrow from theosophical sources is the firm conviction that all life is directed towards evolution, and that ... it is the goal of art to give expression to that evolution. To Mondrian evolution was 'everything'. ... In order to understand this, we must take into account that in Mondrian's thinking evolution was closely bound up with *destruction*. He did not view this as a negative concept: on the contrary, the destruction of old forms was a condition for the creation of new, higher forms.[5]

[5] Coming from a Shaivite background from old Ceylon, my first encounters with Mondrian

What this brought to mind, when I was wearing my economics hat and remembering the lessons learned at the feet of Richard Goodwin who, in his turn, had imbibed his own many and wonderful lessons directly from Schumpeter, was the concept of *creative destruction*, much maligned, these days, at the hands of endogenous growth theorists but, nevertheless, a concept of much potential felicity for economists with complexity theoretic sympathies.[6]

Imagine, then, my surprise, when I read in the Editor's *Introduction*,[18],p.15, with an obviously warm nod of approval, that:

> Peter Matthews (in Ch.15) suggests that the current teaching of statistics and econometrics is simple and beautiful, with everything in its place, much like a Mondrian painting. And that is the problem. The real world is complicated and what we teach and the real world do not relate very well. He suggests that 'complexified econometrics' will be more like a Paul Klee painting. It will directly confront multiple equilibria in which macrostructure reflects feedback and non-linearities.

My critical antennae were immediately aroused[7] as I wondered in what sense, if any, was 'the current teaching of statistics and econometrics simple

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paintings and, subsequently investing in a Gerrit Rietveld 'Red, Yellow and Blue' Chair, were presaged by a reading of the philosophy, and a natural sympathy with it, that underpinned their works. Shiva, after all, is the Hindu God of *Destruction*, but all the while accompanied by the other two of the triumvirate: Brahma, the *Creator* and Vishnu, the *Preserver*. Incidentally, it may be useful to point out also that 'Mondrian's ... adherence to the three primary colours - red, yellow and blue - owes much to Goethe's colour theories ....' [30], p.15. To complexity theorists in particular, such an adherence should be of particular relevance given that Goethe's view was that light was one and indivisible and cannot or should not be explained by any theory of particles, in grand opposition to Newton. A somewhat similar attitude to lines and planes was adopted by Peirce,[72] who is also mentioned, at times inappropriately, in the books being reviewed.

[6] I recall a particularly amusing incident at one of the *Villa Mondragoni* conferences of a few years ago, to which I was also invited. After a technical talk by Philippe Aghion, full of references to Schumpeter and *creative destruction*, Richard Nelson got up and remarked, quietly but with characteristic firmness: 'You know, some of us have actually read Schumpeter and find it difficult to recognise it in your references.'

[7] My first reaction, given that I live most of the time in Ireland and it is the fiftieth anniversary of the first performance of *Waiting for Godot*, in Paris on January 5, 1953, was to imagine a philistine comparing *Finnegans Wake* and *Waiting for Godot* and coming to the conclusion that since there is so much talking and action in the former it is more like the 'real world'. The exquisite richness of the world of metaphor and its varied possibilities, imagined and real, depicted by Beckett on an empty stage with a tree and two figures and a lot of 'waiting', changed theatre forever, as Mondrian did with his unruly lines and primary colours. As I read on, the stimuli that aroused my critical antennae proved real. Naturally, nothing in my reaction had anything to do with a diminished appreciation of Joyce and his supreme

and beautiful'. Was it the noble art of teaching itself or was it about the contents of these subjects that imparted a sense of beauty and simplicity, I wondered. I had often taught econometrics starting from the projection theorems, the Hahn-Banach theorem, and Duality principles as the fundamental bases on which to define optimization operators. In this way I could avoid the traditional divorcing of the theoretical technology of theory from its applied wing. This, surely, could not be what the authors meant, since that kind of teaching required a certain amount of mathematical maturity to understand how general principles, derived on the basis of criteria of simplicity and elegance, were used to make the bread-and-butter techniques workable. More importantly I was curious to find out whether formal criteria of 'simplicity', say on Ockhamian grounds, and 'beauty', say along the lines once pioneered by George Birkhoff, were being invoked.

I turned, therefore, immediately to chapter 15 and found no formal criteria but intuitive and mysterious references to the Rietveld '*Red and Blue Chair*' and the preposterous assertion that the 'small, handmade bookcase intended to evoke' the Rietveld Chair reminded the author of the 'traditional (liberal arts) courses - simple, beautiful and like the 'L' in BLUE, *linear*'. [18], p.231; (second italics added). Anyone even reasonably acquainted with the geometry of dynamical systems, and some imagination, would attribute to the geometry of 'L' in BLUE, or wherever else it may be found, *piecewise linearity*, which is for all practical, analytical and computational purposes *non-linear*. As for the simplicity and beauty of 'traditional liberal arts courses', I can only comment that my own experiences of two years of such courses was the exact opposite. It was only at higher levels and at greater levels of maturity that ideas of simplicity and beauty in an intellectual endeavour, particularly of theoretical technologies, came to be realised, appreciated and, at the stage of individual research, even as a guiding principle.

The incredible - I nearly said 'incredulous' - analogies do not even begin to end at that point. Matthews goes on:

> Still, if there is to be a 'new econometrics', its character will be better reflected in another piece of 'art' in our house, an exhibition poster of Paul Klee's *Unstable Equilibrium* (1922). The picture resonates with most economists, even if it was Klee's interests in the hard, not soft, sciences that inspired it: there are the familiar 'arrows of motion' for example, that with one curious exception radiate outward - not from a point, however, but from

success in subverting the English language. 'Subversion' of orthodoxy and 'innovation' to replace it are, surely the twin tasks of the economist with a complexity vision!

> some vertical axis. There is also a small(ish) white border that (or so I interpret it) somehow contains explosiveness, and which therefore hints at the existence of some non-linear 'border dynamics'. Klee's world, and the world the econometricians must now confront, is one in which there is more than one equilibrium - some of which are stable, and some which are not - and in which 'macrostructure' often reflects the existence of some sort of 'feedback' and/or other non-linearities.

First of all, the naming of this painting in the original German was: *Schwankendes Gleichgewicht*. Whether *Schwankend* can be translated as *Unstable* or something else is almost impossible to answer. I tried the word, in the context of a 'Klee discussion' with two German friends, one a philosopher (of Wittgensteinian background) and another, a philologist. The former came up with the translation for it as *wavering*; the latter's version was *swaying*[8]. In a widely available French language 'coffee table' publication the painting is reproduced and referred to on two different pages (pp. 24 and 34, in [50]) as *Équilibre vacillant* and *Équilibre instable*, respectively ! Secondly, were the 'arrows' those of 'motion' or should we recall that:

> It was Klee's great merit to realize at once that Braque's 'inventions', such as the insertions of letters or numbers in a picture, have no other scope than a poetic one. So too with the newspaper cuttings and the coloured paper in the 'papiers collés'. As he was taken by Braque's letters so he was by *De Chirico's arrows*, Delaunay's little crosses and windows, and the doors of the 'metaphysical' painters.[83],p.97; italics added.

So, could they not have simply been *De Chirico's arrows* rather than isoclines in a vector field? Klee was, at the time of completing *Schwankendes Gleichgewicht*, lecturing at the Bauhaus in Weimar and his lecture notes of the period do contain vectors appropriately 'arrowed'. But the inspiration came

[8] His letter to me expressed it as follows: 'The best translation for Klee's painting that I can come up with is "Swaying Equilibrium"; this best captures the motion expressed by "Schwankend":a kind of slow, swinging, oscillating movement like the one performed by a metronome's pendulum in its slowest pace; only that with "Schwankend" the impetus comes from outside, not from inside the machine. ...Klee's German title evokes the picture of a balancing act.' This is not a surprising translation if one keeps in mind the paintings *Die Zwitscher-Maschine* of the same year as *Schwankendes Gleichgewicht* and, even more appropriately, *Der Seiltänzer* of a year later, 1923. Indeed, all these evocations are quite compatible with the kind of *linear* mathematics on which Klee relied for his analysis, via rhythms, of the link between music and art.

not from the so-called 'hard sciences', as Matthews alleges, but from a theory of form with a view to study the interrelationship between music and art using rhythm (cf.[23], esp. pp. 33-64). Contrary to Mondrian's evolutionary, holistic view and vision, Klee, at least in his Bauhaus period, was being a reductionist. Moreover, the arrows, whether of the *De Chirico* inspired variety or the 'arrows of motion' of Matthews, appear in the Klee *oeuvre* only for a brief subperiod of the Bauhaus sojourn (for example: *Betroffener ort* and *Scheidung Abends*, both from 1922, the same year as *Schwankendens gleichgewicht*).

But Klee's paintings did not rely on 'arrows of motion' to depict movements of varying subtleties. He used, like Mondrian, LINES with a capital and piecewise linear 'L' to achieve subtle depictions of varying motion. They may or may not have signified instabilities or disequilibria; evolutionary creation or 'creative destruction' - as they did in the case of Mondrian. To resort to facile and tenuous analogies and make a mathematical case to economists - or anyone else, for that matter - one must, surely, have a thorough grasp of the field one is appealing to and to the relevant area of mathematics. In all of these senses, these analogies by Matthews, uncritically accepted by the Editor, does a great disservice to Mondrian, the incorrigible evolutionist and holist, to Klee, the musical artist (and much else besides) searching for means to depict the common rhythms in both. There is nothing intrinsically non-linear underpinning *Schwankendes Gleichgewicht*; there is everything creatively evolutionary and holistic in Mondrian's lines and colours in his later purely abstract phases. A knowledgeable scholar, more careful in his analogies, would have reversed the parallels for the economist aiming for a complexity vision.

### 2.2 Screwed up - and down - Chaos

The next infelicitous example is from the first of the books being reviewed here - a book devoted to endowing the 'History of Economic Thought' with a complexity vision, [17]. In the very first section of the first chapter of the lead essay in [17], James Wible tries to discuss, from a loose description of the Rössler System in Nicolis and Prigogine [68], concepts of chaos in complex dynamical system. He repeats, uncritically and without reflection - let alone after experimental simulations - the alleged Rössler System and its properties as given and discussed in [68], pp124-6, prefacing it by the following:

> One very simple model of chaos, the Rössler model, gives rise to unpatterned rotational chaos. This model describes the movement of a particle in three dimensions around an unstable focus using three dynamic equations. Two of the equations are

> linear and the third is non-linear. The variable rotational path of the equations of evolution of the system means that the behavior of the system is non-periodic. Furthermore, the type of chaos depends on the initial conditions of the system. If the system is started on one side of *the vertical plane* through the origin, screw chaos appears. If started on the other side, the system exhibits spiral chaos.[17],p.19; italics added.

Any attentive reader, even without any general knowledge of dynamical systems theory or particular expertise in so-called chaotic dynamical systems or the genesis and properties of Rössler systems, would immediately wonder whether this statement implies the following:

1. Why pick out the Rössler system , and not any one of the other available dynamical systems, say the well known Lorenz system of equations or the Henon mapping, as a model to encapsulate the complex dynamics of so-called chaotic phenomena?

2. Why choose a continuous dynamical system rather than a discrete (mapping) one?

3. Do the two 'classes', *screw* and *spiral* chaos exhaust the geometry of the Rössler system?

4. Does *the vertical plane* endow particular parameter values with a bifurcating property, and if so can they be determined analytically or computationally?

5. Is any significance, of an analytical or any other sort - based on natural phenomena, experiments in physical, chemical, biological or any other theory - to be attached to the third, non-linear, equation?

6. *What* kind of non-linearity did the 'third' equation embody (which was, of course, easy to check by inspecting the equation), and *why* that particular kind and not any other?

Obviously some of the answers are trivial: the author is simply rehashing the contents of [68] - but the uninitiated reader would not know it as such. The six questions themselves are nothing but commonsense queries that would arise in the mind of any serious scholar exploring mechanisms for encapsulating complexity in dynamical systems and looking for guidance in choosing from a vast menu of wondrous possibilities.

Motivated by the innocence of these commonsense questions and curious about possible answers - for without explicit or implicit answers to them it would be useless to advocate the use of the Röossler system, or any other system for that matter - I decided to explore the womb, so to speak, and went back to the relevant sections in [68], pp.124-6. An extensive quote, with the *alleged* Rössler equations which Wible reproduces uncritically and without comment in the Appendix to his chapter in [17], p.26, might be useful at this point(underlined emphasis, added):

> A very interesting mathematical model of chaotic behavior has been suggested by Otto Rössler. It contains three variables and only one quadratic nonlinearity:
>
> $$\frac{dx}{dt} = -y - z \tag{1}$$
>
> $$\frac{dy}{dt} = x + ay \tag{2}$$
>
> $$\frac{dy}{dt} = bx + z(x - c) \tag{3}$$
>
> a, b, c being positive constants. The equations of evolution have two fixed points, one of which is the trivial solution $x_s = y_s = z_s = 0$, and the other the point $x_s = c - ab, y_s = b - (c/a), z_s = (c/a) - b$. We will discuss only the phenomena occurring around the first fixed point, $P_0$. <u>For a large range of parameter values</u> ... linear stability analysis ... predicts that $P_0$ is unstable. The behavior in its vicinity has the following peculiar features. The trajectories are repelled away from $P_0$ along a two-dimensional surface of phase space, in which the fixed point behaves like an unstable focus; they are attracted along a one-dimensional curve .... We call such a fixed point a *saddle-focus*. Such a configuration gives rise to instability of motion, a basic ingredient of chaotic behavior, but it also allows for the *reinjection* of the unstable trajectories in the vicinity of $P_0$ and thus for the eventual formation of a stable attractor. [From the geometry of] chaotic attractors attained for two different parameter values. Both of the features just mentioned are clearly present. However, [for the chosen parameter values] <u>all trajectories</u> are reinjected toward $P_0$ on the same side of the plane xz, whereas <u>some</u> [of the others] are reinjected on the other

side. We call these two motions *spiral* chaos and *screw* chaos, respectively.

As the attentive reader, that elusive creature, might easily and immediately note, there is nothing in these elementary *non-analytic* observations, based on a restricted set of computational exercises in [68], from which Wible could have inferred any definite answers to any of the commonsense queries posed above. There is not even the hint of a suggestion that any particular *vertical plane* possesses a privileged status; nothing at all about *screw* and *spiral* trajectories forming an exhaustive class; and no reasons given as to why a 'quadratic non-linearity' rather than a cubic one should govern the dynamics of the 'third equation'. But most disturbingly, even the Gods, from time to time, nod! The system of equations given above are *not* the Rössler system! The standard Rössler equations are [78], p.397:

$$\frac{dx}{dt} = -y - z \tag{4}$$

$$\frac{dy}{dt} = x + ay \tag{5}$$

$$\frac{dz}{dt} = b + z(x - c) \tag{6}$$

Where, in Rössler's case, the parameters had values: a=b=0.2; c=5.7.

The phase-plane plots of the Rössler and 'Wible' systems are given in Figure 1:

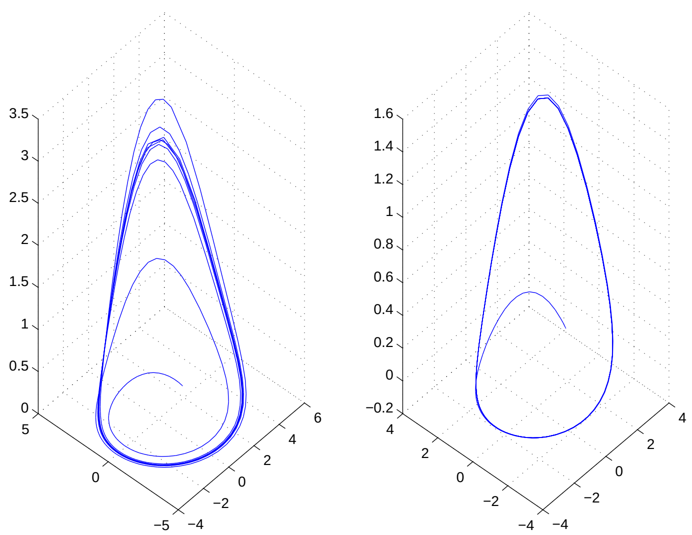



Figure 1: Dynamics of the Rössler and Wible equations

Note the crucial difference between (6) and (3) (and in the corresponding phase-plane dynamics of Figure 1) - even if, to the untrained eye, mind and brain it may seem a trivial one, it is fundamental in such non-linear systems. The difference between 'b' in (6) and 'bx' in (3) is fundamental from the point of view of the genesis of the Rössler system and the methodology with which Rössler embarked upon its construction - not to mention the resulting geometry of the dynamical system (based, as always in these cases where the *existence* of a strange attractor is yet to be 'proved' analytically, on selective numerical exercises, although Wible's discussion might give the contrary and incorrect impression)[9]. Therefore, it may be apposite to inform the unwary reader of Wible (and readers of [68]) of the motivation which underpinned Rössler's construction of the system (4) - (6). Rössler observed, as legions of others before and after him had done[10], that the geometry of the famous Lorenz equations[11], containing two quadratic non-linearities, generated,[78],p.397:

> '...two unstable foci (spirals) suspended in an attracting surface each, and mutually connected in such a way that the outer portion of either spiral is "glued" toward the side of the other spiral, whereby the outermost parts of the first spiral map onto the innermost parts of the second and vice versa.' ([78],p.397; cf. Annex, fig.1).

Then, went on to wonder whether or not:

> [A] *simpler* equation which directly generates a similar flow and forms only *a single spiral* may be of interest, even if this equation has, as a "model of a model", *no longer an immediate physical interpretation.* (ibid, p.396; italics added)

[9]I was unable to reproduce the dynamics in [68] simply because the precise initial conditions and the numerical methods used for integration were not adequately specified.

[10]But had not asked the imaginative question that Rössler then posed and answered so effectively.

[11]The Lorenz equations (for which I have simulated fig.1 in the Annex) are:

$$\frac{dx}{dt} = 10(x-y) \tag{7}$$

$$\frac{dy}{dt} = x(28-z) - y \tag{8}$$

$$\frac{dz}{dt} = xy - \frac{8}{3}x \tag{9}$$

What can be a *simpler* non-linear system of equations than the Lorenz system, if one starts with it? Clearly, the next level of *simplicity*, maintaining non-linearity, would be to eliminate one of the two quadratic non-linearities, subject to the geometric criterion of generating 'a single spiral'. These are, of course, classic Ockhamist principles of mathematical modelling and Rössler succeeded, with these principles and criteria to construct his 'unnatural' system of equations (4)-(6). But why was the quadratic non-linearity inserted in the 'third', inhomogeneous, equation rather than in one of the other two? Because of a third criterion in the construction which Rössler called *Liénard's "building block principle"*, which is, basically a modelling principle whereby 'slow' and 'fast' variable dynamics are coupled [80], p.376. Essentially, he started with a two-dimensional linear oscillator with an *unstable focus*, i.e., choosing parameter values such that the attractor is an unstable focus:

$$\frac{dx}{dt} = -y \tag{10}$$

$$\frac{dy}{dt} = x + 0.15y \tag{11}$$

To this system he added the 'third' variable coupled to the 'first' one non-linearly:

$$\frac{dz}{dt} = 0.2 + z(x - 10) \tag{12}$$

It can be seen that $z$ tends towards a value close to zero so long as $x$ is below a threshold value of 10, but rises 'autonomously' and without limit above this threshold value. Next, armed with his geometric criterion of requiring the system to generate one, single, spiral, Rössler sought a 'folding' mechanism. This he obtained by requiring the increase in $x$ to be negatively coupled to the increase in $z$ and tightening the coupling in the system by modifying (10) to:

$$\frac{dx}{dt} = -y - z \tag{13}$$

Thus, the genesis of the famous Rössler system with its particular parameter values. Two cardinal modelling principles were invoked: *Ockham's Razor* and *Liénard's "building block principle"*. In what sense are they of relevance to an interpretation of the 'History of Economic Thought'? Wible does not even pose this question meaningfully. However both of these principles were invoked in what I have often called, in other contexts, *Classical Non-linear Multiplier-Accelerator Models*, to which I shall return in a later section. They were invoked as *modelling principles*, not as a justification for the use of any particular dynamical system for encapsulating complex or any other kind of dynamic behaviour.

Finally, as for whether *spiral* and *screw* chaos are exhaustive classifications for the Rössler system, it is again best to go back to the 'womb', i.e., Rössler, who defined such behaviour for variations of his equations (cf, for example [79]). In fact he defined four types of behaviour, not just the two mentioned in [68] and repeated without reflection by Wible: (a). Spiral-type chaos; (b). Screw-type chaos; (c). Torus-type chaos; and (d). Inverted Spiral-type chaos. Having described the geometric structure of the behaviour represented by each of these types, he observed ([80], p.188):

> *Assuming* that the set of 'simplest' possibilities is already *exhausted* with the preceding examples (*which is hard to prove, however*), the next simplest examples will be of the composite type ....

I think we now have almost all the answers to the six commonsense question I posed immediately after stating and quoting Wible's uninformed assertions about the Rössler system. *Screw* and *Spiral* type chaos do *not* exhaust the type of dynamical behaviour in such systems; indeed, there is no such result about exhaustive behaviour for such - or many other popularly used systems that generate so-called chaotic-equation systems. Nor is there any analytical information whatsoever about which of the uncountable infinity of vertical planes that can be drawn through the origin would act as boundaries of basins of attraction. In fact, it is also easy to prove that such basins of attraction, even if they existed, could not be detected and computed even by an ideal computer (i.e., the *Turing Machine*). We know, now, why the quadratic non-linearity was added to the 'third equation'; and we also know why certain parameter values were chosen in the construction of the Rössler equations. Also, we know that the system does not represent and was not constructed to encapsulate the behaviour of any naturally occurring or experimentally determined system but was constructed artificially, albeit invoking two fundamental modelling principles. That leaves the question about choosing a continuous vs. discrete system in the context in which Wible's discussion takes place. This is too thorny a question to be dealt with at this stage and, in any case, is neither raised nor discussed in Wible's chapter or any other chapter in either of the books. I shall have something to say about this in the concluding section, (cf. also, [100]).My reflections on these will touch on the sensitive interactions between non-linear dynamical systems, numerical analysis and computability theory. Surprisingly, in none of the 31 chapters (including the two Introductions by the common editor of both books) of the two books being reviewed here, are the latter two issues even raised, let alone considered in the context of any kind of complexity theory or its applications.

# 3 Evolution and Varieties of the Complexity Vision I

> ...[A] fundamental problem in methodology [is that] the traditional theory of "dynamical systems" is not equipped for dealing with constructive processes. ...We seek to solve this impasse by connecting dynamical systems with fundamental research in computer science .... ...*Many failures* in domains of biological (e.g., development), cognitive (e.g., organization of experience), social (e.g., institutions), and economic science (*e.g., markets*) *are nearly universally attributed to some combination of high dimensionality and nonlinearity. Either alone won't necessarily kill you, but just a little of both is more than enough. This, then, is vaguely referred to as "complexity".* [28],pp.56-7; italics added.

Complexity is, partly[12], about *nonlinear* processes. Hence, a natural mathematical tool to tame them theoretically, to extract laws, would be dynamical systems theory. But the other horn of complexity, *high dimensionality*, makes it almost impossible to handle them - i.e., high-dimensional, nonlinear, processes - analytically and we must resort to numerical exercises, hence rely on a theory of computation to simulate alternative scenarios and inductively identify patterns from which to infer plausible laws.[13] Hence one of the core theoretical technologies of any complexity vision will be some conjunction of dynamical systems theory and the theory of computation or recursion theory. At some point in the evolution of these two theories, versatile researches must

[12]There is a danger in concentrating on nonlinearity and analysis and forgetting that *combinatorial* and *diophantine* structures are equally fertile generators of 'complexities'. I shall deal with these issues in the first section of Part II.

[13]Strictly speaking it should be a conjunction of dynamical systems theory, a theory of computation and the theory of numerical analysis. This latter field, autonomous in its own right, has been almost totally neglected for its relevance to the exercises involving the other two. A notable exception is the recent comprehensive text by Stuart and Humphries ([92]), where one set of conjunctions - that between dynamical systems and numerical analysis has been thoroughly and rigorously explored. The other conjunction, that between computation theory and numerical analysis has been imaginatively explored by Smale and his co-workers in a series of papers and a comprehensive book (cf. [12]). The conjunction between all three, partially discussed in [12], remains, largely, an unexplored minefield, with potentially explosive, but also exciting, results to be expected.

have seen the fertility of a judicious conjunction that, I think, gave birth to the methodological underpinnings for the sciences of complexity, from which, gradually, an interdisciplinary complexity vision has emerged.

There have been many serious and less-than-serious 'stories' written about the origins of the sciences of complexity. The story I wish to tell has to have some element of topicality for the purpose of this essay and, hence, will be somewhat biased and even slightly personal. The same story could have been told with alternative personalities and varied emphases, had the purpose been different. These caveats should be kept in mind when reading this section which, in any case, is highly telescoped and cannot be anything other than an unsatisfactory synopsis. Perhaps I can refer that famous elusive creature, the interested reader, to [99] for a more comprehensive and less biased story of the 'evolution and varieties of the complexity vision'.

Keeping in mind the antonyms *easy* vs. *hard*, *simple* vs.*complex*, *tractable* vs. *intractable* conjoined with the distinction between stable vs. unstable, equilibrium vs. dis-equilibrium, and linear vs. non-linear, in thinking about the difference between **self-reproducing** *states* and **self-organising** *orders* provide, I believe, the clue towards being able to tell a plausible story about the evolution and varieties of the complexity vision. [14]

In everyday life, most of us encounter or bring about events that are typical of transition from one kind of state to another. The homely examples are the boiling of water, the freezing of water, the changes in seasons as , for example, the emergence of colours heralding autumn and the falling of leaves signalling, at one end, the end of summer and, at the other end, the onset of winter, the action of magnetizing a piece of metal, and so on. Paradoxically, the self-organisation seems to take place in the transition regimes between identifiable stable regimes - in that twilight region where the bright summer sunset gives way to an autumnal dusk, at the various equinoxes. Once the self-organisation takes place, endogenously, and the transition to a new regime is accomplished, then one returns to dissipative or evolutionary self-reproduction till critical values of the characterising coordinates signal the onset of a new transition and so on forever.

[14] To this one may be add the more specialised and technical differences between *computable* vs. *uncomputable* and *decidable* vs. *undecidable.* The complex regimes are not located in the uncomputable or the undecidable domains; they are located within the computable and the decidable domains. Within these latter domains a finer stratification generates the more interesting characterisations that delineate the easy from the hard, the tractable from the intractable and the simple from the complex using various mathematical representations of abstracted real world problems.

That which has brought the possibility of the new sciences of complexity and a complexity vision of the sciences is, most of all, the feasibility of *mathematical formalisms* of and for these transition regimes, the possibility of identifying and determining numerical values that characterise critical parameters and coordinates of the transition regimes and conjectures, validation and speculations that such characterizations are **universal**. In other words, there is a kind of universality encapsulated in parameters that characterise transitions of various structures in diverse mediums - water to steam, ice to water, autumn to winter, spontaneous magnetization and, perhaps, from steady state growth economies to cyclically growth economies or from underdeveloped economies to developing and, then, developed economies, too.

It was early recognised that it was useless to try any mathematical formalism of transitional phenomena using 'classical mathematical analytical solutions'. This was because of the twin horns of *non-linearity* and *high-dimensionality*. Ingenious techniques were developed to tackle linear, high-dimensional, systems; and, non-linear, low-dimensional systems: co-ordinate transformations (diagonalisations)of high-dimensional, linear systems, linear approximations of non-linear systems, brute-force theoretical straitjacketing of formalisms to fit the available results in non-linear dynamical systems[15], and worse.

The *new* mathematical formalisms for the *new* sciences of complexity came from two very special directions, almost simultaneously: developments in the theory and *numerical* study of non-linear dynamical systems and new paradigms for representing, in a *computational* format, ideas about self-reproduction and self-reconstruction in high-dimensional systems. The interpretations of the latter in terms of the theory of the former and the representations of the former in terms of the paradigms of the latter was the serendipitous outcome that gave the sciences of complexity its most vigorous and sustained mathematical framework. It is that particular story that I wish to outline in this section. The classic contributions to this story are the following: [95], [102], [60], [64], [81] and [91], from which emerged a vast, exciting and interesting work that has, in my opinion, led to the complexity vision and the sciences of complexity. These contributions are classics and, like all classics, are still eminently readable - they have neither aged nor have the questions they posed become obsolete by the development of new theoretical technologies; if anything, the new theoretical technologies have reinforced the visions they foresaw

[15]For years, endogenous business cycle theory was confined to reducing the macroeconomy to the phase-plane so as to facilitate the use of standard results in non-linear dynamics, like the Poincaré-Bendixson theorem, and so on.

and the scientific traditions and trends they initiated.

The new sciences of complexity and their mathematical formalisms emphasise, therefore, the numerical and computational underpinnings of dynamical systems, optimization, evolution and emergence in observable phenomena. They are also about the emergence of novelty and patterns from elementary and transparent rules of interactions between formless entities, thus posing what I consider to be the fundamental question in these sciences: how does form arise from non-form and how are these new forms made to persist or generate endogenous forces to persist and perpetuate themselves: self-organise, self-reconstruct and self-reproduce themselves? This question poses the ultimate challenge to every form of reductionism.

In telling my story I want to resurrect a tradition that has, sadly, been neglected by the evangelists of the complexity vision in economics, many of whom are featured in the essays collected in the two books being reviewed. Moreover, when there is a reference to this tradition, as in the chapter by Brock and Colander, via a further reference to Krugman's booklet on *The Self-Organizing Economy* (cf. [54]), or to the work of 'the Brussels school', it is a careless one. In addition, there seems to be a kind of monumental unawareness as to how an ingenious use of *linear* models is often sufficient for the analysis and discussion of emergence, self-organization and complexity. It is what I call the 'The Turing Tradition -Mark I'.

The story that I try to reconstruct in the rest of this section is divided into two parts. The first part is told in term of the genesis of the idea of using the paradigm of the cellular automaton as the vehicle through which ideas of self-reproduction, self-reconstruction and self-organisation came to be encapsulated. This has its origins in the fundamental work of von Neumann on these issues, but relying on Turing's ideas on computability, the universal computer and computation universality. The key mathematical theorem in this part of the story is the recursion theoretic fix-point theorem.

The second part of the story is about Turing's attempt to discuss, along early dynamical systems lines, the generation of new forms from formless elements. I try to tell this story in a way that will highlight the connection between the cellular automata tradition pioneered by von Neumann and the dynamical systems tradition that has some roots in Turing's characteristically audacious attempt at creating a theory of morphogenesis long before bifurcation theory and the theory of catastrophes became fashionable.

Finally, there should be a fourth and a fifth parts. These I have postponed to $5 which is, in turn, divided into two parts. Those stories have their roots, on the one hand, in computability theory[16] and I have come to call it applied recursion theory; and, on the other hand, in dynamical systems theory. The former lies at the interface between computational complexity theory and algorithmic complexity theory, linking problem solving difficulties and the randomness of individual objects viewed combinatorially. The idea of 'criticality' is given a more precise formal definition in the first part of $5. The latter is a more conventional and more familiar story, couched in terms of dynamical systems and their limit sets. But I do try to link the discussion of criticality to universality and decidability in a way that is not common in the standard literature on complexity visions.

### 3.1 The von Neumann Tradition

> The *formalistic* study of *automata* is a subject lying in the intermediate area between logics, communication theory, and physiology. It implies abstractions that make it an imperfect entity when viewed exclusively from the point of view of any one of the three above disciplines - the imperfection being probably worst in the last mentioned instance. Nevertheless an assimilation of certain viewpoints from each one of these three disciplines seems to be necessary for a proper approach to that theory. Hence it will have to be viewed synoptically, from the combined point of view of all three, and *will probably, in the end, be best regarded as a separate discipline in its own right.* ([102], p.91; last set of italics added.)

It is interesting to reflect that he did not mention a fourth 'area' as being pertinent for the 'formalistic study of automata' - the theory of dynamical systems. But, then, dynamical systems theory became a separate discipline in its own right only after the codification by Smale in 1967 and the other great insights and results of Lorenz, Ruelle, Thom and others in the 60s - in spite of the fact that the great traditions of Poincaré, George Birkhoff, van der Pol, Andronov and Cartwright-Littlewood results and works were available even at that time[17].

As always, von Neumann's intuition is beginning to be realised. 'The formalistic study of automata' is beginning to be considered 'a separate discipliine in its own right', largely due to Wolfram's work, but based on the prior work of

[16] Hence, again, owing much to Turing, which is why I have called it 'The Turing Tradition - Mark II

[17] For a masterly and brilliantly constructed story of dynamical systems theory, cf. [7]

von Neumann, Ulam, Toffoli and Margolus, to mention just the great pioneers only.

von Neumann listed five criteria to be considered or satisfied by 'a formalistic study of automata' when viewed from the point of view of 'logics and construction':

1. Logical universality
2. Constructibility
3. Construction-universality
4. Self-reproduction
5. Evolution

I shall deal with details of these aspects in $9. For the moment I shall simply describe one way of approaching the von Neumann theory of the formal and logical study of automata by means of the construction of cellular automata, a strategy that Ulam suggested to von Neumann.

Consider a space of cells; a cell may have one, two or any discrete, natural, number dimension. The space of cells may have a finite number of cells, as in a tape of finite length, say $n$ meters, divided into equal rectangular blocks or two-dimensional cells like a page in a child's square-ruled exercise book; or the space could be infinite-dimensional, imagining the tape to be extended in one or both directions by the additions of new tapes whenever necessary and, analogously for the square-ruled page in the two dimensional case and so on for higher dimensions. If the space is finite, some boundary conditions would have to be specified when talking about events taking place inside the cells at the borders, two in the case of a one dimensional tape, and correspondingly more in higher dimensions.

Inside each cell there are mechanisms that are activated at regular intervals and result in displays or actions or switches or analogous devices being set in motion. For simplicity let us assume that the nature of the action results in a display of a discrete signal, say a natural number. Suppose each cell can take on $k$ such values. Then, the state or configuration of the entire system is defined by the state values in each of the cells, taken simultaneously at any given point in time. In the one-dimensional case, with each cell assuming one of $k$ possible natural number values, the value in any cell, say, $c_i$ could be any of:

$$s_i = 0, 1, 2, \ldots, k - 1 \tag{14}$$

Now, suppose we introduce some *interlinked dynamics*. By this I mean the following. Each cell is in communication with its 'nearest' neighbours, which means, in the one-dimensional case at most three: it can communicate with itself, a kind of reflexivity principle[18], and with its neighbours on its left and its right. In the case of two-dimensional arrays of cells, any cell can communicate with at most nine cells; and so on. However, in the case of the one-dimensional array of cells, if the tape does not extend infinitely in both directions, the end cells will not have, respectively, their right and left neighbours to communicate with and, hence, some extra rules - called, technically, *boundary conditions* - will have to be imposed exogenously. The communications between cells is for the purpose of ascertaining their mutual values and of determining what values they will take in the next time period or the next step in some process of which they are a part. The reflexivity assumption means that the new value taken by any cell depends on its present value (in addition to the present values of its 'nearest' neighbours). Suppose, now, also, that each cell can only take the Boolean values, 0 or 1, and the updating of their cell values takes place *synchronously*, timed to a *global clock* linked to the collection of cells as a whole.

I have gone into almost child-like details in the description of this mechanism for three reasons: one, to show how *simple* the mechanism is; two, to emphasise the fact that only *local* information transmission is being supposed; three, to indicate where the local-global divide resides (in this case it is activated by the global clock/ synhronicity assumption)[19]. Now, the question is whether such a simple mechanism is sufficient for our purposes? But, what are 'our purposes'?

A possible cellular configuration, say at time t=1 and, applying a local-neighbourhood rule at the click of the global clock, its value at the next time instant t=t+1 could be as follows, where k=2 and i=20.[20]

| | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 1 |

[18] Or even a self-reference principle

[19] Any kind of interesting dynamics comes about only with a local-global divide. Wherever there is no local-global divide, or where local information is sufficient for global dynamics, the system, except for exceptional cases, cannot generate interesting dynamics. In the economic theory of more recent times the most famous example was the Lucasian local-global divide via the 'Island paradigm'. There, however, the exogeneity was stochastic.

[20] The way to handle the incompleteness in a rule for the end elements, 0 at the left end and 1 at the right end, would be to assume the usual 'wrap-around' pairing. This means the right neighbour of the last element is the first element on the left hand side of the array and vice versa.

| | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 1 | 1 | 1 | 1 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 1 | 1 | 1 |

What is the point of a series of tables of sequences of 0s and 1s? Note, first of all, each cell could be, for example an ideal computing machine, i.e., a Turing Machine. Each value $c_i$ could be any natural number and each of them, in turn, signifying anything nameable: a function, a proposition and so on.

It is, of course, tedious to keep on enumerating each cell individually, at each point in time, for any given local rule. I shall now introduce the mathematical formalism, but implement different rules, using MATLAB, in the Appendix. However, one particular famous rule is given in Figure 2.

The local neighbourhood rule, in the one dimensional case can be formalised as follows:

$$c_i(t+1) = \rho[c_{i-r}(t), c_{i-r-1}(t), \ldots, c_i(t), \ldots, c_{i+r-1}(t), c_{i+r}(t)] \tag{15}$$

In the special case, therefore, of immediate neighbour influences, in the one-dimensional case, the local rule would be:

$$c_i(t+1) = \rho[c_{i-1}(t), c_i(t), c_{i+1}(t)] \tag{16}$$

Formally, this rule becomes:

$$\rho : k \times k \times k \longrightarrow k \tag{17}$$

This rule, a mapping, from the set of all neighbourhoods, call it $\nu$, to the set of all states, call it $\sigma$. If the number of states is k ($\in \sigma$), the set of all possible neighbourhoods contains $k^{2r+1}$ members for a neighbourhood extending to r cells on either side of the cell in question. Hence, the rule (17) is a definition of a map from the set of integers $\nu \in \boldsymbol{Z}^{2r+1}$ to $\sigma \in \boldsymbol{Z}$:

$$\nu \longrightarrow \sigma \tag{18}$$

Clearly, this rule is, in general, non-invertible. How do we go from the local rule and the behaviour of singular cells to the behaviour of the whole system - i.e., what is the passage from the parts to the whole? Corresponding to the state of any individual cell, $c_i$, the state of the entire system of cells will be denoted by $\boldsymbol{C}$:

$$\boldsymbol{C} = [c_1, c_2, c_3, \ldots] \tag{19}$$

Rule (18), which endows the whole system with its dynamics by providing an updating formula, yields the new state for the whole system by changing the values of each individual cell, say from $c_i$, $\forall$ i, to $c_i^{'}$, $\forall$ i, as the global clock signals to all of them simultaneously:

$$\boldsymbol{C'} = [c_1^{'}, c_2^{'}, c_3^{'}, \ldots] \tag{20}$$

Hence, the dynamics of the whole system is represented by the transformation:

$$\boldsymbol{C} \longrightarrow \boldsymbol{C'} \tag{21}$$

The idea of a system-wide self-organising order, if it is to be captured by specifying only local rules of interaction between a large class of single units, means finding the relationship between (18) and (21). Suppose we naively presume that this relationship can be established by a simple 'study' of all possible local rules (16) by means of simulating them in some order. How many of them are there? For a general neighourhood of r-cells in a 1-dimensional system with k states, there will be $k^{2r+1}$ neighbourhoods; each of these neighbourhoods can result in one of k different states. Therefore, the total number of $\nu$ functions to be investigated will be the astronomical number: $k^{k^{2r+1}}$! So, even here resort to simple enumerations after even systematic simulations is a daunting, essentially impossible, task[21]

But plucking a leaf from the *universality* results in dynamical systems, we can wonder whether, leaning on *Ockham's Razor*, a low dimensional consideration - without compromising on the # of singular or elementary cells - may not yield equally *universal* results in this case. In other words, say beginning with cells in a 1-dimensional array and restricting the range of the neighbourhood # to the immediate next-cells, i.e, the minimum non-trivial # of 3, and the number of states, too, to the minimum meaningful #, i.e, 2, can we not enumerate and classify possible global behaviours based only on specifying local rules? If we can, and if the global behaviours yield meaningful structures and comprehensible dynamics, then there might be scope for an exact theory of self-organising order achieved dynamically.

For this restricted class of 1-dimensional cell arrays with r=3, k=2, the # of potential $\nu$ functions will be: $2^{2^{(2\times1)+1}} = 256$. They can be given a numerical labelling, using a formula suggested by Wolfram, as follows:

$$\boldsymbol{R} = \sum_{n=0}^{7} c_n 2^n \tag{22}$$

[21]Any residual Bourbakian or Formalist who would resort to analytical methods will have to appeal to various kinds of black magic and alchemy, such as the axiom of choice, some well-ordering principle, the law of the excluded middle, etc. This may well be God's mathematics but, then, if it is so, let him/her do it - as Errett Bishop wryly observed.

where the $c_n$ are determined according to the following scheme for the 1-dim, k=2, r=3 case. The number of cells in the neighbourhood of each cell is 3 and each of them can have one of two values, say, 0 and 1. Hence the list of the eight possible neighbourhood cells are:

| $c_7$ | $c_6$ | $c_5$ | $c_4$ | $c_3$ | $c_2$ | $c_1$ | $c_0$ |
|---|---|---|---|---|---|---|---|
| 111 | 110 | 101 | 100 | 011 | 010 | 001 | 000 |

Each $c_i$ is assigned a subscript, i = 1 to 7, according to the binary number value of the neighbourhood which it names. Thus, for example:

$$111 = 1 \times 2^0 + 1 \times 2^1 + 1 \times 2^2 = 7.$$

If the neighbourhood rule for the array tabulated above is as follows:

| 111 | 110 | 101 | 100 | 011 | 010 | 001 | 000 |
|---|---|---|---|---|---|---|---|
| 0 | 1 | 0 | 1 | 1 | 0 | 1 | 0 |

In other words, any active cell, meaning any cell with value 1, if neighboured by two active cells or two non-active cells, becomes non-active at the next tick of the global clock. In all other cases, it remains (or becomes) active in the next period. For this neighourhood rule, the rule number $\boldsymbol{R}$, according to (22), will be:

$$\boldsymbol{R} = c_0 2^0 + c_1 2^1 + c_2 2^2 + c_3 2^3 + c_4 2^4 + c_5 2^5 + c_6 2^6 + c_7 2^7 = 0+2+0+8+16++0+64+0 = 90 \tag{23}$$

Consider the following array of cells, initialised 'randomly' and subject to the same local rule as above:

| | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 |

I shall assume that the array extends indefinitely to the left and right with 0s for all entries in these invisible cells. How does it propagate itself as the global clock ticks (the only other exogenous mechanism connected to it - apart from the local rule endowed to it, by chance, providence, design, or whatever).

The first 15 global clock ticks result in the following development of and in the cells[22]:

[22]See the appendix for a more graphic depiction of the development, i.e., dynamics

| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | 0 | **1** | **1** | 0 | 0 | 0 | 0 | 0 | 0 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | 0 | 0 | **1** | **1** | **1** | 0 | 0 | 0 | 0 | 0 |
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | 0 | **1** | **1** | **1** | 0 | **1** | **1** | 0 | 0 | 0 | 0 |
| 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | 0 | 0 | **1** | 0 | **1** | 0 | **1** | **1** | **1** | 0 | 0 | 0 |
| 0 | 0 | 0 | 0 | 0 | 0 | **1** | 0 | **1** | **1** | 0 | 0 | 0 | 0 | **1** | 0 | **1** | **1** | 0 | 0 |
| 0 | 0 | 0 | 0 | 0 | **1** | 0 | 0 | **1** | **1** | **1** | 0 | 0 | **1** | 0 | 0 | **1** | **1** | **1** | 0 |
| 0 | 0 | 0 | 0 | **1** | 0 | **1** | **1** | **1** | 0 | **1** | **1** | **1** | 0 | **1** | **1** | **1** | 0 | **1** | **1** |
| 0 | 0 | 0 | 0 | 0 | 0 | **1** | 0 | **1** | 0 | **1** | 0 | **1** | 0 | **1** | 0 | **1** | 0 | **1** | **1** |
| 0 | 0 | 0 | **1** | 0 | **1** | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | **1** | **1** |
| 0 | 0 | 0 | **1** | 0 | 0 | **1** | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | **1** | 0 | 0 |
| 0 | 0 | 0 | **1** | **1** | **1** | 0 | **1** | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | **1** | **1** | **1** | 0 |
| 0 | 0 | 0 | **1** | 0 | **1** | 0 | 0 | **1** | 0 | 0 | 0 | 0 | 0 | 0 | **1** | **1** | 0 | **1** | 0 |
| 0 | 0 | 0 | 0 | 0 | 0 | **1** | **1** | 0 | **1** | 0 | 0 | 0 | 0 | **1** | **1** | **1** | 0 | 0 | 0 |
| 0 | 0 | 0 | 0 | 0 | **1** | **1** | **1** | 0 | 0 | **1** | 0 | 0 | **1** | **1** | 0 | **1** | **1** | 0 | 0 |
| 0 | 0 | 0 | 0 | 1 | 1 | 0 | 1 | 1 | 1 | 0 | 1 | 1 | 1 | 1 | 0 | 1 | 1 | 1 | 0 |

The discerning reader might *infer*, by inspection of the array of cells, developing downwards as the global clock ticks, some of the **1**s and some block of **1**s propagating themselves[23] Can any such inference be full-proof? In other words, given any arbitrary rule determined from (22) and arbitrary initial conditions, is it possible to infer or deduce the long-term, steady state configuration of the system of cells? Due to a famous result in classical recursion theory, *the Halting Problem for Turing Machines*, the answer is NO! We can only simulate, inspect and infer.

The geometry of the evolution of Rule 110 is given below. This is a rule that propagates *universally* in the sense that it can simulate the activities of a Universal Turing Machine. Hence, we can discuss *reproduction* and *reconstruction* using a Cellular Automaton of this type. Is there a meta-rule that can be implemented effectively such that a search can be carried out to classify all possible evolutions, starting from arbitrary initial conditions? Again, the answer is NO. Can a structured search be carried out, starting from any given evolution, to classify the possible classes of initial conditions that may have generated them? Again, the answer is NO.

History is intractable and so is the future.

[23] The diagrammatic technique used in the Appendix and the longer, but more compressed, time-scales for which the development is depicted may provide a better basis for *inference*.

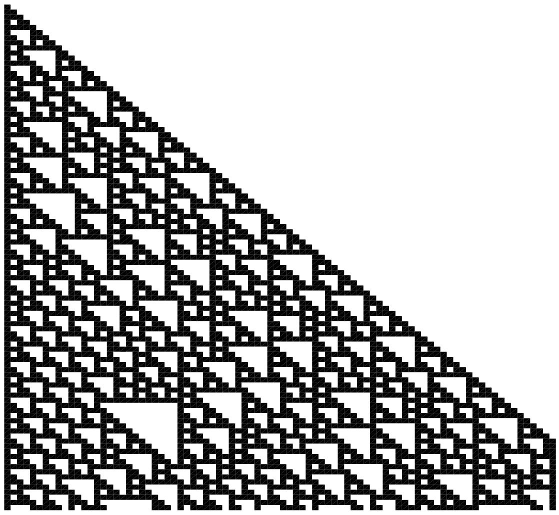


Figure 2: CA Rule 110

On the other hand there have been fertile conjectures on various kinds of classifications of possible evolutions. These attempts have harnessed the theory of dynamical systems and tied it to the theory of computability to provide interesting classifications. Here there seems to be some hope in that the following four configurations, using the language of dynamical systems theory, appear to exhaust the possibilities:

1. Limit Points.
2. Limit Cycles.
3. Strange Attractors.
4. Configurations delicately poised at the borderline of Strange Attractors and Limit Cycles.

The fourth class, it has been conjectured, is capable of universal computation. The dynamical system equivalent of the CA capable of universal computation depicted in Figure 2 should have its basin of attraction lying in that delicate frontier between a limit cycle and a strange attractor. This is, therefore, one route to disciplining the anarchy of simulation. But for this to be an effective route, a recursive rule transforming any given CA to an equivalent dynamical system has to be devised. I doubt that such a rule can be devised but I have no doubt that particular rules can be constructed.

In any case, I shall return to these themes within, the context of the five desiderata of von Neumann with which I began this section, in $9 and $10.

## 3.2 The Turing Tradition - Mark I

> If chemical reactions and diffusion are the only forms of physical change which are taken into account the argument ... can take a slightly different form. For if the system originally has no sort of geometrical symmetry but is a perfectly homogenous and possibly irregularly shaped mass of tissue, it will continue indefinitely to be homogeneous. In practice, however, the presence of irregularities, ..., will, if the system has an appropriate kind of instability, result in this homogeneity disappearing. ...
> Unstable equilibrium is not, of course, a condition which occurs very naturally. ... Since systems tend to leave unstable equilibria they cannot often be in them. *Such equilibria can, however, occur naturally through a stable equilibrium changing into an unstable one.* For example, if a rod is hanging from a point a little above its centre of gravity it will be in stable equilibrium. If, however, a mouse climbs up the rod the equilibrium eventually becomes unstable and the rod starts to swing. ... The system which was originally discussed ... might be supposed to correspond to the mouse somehow reaching the top of the pendulum without disaster, perhaps by falling vertically on to it.
> [95], pp.42-44; italics added.

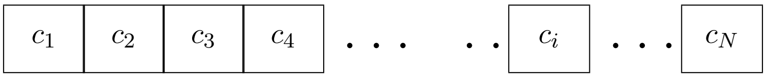
With characteristic ingenuity and simplicity, Turing devised what may best be called a *linear mouse theory of self-organization*! I shall describe, as faithfully as possible, even to the extent of using Turing's own original notations wherever possible, this *linear* mouse theory of morphogenesis.

As mentioned in the introductory paragraphs to this section. one of the purposes of presenting this section is to disabuse the reader of a commonly held mis-conception that nonlinearity is necessary, in a dynamical system context, to generate self-organizing complexities. Secondly, to show the close affinity of the Turing mouse theory to von Neumann's cellular theory. In one of those remarkable coincidences with which scientific practice is blessed, the two great pioneers worked on these issues almost simultaneously. Turing, as always, in complete isolation.

But a third purpose is also to show where the theory can be made non-linear and how Smale, two decades and more after Turing's original publication, returned to the same theme and generalised it in one fruitful direction. However, I shall present this work in $10, below.

Consider the following $N$ chain of cells:

**Figure 3**:Turing chain of a Ring of N Cells




Suppose each of the $N$ cells contain an *activator* and an *inhibitor*[24] X and Y, respectively. The concentration of each type of morphogen in any cell, i, is denoted by $X_i$ and $Y_i$, respectively.

$$\frac{dX_r}{dt} = f(X_r, Y_r) + \mu(X_{r+1} - 2X_r + X_{r-1}) \tag{24}$$

$$\frac{dY_r}{dt} = g(X_r, Y_r) + \nu(Y_{r+1} - 2Y_r + Y_{r-1}) \tag{25}$$

With deviation from steady state values denoted by respective lower case vaiables:

$$\frac{dx_r}{dt} = (ax_r + by_r) + \mu(x_{r+1} - 2x_r + x_{r-1}) \tag{26}$$

$$\frac{dy_r}{dt} = (cx_r + dy_r) + \nu(y_{r+1} - 2y_r + y_{r-1}) \tag{27}$$

[24]Turing called them *morphogens*, 'form-producers':'The systems actually to be considered consist therefore of masses of tissues which are not growing, but within which certain substances are reacting chemically, and through which they are diffusing.These substances will be called *morphogens*, the word being intended to convey the idea of a *form producer*.It is not intended to have any very exact meaning, but is simply the kind of substance concerned in this theory.' [95], p.38;italics added. Morphology, the study of *forms*, was very much a creation of Goethe.

This linear system of equations can be orthogonalised by introducing the coordinate transformations, $\xi_i$, $\eta_i$ ($\forall i = 0 \dots N$), for x and y, respectively, and using the relationship[25]:

$$\sum_{r=1}^{N} \exp[\frac{2\pi i r s}{N}] = \begin{cases} 0 & \text{if } 0 < \text{s} < \text{N} \\ N & \text{if s=0 or s=N} \end{cases}$$

Then, it is easy to show that the orthogonalised system of 2N decoupled equations in the new coordinate system are:

$$\frac{d\xi_s}{dt} = (a - 4\mu \sin^2 \frac{\pi s}{N})\xi_s + b\eta_s \tag{28}$$

and:

$$\frac{d\eta_s}{dt} = c\eta_s + (d - 4\nu \sin^2 \frac{\pi s}{N})\eta_s \tag{29}$$

The characteristic equation for this system, with roots $p_s$ and $p_s$, is:

$$(p - a + 4\mu \sin^2 \frac{\pi s}{N})(p - d + 4\nu \sin^2 \frac{\pi s}{N}) \tag{30}$$

Then, the general solutions for $\xi_s$ and $\eta_s$, respectively, are:

$$\xi_s = (A_s \exp^{p_s t} + B_s \exp^{p_s^{'} t}) \tag{31}$$

and:

$$\eta_s = (C_s \exp^{p_s t} + D_s \exp^{p_s^{'} t}) \tag{32}$$

Where the coefficients are constrained by the relations:

$$A_s(p_s - a + 4\mu \sin^2 \frac{\pi s}{N}) = bC_s \tag{33}$$

and:

$$B_s(p_s - a + 4\mu \sin^2 \frac{\pi s}{N}) = bD_s \tag{34}$$

Finally, substituting the solutions from the orthogonalised case, i.e, ((31)-(32), to the original system, we get the solution for $X_r$ and $Y_r$:

$$X_r = h + \sum_{s=1}^{N} (A_s \exp^{p_s t} + B_s \exp^{p_s^{'} t}) \exp[\frac{2\pi i r s}{N}] \tag{35}$$

[25] In the first case, since the l.h.s is a geometric progression and 0 < s < N, the result is immediate; in the second case, in either of the cases, s=0 or s=N, all terms are equal to 1, hence the l.h.s sums to N (cf. [95], p.39).

and:

$$Y_r = k + \sum_{s=1}^{N}(C_s \exp^{p_s t} + D_s \exp^{p_s^{'} t}) \exp[\frac{2\pi i r s}{N}] \tag{36}$$

In the case of a continuous ring of cells (tissues), the relevant relations and equations are:

$$\mu = \mu^{'}(\frac{N}{2\pi\rho})^2$$

$$\nu = \nu^{'}(\frac{N}{2\pi\rho})^2$$

Then:

$$\frac{\partial X}{\partial t} = a(X-h) + b(Y-k) + \frac{\mu^{'}}{\rho^2}\frac{\partial^2 X}{\partial \theta^2} \tag{37}$$

$$\frac{\partial Y}{\partial t} = c(X-h) + d(Y-k) + \frac{\nu^{'}}{\rho^2}\frac{\partial^2 X}{\partial \theta^2} \tag{38}$$

It is clear, then, that (37)-(38) are the limiting cases of (28)-(29). The general solutions for the system (37)-(38) are, then, given by:

$$X = h + \sum_{s=-\infty}^{\infty}(A_s \exp^{p_s t} + B_s \exp^{p_s^{'} t}) \exp^{is\theta} \tag{39}$$

and:

$$Y = k + \sum_{s=-\infty}^{\infty}(C_s \exp^{p_s t} + D_s \exp^{p_s^{'} t}) \exp^{is\theta} \tag{40}$$

The characteristic equation for the two roots $p_s$ and $ps^{'}$ are:

$$(p - a + \frac{\mu^{'} s^2}{\rho^2})(p - d + \frac{\nu^{'} s^2}{\rho^2}) = bc \tag{41}$$

and the coefficients are constrained by the relations:

$$A_s(p_s - a + \frac{\mu^{'} s^2}{\rho^2}) = bC_s \tag{42}$$

and:

$$B_s(p_s^{'} - a + \frac{\mu^{'} s^2}{\rho^2}) = bD_s \tag{43}$$

One obtains the characteristic equations for the above orthogonalised, decoupled, system of 2N equations, using the following notations:

$$\theta = \sin^2(\frac{\pi s}{N}) \tag{44}$$

$$a(\theta) = a - 4\mu\theta \tag{45}$$

and:

$$d(\theta) = d - 4\nu\theta \tag{46}$$

Then, the characteristic equation is:

$$\lambda_{1,2} = \frac{1}{2}\sqrt{\{[a(\theta) + d(\theta)]^2 + 4[bc - a(\theta)d(\theta)]\}} \tag{47}$$

Assuming, in the first instance, stability for the original linear system, (26)-(27), then:

$$\alpha = a + d \leq 0 \tag{48}$$

and,

$$q = ad - bc \geq 0 \tag{49}$$

The dynamics for (26)-(27), in case of strict inequality for (48) and (49), results in attractors of the stable node or focus types and a centre-type attractor results for the equality.

The corresponding relations determining stability for the decoupled system, (28)-(29), are:

$$p_\theta = a_\theta + d_\theta \tag{50}$$

and:

$$q_\theta = a_\theta d_\theta - bc \tag{51}$$

From (30), (31) and (32) it can be seen quite easily that $p_\theta$ decreases monotonically with increases in $\mu$, $\nu$, and $\theta$. On the other hand, since $q_\theta$ depends on $a_\theta$ and $d_\theta$ multiplicatively (cf.(37)), there is no unambiguous way of relating changes in $q_\theta$ to changes in the relevant parameters, $\mu$, $\nu$ and $\theta$. However, it is clear that as the parameters are varied or change autonomously, and given that $p_\theta$ decreases (or increases) monotonically, the dynamics of the system loses stability as $q_\theta$ becomes negative. The loss of stability, from a stable node or focus, to a saddle-point, is called a *Turing Bifurcation*.[26]

It is this loss of stability that Turing exploited to provide a beautiful, simple but counter-intuitive example of how, from a formless initial structure, form

[26]This can be contrasted and compared with the more familiar *Hopf Bifurcation*, where the parameter variation and loss of stability involves foci and limit cycles.

- i.e., 'order' - can be generated. It is counter-intuitive in that one expects a diffusive mechanism to iron out - i.e., smooth out - inhomogeneities. Instead, starting with a minor inhomogeneity in a linear, coupled, dynamical system, the Turing Bifurcation leads to a growth of form - i.e., inhomogeneities.

The attentive reader may have noted that the diffusive coupling assumed for the linear coupling was 'nearest-neighbour'. In this sense, the discrete dynamical case can easily be re-interpreted in terms of the von Neumann system activated on a grid.

But the same reader would also realize, with a little extra thought, that this is an inessential assumption. I make this minor observation in view of a mis-conception that may arise if one takes a Krugman comment too seriously[27]
.

For the moment this must suffice. The themes that Turing broached, and the kind of analysis he developed, provides a fertile source for those interested in complexity analysis and a wholly different, essentially dynamical *ab initio*, approach for a complex adaptive systems analysis. This is amply illustrated by works such as those by Kelso ([46])and Levin ([58]. In particular, Kelso's work integrates pattern detection with a dynamical systems and simulation perspective at the forefront, eschewing all the paraphernalia of 'statistics and probability' and showing the virtues of metastability - more particularly, but misleadingly, sometimes refereed to as 'life at the edge of chaos'. The Turing bifurcation generates a positive feedback; the propagation of inhomogeneities in a self-organizing, order, generator; and so on.

# 4 Economic Theory, the 'Teaching of Economics' and the Complexity Vision

> 'I adore *simple* pleasures,' said Lord Henry. 'They are the last refuge of the *complex*'
> Oscar Wilde: *The Picture of Dorian Gray*, ch.2; italics added.

Teaching the complexity vision and endowing aspects of economic theory with a complexity vision are the primary subject matters of [18]. The book is divided into five parts, in addition to a general Introduction by the editor.

[27] "The edge city model ... was inspired by Turing's reaction-diffusion model. It differs greatly in the details: in particular, in the biological analysis *it is essential to model cells as being affected directly only by their immediate neighborhood*, whereas in an urban model nothing is disturbing about the idea that a mall's profitability is affected by conditions 10 miles away."([54], pp.48-49; italics added). Incidentally, Turing's analysis was *not* biological but chemical.

Parts one, three, four and five tackle the problem of 'teaching the complexity vision' to economists. However, parts three, four and five are devoted to the subject in more particular ways. Part one is a general sweep over possible complexity visions for and in economics. Part two is on the specific issue of how a complexity vision may temper our enthusiasm on the policy front. Part five, finally, deals with the very specific issue of the complexity vision coupled to biological metaphors and how this coupling might be utilized felicitously in the teaching of economics.

The opening chapter, by the editor, is an introductory broad sweep, of *a* possible complexity vision from the point of view of an economist and a general summary of the various chapters included in this volume.

In the citadel of economic theory, *General Equilibrium Theory*, there is more than a 'little of both', *nonlinearity* and *high dimensionality.* The general equilibrium theorist can justifiably claim that the core model has been studied with impeccable analytical rigour, 'using classical mathematical analytical methods'. Therefore, since there is so much more than 'just a little of both', nonlinearity and high dimensionality, in the general equilibrium model, it must encompass enough 'complexity' of some sort for us to be able to endow it with a complexity vision. Why, the puzzled economic theorist may ask, don't we do precisely that, instead of building *ad hoc* models, without the usual micro closures.[28].

There are three fundamental criticisms, strongly emphasised by many of the authors in the volumes being reviewed here, of orthodox theory - by which I shall understand the 'rigorous' version of general equilibrium theory and not some watered down version in an intermediate textbook - from an alleged complexity theoretic point of view. Orthodox theory is weak on processes; it is almost silent on increasing returns technology; it is insufficiently equipped to handle disequilibria. The first obviates the need to consider adaptation; the second, in conjunction with the first, rules out positive feedback processes; and the third, again in conjunction with the first, circumvents the problem of the emergence of self-organised, novel, orders. I am in substantial agreement with these fundamental criticisms. The question, now, is whether the essays in the two volumes being reviewed here offer an alternative vision - the 'complexity vision' - which provides a new closure or new interpretations of the old closure such that the textbooks can or will be rewritten.

[28] I use this word 'closure' instead of the more loaded word 'microfoundations' deliberately. I believe the idea of the 'closure' of neoclassical economics, i.e., based on *preferences*, *endowments* and *technology*, is more fundamental and can be given many more foundations than the orthodox one.

On the other hand, a more modest challenge would be the suggestion that the 'complexity vision' does not challenge the foundations of the citadel but it offers complementary tools, methods and an alternative epistemology - say, more emphasis on induction, broadly conceived, rather than deduction. Thus, for example, without economically unmotivated mathematical assumptions, such as compactness where there are no intuitive grounds for them in economic reality, which may be necessary for the application for this or that fix-point theorem, to go direct to a numerical investigation of possible worlds by simulation exercises. Or, for example, without linearising everything in sight[29], retaining essential nonlinearities and coping with them via explorations in a world of structured simulations and inferring, systematically, about patterns that may make sense from an a priori economic theoretic point of view. Such complementarities may well be the best case that can be made at this point in the development of the sciences of complexity till, some day, such a science attains the status of a unified theory that, in any case, seems to have eluded even a much-mined field like nonlinear dynamical systems theory.

If, therefore, the 'teaching of economics' is to be challenged and given a complexity vision, particularly at the most foundational levels, then it seems to be clear - at least to this writer - that the citadel has to be confronted fairly and squarely. This must, surely, mean to replace the closure in some sensible way that will reflect the importance of *processes*, alternative possibilities for *returns to scale* in costs and technology and the persistence of *disequilibrium processes*. A framework that is weak on processes *ab initio* would not, of course, be expected to be strong on *disequilibrium processes*.

However, at least as I understand the general message in the two volumes, the vision of a starting point in closures is considered *reductionist* and the idea is that there are aspects of reality that do not need or cannot be squared with either a *reductionist science* or a *reductionist vision*, both of which are coupled to an exaggerated importance being endowed to the search for first principles and its alleged handmaiden, *deduction*. These ideas are concisely and cogently put forward by the editor in his introductory 'manifesto' to [18], p.3:

> It is important to note that complexity science is not a replacement for standard science; it is a supplement. It does not say that standard science is not a reasonable approach to take. It simply states that there may be other approaches that offer insight into areas that standard *reductionist* science has not been

[29]Whilst adding the caveat 'with no loss of generality'!

> able to crack. These areas involve large systems of interacting entities - complex systems. [emphasis added]

Colander then goes on (ibid), referring to arguments made in a recent book by Sunny Auyung ([8])[30]:

> [Auyung] makes a similar point. She argues that science has never been exclusively reductionist in nature; instead it has involved a two-part approach - one reductionist in nature, and one a study of large composite systems. She explains how physics has a separate branch called solid state physics, which analyzes how certain aspects of reality are understood without appeal to *first principles. They appear. and are, in some way connected to first principles*, but the connection is too complex for us to understand or model. The complex systems have emerged and exist, but cannot be understood through *reductionaism.* [emphasis added]

In economic theory of any variety, on the other hand, it is almost sacrilegious to eschew first principles; hence the plethora of attempts to provide 'microfoundations' for macroeconomics and foundations, indeed, for everything else and the abhorrence of the *ad hoc*. If, therefore, the complexity vision is an attempt to provide a justification for confronting the reality of economic phenomena that cannot usefully be analysed by reductionist, analytical, methods that rely on first principles, then it is, surely, to be welcomed. Why, then, is there the reluctance in the economic community or, at least, by the high priests in the citadel? Why is there the '15% rule'?[31]

[30] I have not read Sunny Auyung's book. However, as an undergraduate student in the faculty of engineering at Kyoto University, in the late 60s, it was impressed upon me, with crystal clarity, that there was such a thing as *phenomenological thermodynamics* clearly distinguished from *statistical thermodynamics*. In the former one did not worry about first principles in the traditional sense; the whole *raison d'être* of the latter was, of course, its underpinnings in 'first principles'. In the former one worked with *measurable* aggregates, such as pressure, volume and temperature, akin to the national accounting aggregates that underpin macroeconomics. But, my teachers never failed to add, that the 'first principles' themselves were normally hanging by their own bootstraps. We knew, or were pretending to know, that the trouble lay with 'entropy', but we also knew that we had to ignore the 'trouble' it caused or face an *impasse*; like that great Scottish Preacher of whom Dennis Robertson was so fond of, we chose the former path - or, rather, were taught to do so.

[31] This is Colander's '15% rule': 'a textbook *can* differ from its previous edition by 15 percent each time' ([18], p.127, emphasis added). Surely, then, by its sixth or so edition a textbook could present entirely new material without too many citadel feathers being ruffled? Contrast this with a view held by that great, unheralded - at least in the two volumes being reviewed here - pioneer of the complexity vision in economics, Herbert Simon:

Finally, why now and not earlier - why this attack on reductionism and the search for first principles at this particular point in time? Colander and many of his collaborators in these two volumes seem to argue that the difference is the emergence of a new tool: *the computer*. But, of course, the computer has been around for quite a long time - and, conceptually, at least since Pascal, Leibniz, Babbage, Jevons and a host of others. So, why was its potential to tackle issues of complex reality not realised earlier? I am not satisfied with the facile answer provided by Colander, [18],p.3:

> Since scientists have always known that reality is complex, a natural question is: why a new science now? If the complexity approach is a reasonable way of looking at reality, would it not have started long ago? The answer is that what is different is the computer - or, better expressed, the potential of the computer. Developments in computer technology are offering a means to gain far more insight into more complex systems of dynamic equations that previously could be imagined.

Is the advocacy, then, that the citadel is to be transformed by an abode of 'complex systems of dynamic equations'? If so, in what sense are we to take seriously the alternative claim that the complexity vision is not trying to supplant the citadel but only to supplement it with more potent auxiliary tools? I find all this *ad hockery* utterly confusing and it defeats the essential and laudable purpose of at least remedying the traditional reliance on classical mathematical tools and their logical handmaidens for the formalization of economic reality.

The first main chapter in [18] is by Brian Arthur and is titled *Complexity and Economic Theory*. It is 'an extended version of a paper that appeared

'. . . I am encouraged by the great upswell, in the US and especially in Europe, of experimental economics and various forms of bounded rationality. I think the battle has been won, at least the first part, although it will take *a couple of academic generations to clear the field and get some sensible textbooks written and the next generation trained.* [Letter to the author, 25 May, 2000; emphasis added]. This is a view to which Simon came after almost half a century of 'battles' with the citadel, and he was only then ready to think that a few academic generations down the line the time may be ripe for 'sensible textbooks' to be written for the training of the future members of a hopefully more enlightened citadel! I think Colander and his collaborators are in an undue hurry. The ground has not been laid sufficiently clearly for the battle to be fought - the opponent not clearly identified, the ground on which the battles are to be fought not well specified and, indeed, it is not even clear that the proponents of a complexity vision are fighting a battle, if the task is simply to provide supplementary tools, unlike Simon and his fellow- behavioural economists (whose terrain has also been usurped by false pretenders from the citadel, against whom Simon fought an unrelenting battle to his dying day).

in *Science*, in 1994.' It continues the theme, partly, broached in the Editor's 'Introduction', on 'why now, not earlier', the answer being 'computers' - but for Arthur (p.19):

> Complexity as a movement came along in the late 1970s and early 1980s because at that time scientists got workstations.

The chapter is divided into five subsections: complexity, positive feedbacks, expectational problems in economics, financial markets and a conclusion.

The first section is quite illuminating, especially for a novice to issues of complexity but might be slightly exasperating, to the expert - in complexity theory and in economic theory. Let me illustrate with one small example.

Arthur's characterization of 'conventional theory', in a variety of cases, is both misleading and incomplete. Thus when he states that 'general equilibrium theory asks: what prices and quantities of goods produced and consumed are consistent with ... the overall pattern of prices and quantities in the economy's markets', very few economists, trained in any kind of 'conventional theory' would recognise it as a characerization of what they learned in their graduate classes and read in their textbooks. Where in Debreu [21] or Arrow-Hahn [3] or any other standard textbook on general equilibrium theory is such a question posed?

On the other hand, the discursive, intuitive, way he describes the idea of a complexity approach is most convincing and should convince many readers of the efficacy and desirability of a complexity vision for economic theory and applied economics. His simple, unadorned, discussions of standard issues in economics approached in the orthodox, analytical, ways contrasted with a vision that emphasises the complexity vision suggests that the latter is a generalized method of which the former is a special case. This writer is wholly in agreement with such an interpretation, although this does run counter to the message in the Editor's 'Introduction'. There we were told that the sciences of complexity are an adjunct to the current visions and methods of standard, reductionist, approaches to a discipline.

Arthur is a respected scientist and a would-be economist. His celebrated forays into economics are, usually, based on the monomaniacal advocacy of the importance of positive feedback and its causes: 'in economics, positive feedback arises from increasing returns' (p.21). Those of us who were brought up on nonlinear trade cycle theory of the classic Keynesian variety must wince at this assertion (as does at least one other enlightened author in this volume, Robert Prasch, in chapter 11). For anchoring, in turn, the importance of increasing returns in economic *theory*, Arthur systematically refers to Marshall, always - at least so far as I have been able to verify in many of his writings[32] - incorrectly and out of context. Let me explain. First of all it seems always a reference to something called 'Marshall (1891)' which, according to Arthur is the ' 8th edition' of the *Principles*. The first edition of Marshall's *Principles* was published in 1890 and the 8th edition in 1920. Arthur's reference is to a mythical '9th edition' of 1891. The actual 9th edition was the Guillebaud edited 'variorum' edition of 1961. Now, if this was an inessential typo, at some point in the cut and paste exercise Arthur should have corrected it. But it persists in his writings through more than a decade of such references.

Secondly, the content of the reference is precisely stated, not in the essay in [18] but in [4], ibid as 'see Book IV, Ch.13 and Appendix H'. One wonders why a page reference is not given to this mythical edition of the *Principles*, but having once been a student at Cambridge and attempted to read Marshall diligently and felt obliged to read the pioneers of that school, I had some familiarity with the actual 8th edition. The particular reference by Arthur is an appeal to Marshall to support his other handmaiden to positive feedbacks: 'lock-in' and 'path dependence'. In the case of increasing returns in the form of the advantages gained by firms who are the first entrants to a market. Arthur quotes as follows:

> A hundred years ago in his *Principles*, Alfred Marshall (1891) noted that if firms gain advantage as their market share increases, then 'whatever firm first get [sic!] a good start will obtain a monopoly'. But the conventional, static equilibrium approach gets stymied by indeterminacy: if there is a multiplicity of equilibria, how might one be reached?

But the actual quote in Marshall (1920), [63], is in footnote 1, on p.459 and is as follows:

> Abstract reasonings as the the effects of the economies in production, which an individual firm gets from an increase of its output are apt to be misleading, not only in detail, but even in their general effect. This is nearly the same as saying that in such case the conditions governing supply should be represented in their totality. They are often vitiated by difficulties which lie rather below the surface, and are especially troublesome in attempts to express the equilibrium conditions of trade by mathematical formulae. Some, among whom Cournot himself is

[32]cf. for example, [4], p. 718 and reference 43 therein. In [5], p.32, the reference is to 'Marshall, 1891' as the 2nd edition, and so on.

> to be counted, have before them what is in effect the supply schedule of an individual firm; representing that an increase in its output gives it command over so great internal economies as much to diminish its expenses of production; and they follow their mathematics boldly, but apparently without noticing that their premises lead inevitably to the conclusion that, whatever firm first gets a good start will obtain a monopoly of the whole business of its trade in its district. While others avoiding this horn of the dilemma, maintain that there is no equilibrium at all for commodities which obey the law of increasing return; and some again have called in question the validity of any supply schedule which represents prices diminishing as the amount produced increases. See mathematical Note XIV where reference is made to this discussion. The remedy for such difficulties as these is to be sought in treating each important concrete case very much as an independent problem, under the guidance of staple general reasonings. Attempts so to enlarge the *direct* applications of general propositions as to enable them to supply adequate solutions to all difficulties, would make them so cumbersome as to be of little service for their main work. The "principles" of economics must aim at affording guidance to an entry on problems of life, without making claim to be a substitute for independent study and thought.

I leave it to the reader to compare the Arthur quote with Marshall's characteristically guarded cautions and assertions. Arthur goes on to claim that, in order, for example, to solve the selection problem when faced with multiple equilibria, 'increasing returns problems in economics are <u>best</u> seen as dynamic *processes* with random events and positive feedbacks - as nonlinear stochastic processes.' But on what evidence are we believe this to be the <u>best</u> method? He goes on to cite various studies (mostly referring to his own impressive work) to substantiate his assertion and infers that this approach leads to the policy prescription that 'governments should ... seek to push the system toward favored structures that can grow and emerge naturally. Not a heavy hand, not an invisible hand, but a nudging hand.' How the government or anyone else with these 'nudging hands' are to identify the 'favoured structures' is left, as usual, hanging in the air.

Suppose the increasing returns instances are due to *indivisibilities.* Is the 'best' method, in this case also, that of viewing them as 'nonlinear stochastic processes rather than as combinatorial problems leading to issues in computational complexity theory? This latter line of research has been fruitfully pursued by Herbert Scarf, [84], [85], [86] in a series of influential papers where, also, the issue of indeterminacy arises and is resolved in other ways than those suggested by Arthur.

The third section is a variation of the theme on tackling the perennial infinite regress problem in 'expectational economics'. It would have been advisable had Arthur made himself familiar with the origins of the rational expectations concept and its original formalization as a fix-point problem. He would then have realized that one way to treat the infinite regress conundrum is to formalize the problem recursion theoretically and apply the famous fix-point theorem of recursion theory.[33] Then the natural inference problem would also have been framed algorithmically with a deft application of Rissanen's stochastic complexity scheme. The claims of an 'emergent ecology' (p.23) in his formulation of the famous *El Farol Problem* is a red herring. Nevertheless, his formulation and solution of the *El Farol Problem* is interesting and original, although it has nothing to do with 'emergence', 'novelty', 'complex adaptive processes' or a 'system of large interacting agents' - all those characterizing elements of the complexity vision. He solves, imaginatively, an inference problem inductively, and that's all there is to it - but that *is*, without doubt a considerable achievement.

The brief fourth section on financial markets is also somewhat unsatisfactory and does not do justice to the possibilities of a complexity vision enriching the analytical capabilities of this field. Arthur, essentially, reports the results of his own work (with others), where a financial market is a collection of computer programs, each one representing an investing agent. In the general case, this would mean a financial market composed of a collection of coupled Turing Machines or, equivalently, coupled nonlinear dynamical systems capable of universal computations. Now, naturally, a collection of very simple finite automata, again equivalently, a collection of simple dynamical systems coupled linearly may converge to halting states or simple attractors and analytical proofs of observed simulations would be quite easy. Once we enter the domain of nonlinear coupling of nonlinear systems or a collection of Turing Machines no reasonable theorist would expect to observe convergence or halting behaviour respectively. In what sense the results rather cursorily described make a case for a complexity vision or add to the large literature on modelling financial markets with nonlinear dynamical systems is unclear. Moreover, the burgeoning literature on *behavioral finance* is not even mentioned.

[33] Rather than rely on formalizing the economic problem topologically and applying Brouwer and related fix-point theorems, (cf. [101].

Finally, the concluding section, once again, is intuitively clear on one significant point: the need 'to study the general emergence of structures and unfolding of patterns in the economy' (p.25) and that this study must be coupled to the adjustments and adaptive behaviours of (rational) agents who are coupled in nonlinear ways to the aggregate economy and to each other. That this underlies a complexity vision is also crystal clear. But that such a vision 'is making itself felt in every area of economic theory' is, surely, wishful thinking! My reluctant scepticism on this point is further buttressed by the curious fact that of the 29 references to this chapter, 16 are to works by Arthur (jointly, or by himself, or in volumes edited by himself - and one incorrect reference)[34].

Chapter 2, by Brock, is both interesting and persuasive. Brock gives an intuitively persuasive definition of what complexity means, across disciplines, and invokes *a* concept of 'universality' to quantify it that is equally convincing. He couples this, in a felicitous way, to *Some Santa Fe Scenery* (the title of the chapter) - meaning by it the kind of theoretical structures and the universal measures related to them that have been the hallmarks of the complexity vision pioneered by *SFI*[35].

> The study of complexity is the *opposite* of the study of chaos; it is the study of how a *very complicated set of equations* can generate some very simple patterns for certain parameter values. Complexity considers whether these patterns have a property of *universality* about them. Here we will call these patterns *scaling laws*. .... The study of complexity is the study of these patterns. ([18],p.29;italics added.)

Thus, it seems clear that Brock emphasizes the detection, characterization and study of 'very simple *patterns'* in the data generated by 'a very complicated set of equations' in his definition of what a study of complexity entails or means. Others, who appear to form a majority, emphasize the generation

[34]Even apart from the infelicity of the dominance of 'self-reference', there is also the issue of whether the papers cited do make the case that Arthur claims they do. In particular, I am not sure that the papers by Marimon, et. al (by the way the reference list is incomplete on this paper as well - the title is not given!) and Leijonhufvud do so. For example, Leijonhufvud's paper is pregnant with suggestive metaphors, drawn from nonlinear dynamics and behavioural economics (of the older Simon-Day variety) but is not a complexity-visioned derivation of the issue of high inflation and the changing behaviour patterns of agents populating economies undergoing transitions to high inflationary regimes. Indeed, the definition of complexity suggested by Leijonhufvud (cf. in particular, pp.331-2 in [57]) is diametrically opposed to the one given by Brock in his more focused discussion in chapter 3

[35]*Santa Fe Institute*

of complicated patterns from simple equations and couplings. How this dissonance can be resolved is not discussed by either of the protagonists in this volume. They go, each their own ways, serenely and with princely unconcern for the confusion they generate in poor readers like me. However that may be, for now, the Brock story continues with an invoking of several metaphors and techniques to illustrate the generation and detection of patterns, but it does not appear, at least not to me, that their origins lie in 'very complicated sets of equations'.

The complexity visions pioneered by 'Santa Fe Sceneries', underpinned by the concepts of *evolution*, *selection adaptation*, *self-organization* and *criticality*, have become part of the seriously popular discourse across disciplines through a trio of highly readable and provocative books written by three eminent scientists: Stuart Kauffman ([45]), Murray Gell-Mann ([29]) and Per Bak ([9]). To this must be added the work by a group of imaginative theorists, working at the frontiers of *dynamical system theory*, and all of whom are variously connected with SFI. There are a host of other popular books on the topic of complexity but these three are at a higher level of exposition in a precise sense. They advocate, particularly [45] and [9], experimentally realizable models that can also be simulated with relative ease. These models can be characterized by universal parameters that give numerical content to imaginative but thorny concepts such as 'criticality', scaling laws, self-organized orders and selection and adaptation in an evolutionary context. Brock's interpretation of a 'Santa Fe Scenery' invokes Bak's work on the *sandpile model*, but, of course, his larger canvas also includes important reliance on a dynamical systems approach to a study of complexity. He refers to Bak's work as that of the 'sandpile man' and describes the workings of the sandpile model to illustrate the issues that confront the complexity theorist:

> Think of a pile of sand on a table that has a *continuous* flow of sand falling on top of the pile. For a while, the sand builds up into a large conical sandpile, but at *periodic* times, when the sandpile builds up to what Bak calls self-organized-criticality, there is an avalanche or series of avalanches until the pile 'relaxes' back to a state where avalanches cease. I am sure that such a reader, if also equipped with some knowledge of classical non-linear dynamics, would be perplexed at the fuss made about sandpile models as metaphors for 'how nature works'. ... [T]hey occur as the sandpile mountain reaches certain proportions as sand drops down upon it from above. The distribution of sizes and 'relaxation times' of these avalanches follows scaling law

patterns. The study of complexity tries to understand the forces that underlie the patterns or scaling laws that develop.(ibid, p.30; italics added)

I doubt any reader who is not already versed in the sandpile model or who has not built and 'played around' with one will become wiser by simply reading Brock's literary - albeit elementary - description of it[36]. Here, therefore, is a diagrammatic realization of the sandpile model and one of its many possible scaling laws and another speculative representation of a possible 'universality' w.r.t parametric variations. The model underlying this particular realization of a sandpile is given and explained in $11. It is exactly that which is discussed and described in [9], p.52,ff.

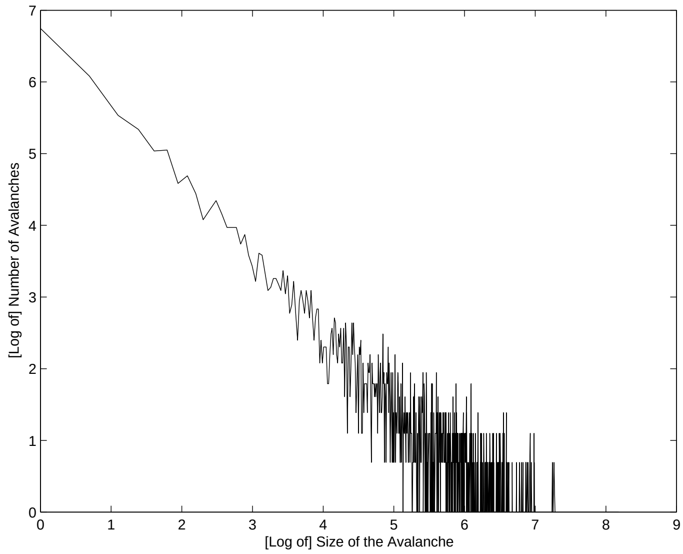



Figure 4:Power Law for a Sandpile Avalanche

[36]Indeed, apart from one diagram in the first of Brian Arthur's two chapters and a few in chapter 9 (at least one of which is slightly misleading), there are no illustrative diagrams depicting simulations or computations in a book strong on invoking the virtues of simulation, computation, induction and pattern detection. Some examples of simulations and a few pedagogical illustrations of the kinds of patterns one is supposed to detect would have helped the sympathetic reader to understand what it is that the authors are advocating

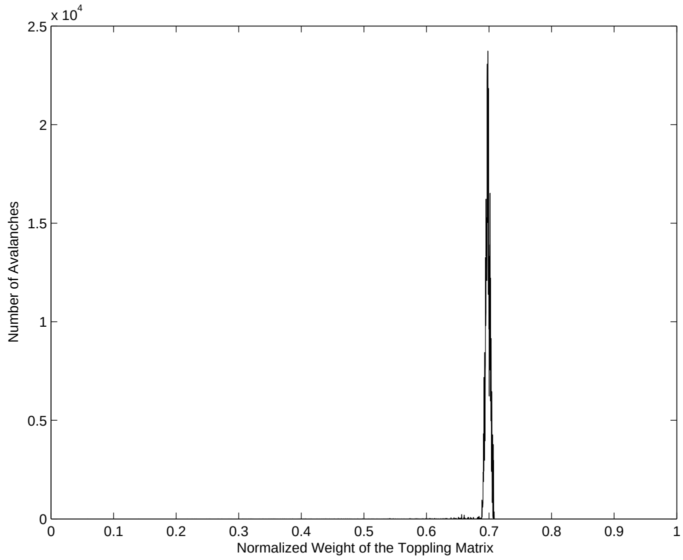



Figure 5:Possible Regularities of Avalanches w.r.t Weighted Toppling Matrices

Now, the power law in figure 4 represents the connection between the size of an avalanche - i.e., the 'amount' of sand falling due to any one portion of a sandpile losing its balance and triggering a chain of such losses in neighbouring portions - and the duration of the event, before the various parts of the whole return to a quiescent state. The distribution of the size vs. for example, the duration is what seems to resemble classical power laws with which economists have long been familiar, such as the Pareto Law of income distribution. 'Universality', in turn, seems to reside in the fact that the shape of such power laws are independent of the particular dynamics of the avalanche in any sandpile.

So, why should this interest economists? Apparently because 'the same type of patterns show up in finance'(ibid, p.30), thus prompting the inference that chain reactions in such and related markets may well benefit from modelling strategies that use sandpile dynamics.

An attentive child, reared on sea shores, may wonder how a '*continuous* flow' of a granular medium can be effected. On the other hand, a gardener in one of the ancient Shinto shrines in Kyoto, in the unlikely event of coming across this description, might wonder why Brock or the 'sandpile man' invoke sandpiles and the incongruity of a granular medium and continuity when the old method of watering the shrine gardens would be an adequate metaphor to generate relaxation oscillations. But let these perplexities pass in the interests of Santa Fe Sceneries. However, I do urge the interested reader to cast an eye over the diagrams of see-saws as water reservoirs, uncoupled and coupled, imaginatively used in [32], especially figures 1.01, 1.2.1 and 1.6.2.

In the interests of Shrine Gardeners and others, like me, who were reared on classical non-linear dynamics and who were encouraged to think of markets as coupled oscillators, I give below a depiction of the watering principle, the geometry of the relaxation oscillation in phase space and a devil's staircase emanating from coupled watering see-saws. All of the mathematics and models are given in \$10. The tap depicts the flow of water for convenience in drawing; the usual mechanism would be a water-reservoir, collecting water from natural flows, and the flow will not, necessarily, be steady. Coupled see-saws are connected in a natural way, the overflow from one feeding into another see-saw and so on. The phase-space diagram of relaxation oscillation is very similar to those that used to be popular in the trade-cycle literature of the 50s. Finally, I advocate the Devil's Staircase, instead of Power Laws, as the Universality principle, simply because I can eschew any reliance on 'statistics and probability' - at least for reasons of providing counter-examples to the dominance of the probabilistic vision championed by a section of the Santa Fe complexity theorists.

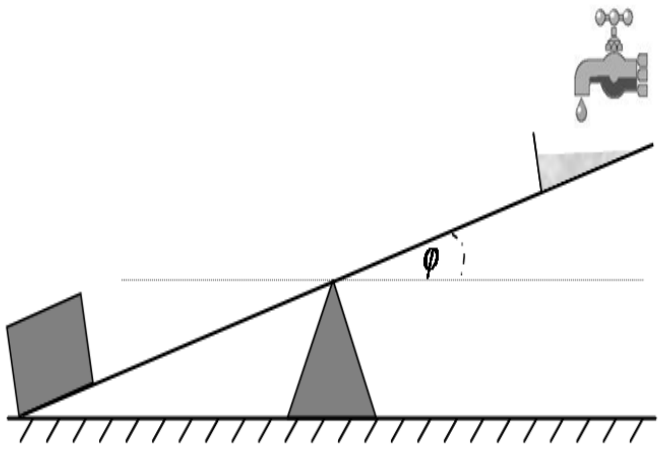



Figure 6:Watering a Shrine Garden

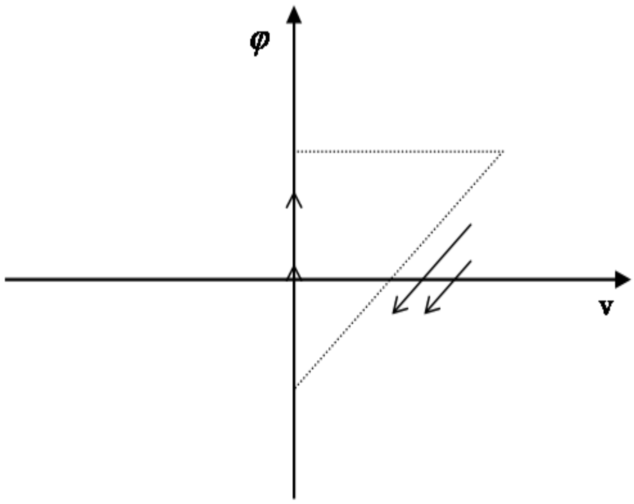



Figure 7:Relaxation Oscillations in a State Space

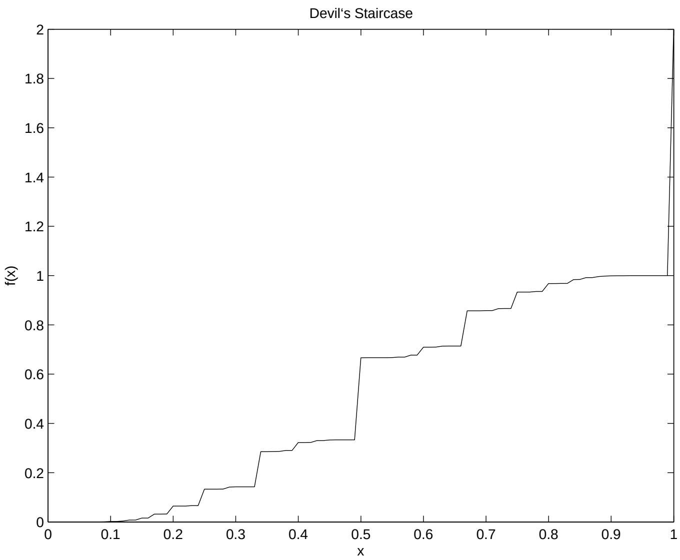



Figure 8: The Devil's Staircase

Brock's next foray is into vague pronouncements on methodology, but basically enunciating a theme that runs through many of the essays in the book: viz., there is such a thing as 'standard economics' [37] which is dominated by

[37] Occasionally given the sobriquet 'orthodox economic theory', but more generally, the inference from context is a reference to mathematical economics in general and general

‘deductive formalism’ as its methodological credo and belongs, therefore, more to ‘mathematical philosophy’ than to the practice and precepts of an (empirical) science. The latter, ostensibly, is characterized by inductive methods[38] inferring, from patterns, underlying generating mechanisms and possible theories that underpin them. Brock’s admirably concise summary of the Santa Fe credo is worth quoting in full, simply because it encapsulates the methodological theme underlying all of the essays in this and its companion volume:

> In terms of methodology, if there is one thing that separates the Santa Fe approach from formalistic and deductive approaches, it is that the Santa Fe approach looks for patterns and constructs explanations using tools that blend ideas from evolutionary computation and statistical mechanics.

But, of course, we are left wondering whether these ‘tools’ themselves have been constructed without reliance on ‘formalistic and deductive approaches’.

Brock, then, goes on to list a couple of famous examples of patterns that have been detected and from which ‘universalities’ in the form of scaling laws have been inferred: *Zipf’s law of city size scaling* and *Mandelbrot’s self-similarity hypothesis*. To understand the latter, Brock suggestion to the diligent reader, without explicit algorithmic instructions on how to do it, is the following:

> Take time-series returns on any asset ... and look at the fraction of daily returns on the time-series data set that is greater than some amount, say X. Call this PR(X). Take the log of PR(X). Plot that on the vertical axis of your graph paper. Then plot the log of that number X on the horizontal axis. What you will get is almost a straight downward sloping line. This slope, alpha, is called the tail exponent.
>
> In my view this is a remarkable and unexpected pattern.

equilibrium theory, in particular. No one will accuse any kind of macroeconomic theory as being dominated by ‘deductive formalism’; nor even large parts of any kind of Marshallian microeconomics can be accused of being preoccupied with that kind of methodology.

[38]Induction is not uniformly advocated in the book (although variant formulations of deduction is subject to wholesale condemnation). This is strange and incongruous because *all* the mathematical methods utilised or invoked in the essays of this and its companion book, without exception, are derived by methods of ‘deductive formalisms’. I shall return to this theme in the appropriate section of this essay. There are those, like Colander, Hoover and Matthews, who seem to prefer a version of a Peircean version of *abduction*, but more on this anon.

I have done as suggested by Brock, taking the publicly available data on daily returns on IBM shares and the result is shown in the following plot.

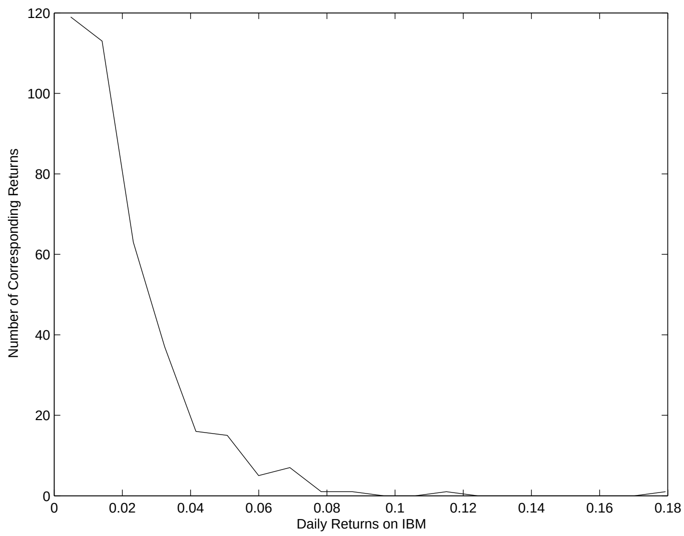



Figure 8:Possible Power Law of IBM Returns

The content of the self-similarity hypothesis and that this is a 'scaling law' is the empirical fact that one gets the same kind of graph - the same slope - whatever the frequency of returns: daily, weekly, monthly or whatever; and for whatever market[39]. 'Self-similarity' carries the connotation of 'repetition' and this, in turn, suggests that there may be an *iterative process* as the underlying generative mechanism. Hence, to return to Brock's definition of the 'study complexity', as 'the study of how very complicated set of equations can *generate* some very simple patterns', means:

> [T]he simplicity of complex systems is to be found in the study of iterative processes, not in the system. So, like all science, the science of complexity looks for simplicity, but it looks for that simplicity in iterative processes, not in the structure of the system. Put another way: simplicity is to be found in the underlying generating functions, not in the complex organization of reality. (ibid, p.32)

But how, from such an inference and definition, one can go on to make the sweeping claim that, therefore, 'the foundations of complexity science are in

[39] Or, dare I say, whatever time-series, recalling data on personal income distribution in the advanced countries.

*statistics* and *probability* study, not in calculus or set theory' is beyond my comprehension. Surely, one of the most useful ways to pursue 'the study of iterative processes' is via recursion theory and the theory of dynamical systems? It is entirely feasible to base one's study of iterative processes along recursion theoretic lines and a tradition in dynamical system theory - symbolic dynamics - that need not rely on 'calculus or set theory', but it would be an unnatural and convoluted study.

Or is the implication that Brock and his Santa Fe adherents have developed theories of 'statistics' and 'probability' that eschew any reliance on calculus and set theory? Perhaps the calculus and set theory underpinning the kind of statistics and probability that is being invoked by the Santa Fe vision is based on an inductive mathematics[40]!

The rest of Brock's chapter is divided into a set of homilies on techniques to detect patterns in data and computational tools to model theories. Brock seems to advocate Bootstrap methods for detecting patterns in view of the feasibility, cheap availability and facility of computation intensive methods. This reviewer, belonging to a generation that was brought up on Monte Carlo methods in an age when none of these criteria were satisfied, approves wholeheartedly this advocacy. But my own approach would be to initiate the students to Bootstrap techniques by first introducing them to the original aim and strategy of Monte Carlo methods and, then, to move onto the more general possibilities of Bootstrapping. This would give them an idea of the fertile interplay between probabilistic and deterministic methods to solve complex analytical problems. It would be a nice stepping stone towards methods for teaching chaos - to which Brock devotes a few felicitous pages - and the interplay between determinism and indeterminism inherent in that area.

There is, however, a curious anomaly in two of the sections titled, respectively: 'How to Teach Chaos and Complexity: The Example of Ecology' and 'Some Ideas About Teaching Pattern Detection', the fifth and sixth sections in a chapter divided into eight sections plus a short introduction and a brief conclusion. Having informed the reader in the very first page of this dense and informative chapter that 'the study of complexity is the opposite of the study of chaos', the second of the above two sections begins by 'turn[ing] to techniques for teaching chaos, *and* the detection of patterns' (p.39; italics added). Why? Perhaps because the eminently plausible characterization of

[40]Perhaps Santa Fe visionaries are groping towards their own version of a constructive mathematics? I doubt it - there is not the slightest evidence, at least not in the essays in the two books being reviewed here, nor in any of the references in these essays, that any of these visionaries is even remotely interested or aware of alternative mathematical philosophies that may bolster a varieties of epistemologies.

the study of complex systems as that of studying *iterative processes*(p.32) or as that of the *study of the generating functions underlying patterns* (p.34) is best approached by an investigation of those functions and mappings that underpin chaotic dynamical systems. But, then, if the definition of complexity itself is 'the opposite of the study of chaos' why bother teaching those iterative processes or generating functions that lie at the roots of chaos?

As for computational tools, for example to model, simulate and detect patterns in dynamical systems, Brock takes as an example the desirability of making students interactively aware of the potentialities inherent in a software like MATLAB[41]. But the examples from ecology, culled from a textbook in that subject, are not about exercises in 'statistics and probability studies', but about and on the study of standard dynamical systems.

I could not agree more fully with Brock's wholly agreeable view that(p.31):

> [C]omplexity type theorizing unleashes the mental imagination onto entirely unexpected directions that stress visual computer graphic constructive devices of scientific dispute resolution that should be attractive to today's students.

My regret is that not one single 'visual computer graphic constructive device' is displayed in an otherwise interesting chapter. Somewhere between page 42 and 43 the author seems to have indulged in the modern way of writing scientific papers: cutting and pasting. This has left its traces in some incoherency at the bottom of page 42, a section that, surely, belongs to the next subsection on 'Bootstrapping'. But, then, it also looks as if the previous subsection, titled 'Bootstrap-Based Tests of Models Fitted to Data', is slightly superfluous, given that it is followed by a full-blown section on 'Bootstrapping' only a page later! Finally, like almost every single chapter in the book, there is an infelicity in the reference list - in this case a minor one of a missing reference. There is a reference to something by Loretan and Phillips (1991) which does not appear in the reference list.

Chapter 3, by Brian Arthur, is titled, 'Cognition: The Black Box of Economics'. The editor, in the lead footnote, states that the text of this chapter 'is adapted from the conference keynote address upon which this volume is based'.

[41] All of the programs implemented in this essay have been done so using MATLAB. But, of course, I could have done all of this using MATHEMATICA, as well or, in some cases, even more easily. There is no general reason to advocate any one kind of software over any other as superior in any generic sense. In it may have been useful to have had a few examples from a wider variety of available software such as MAPLE, MATHEMATICA AND MATLAB.

Arthur's starting point, to make a case for economists taking issues of cognition seriously[42] is that there is an unopened black box between the framing of an economic problem and its solution. Inside this black box, Arthur contends, when opened, there will be found the methods of solutions, the processes of cognition that implement these methods and the mechanisms of mind that are the repositories of these processes. As adjuncts to these elements Arthur also invokes the props of associative memory and a theory of mental representations. This is an eminently plausible scenario in all respects, except for the simple fact that these ingredients, in part or in their entirety, have been proposed as architectures for an epistemology of the mind at least since Aristotle and definitely since, first Karl Lashley, Kenneth Craik, D.O.Hebb, F.A. von Hayek (not, this time as a theorist of 'Austrian Economics', but as the author of *The Sensory Order*[34]) and, then, decisively and definitively, since Newell and Simon's monumental *Human Problem Solving* [67] began circulating in draft form from about 1959. Even this is an incomplete and selective list, if one expects to be serious about opening the black box of cognition, but it will do for the issues broached in Arthur's brief collection of vignettes and highlights that hope to pass for a serious essay on deep and controversial issues. None of this celebrated and basic literature is cited or mentioned and Arthur's whole discussion is framed as if a *tabula rasa* was being confronted as his auditor and reader. This is a shame, since he does have interesting things to say, although none of them original nor presented in any convincing way.

But even before a reading of this chapter as a manifesto for viewing economics as a cognitive science, there is that starting point about the black box between the problem and its solution, in economics ([18], p.51):

> Whether one sees economics as inherently difficult or as simple depends on how one formulates economic problems. If one sets up a problem and assumes rationality of decision making, a well-defined solution normally follows. Economics here is simple.: from the problem follows the solution. But how agents can arrive at the solution is a black box; and whether indeed agents can arrive at the solution cannot be guaranteed unless we look into this box. If we open this box economics suddenly becomes difficult

[42] As if a case needs to be made after a lifelong effort by Herbert Simon, his colleagues, collaborators, students and followers! Incidentally, not a single reference appears to any of the massive amount of work done on economics as a branch of the broader field of cognitive science.

Are there not intrinsically difficult economic problems, independent of the way they are formulated? Suppose we formulate economic problems combinatorially or as diophantine decision problems. No amount of black box peeking would make provably unsolvable combinatorial problems or formally undecidable diophantine decision problems, of which there are demonstrably many, solvable or decidable - whether the economic agent is rational or not, whether the rationality is procedural or substantive, whether context is taken into account or not. In other words, there are intrinsically hard problems, unsolvable or undecidable, that are independent of the cognitive mechanisms for the mind we may envisage, even in ideal worlds.

Secondly, it is also easy to show, without using any kind of 'deductive formalism', even assuming the kind of rationality assumed in economic theory is not remotely sufficient to *solve* a whole host of problems. Without a clear definition of what one means by *methods of solution* or specify more exactly what is meant by '*arrive at the solution*' the above homily is meaningless. Suppose the theorist looks for effective methods of solution and the problem is such that the domain over which it is defined is recursively enumerable and not recursive, as the domains of many sensible and even trivial games are (for example simple, two-player, arithmetical games such as the classic Rabin games, [75]). Then, it is easy – for anyone who knows enough classical recursion theory - to show that a solution exists but no amount of black box knowledge, at least as currently understood research on cognition would allow, enables one to find ways of 'arriving at effective solutions'. Of course, many unsolvable or undecidable problems can be solved and decided by assuming the existence of magic carpets or Aladdin's lamps and chanting, with Ali Baba, of abracadabras to 'arrive at solutions'. But I shall suppose that Arthur's black boxes are about scientific hypotheses and not about fairy tales, although the discussion does not make the distinction very clear.

Arthur does not discuss, define or explain, anywhere in his paper, what any of these things mean or what he understands by them, let alone define what he means by a rational agent[43] or problem. Indeed, such care is not exercised in any of the contributions to this volume even though cavalier references are made to bounded rationality and induction. Simon must be turning in his grave.

[43]It is one thing to assume that the discourse is about the standard definition of rationality as an economist would understand it; it is quite another thing to indulge in a discourse about cognition without specifying the kind of *agent* - i.e.,the nature of the mechanism that is encapsulated in an ideal agent's cognitive system through which processes of rationality are effected

However, at a more basic level, there is a different way to circumvent black boxes when formulating economic problems. This is to work within the mathematical framework of some version of constructivism, whereby the demonstration of the existence of a solution to a problem is not separated from the method with which one is to find the solution. That does not mean that the method devised, usually a constructive proof, will be the one that the mechanisms of the mind will or could implement. If mathematical economics or economic theory had always been formalized in terms of some version of constructivism, or even recursion theory, then Arthur's black box, at least as described above, would not be there at all. But that does not mean cognition is unimportant for economics.

Arthur's 12-page chapter is divided into five sections on, respectively, *Notions of the Mind*, *The Mind as a Fast Pattern Completer*, *Modeling the Cognitive Process*, *Cognition and Graduate Economic Education* and *Do Issues of Cognition Matter* - in addition to the opening section.

The first section is essentially a potted collection of disjointed remarks about associative memory and the problems of context-dependent interpretation issues that were dealt with by legions of philosophers of the mind and epistemologists, in both western and eastern traditions of philosophy, long before Kant and even throughout the years between Kant and the twentieth century[44]. For Arthur to state that 'we construct meaning by the associations we make' is 'a point that *starts* to get recognized in philosophy in the 1700s by Kant, but isn't fully articulated until the twentieth century' (ibid, p.53; italics added) is, surely, slightly absurd.

In the next section we face, from Arthur's imaginative pen, the twin assertions that 'in cognition, association is just about all we do' (p.54) and, a page later, 'cognitively, association is the main thing we do'. It would help the reader if the author makes up his mind without dithering from page to page, but perhaps Arthur is himself not sure what exactly 'association' means. However, in a muddled sort of way, the two points he wishes to make in this section, by concentrating on associative memory, seem to be a substantiation of those two pet Santa Fe hobby horses: 'lock-in' and 'emergence'. With this I have no quarrel and, indeed, agree without qualification. But I doubt the message is made with the clarity that is necessary or possible. A simple and clear example, like the host of them that can be found, for example, in part 4 on 'Problem Solving' in [90], illustrating how, in a problem solving context, memory, representation and context are brought into play would have made

[44]Obviously one of the earliest works in the western tradition is Aristotle's small book entitled *On Memory and Reminiscence*(cf. [51], in particular, p.3)

it very clear in what sense an emergent mind, locked-in by experience, invokes associative memories and implements cognitive processes to solve (or decide to give up attempts to solve) a well-formed problem.

The third section, 'Modelling the Cognitive Process', is a rehashing of the method codified by Kenneth Craik in 1943 but, of course, no reference is made to that pioneer of what came, eventually to be called 'cognitive science'. Craik coupled, felicitously, representation and computation in one fell swoop, and provided a theory of *The Nature of Explanation* [19] that encompasses 'the scheme [Arthur] is suggesting' (p.57). It may well be useful to quote from Craik's classic work, at least to provide a venerable pedigree for Arthur's ahistorical rendering of an important idea [41],p.3:

> If the organism carries a 'small-scale model' of external reality and of its own possible actions within its head, it is able to try out various alternatives, conclude which is the best of them, react to future situations before they arise, utilize the knowledge of past events in dealing with the present and future, and in every way to react in a much fuller, safer, and more competent manner to the emergencies which face it.

Craik's 'organisms', 'actions', 'model', etc are all precisely defined (in fact recursively) and we are not left trying to lift ourself up by our own shoelaces[45]. Moreover, not only did Craik spell out very precisely what should be meant by 'try out various alternatives', but legions of cognitive scientists, inspired by his work, have refined and implemented his suggested mechanism for *learning* in recursive procedures of increasing power. Even a nodding acquaintance with this work may clarify facetious claims about an 'ecology of belief' and put it in the historical perspective that is being advocated when a valid case is being made for 'lock-in'.

'Can such a scheme be put in practice in economics', asks Arthur (p.58), of his own less precise but analogous suggestion. The answer is, of course, in the affirmative as was demonstrated by more than a half-a-century of work after Craik by cognitive scientists, many of them working in economic contexts and

[45]I used, with abandon, the word 'bootstrap' instead of 'shoelaces' till Efron [24]came around and introduced 'Bootstrap Methods' and the econometricians began using the word that had been popular among macroeconomists who had trouble with providing a theory of the rate of interest! I have, now, to be more careful - particularly in writing about a chapter immediately after one in which such methods were advocated for pattern detection - about using the word 'bootstrap', lest the unwary reader starts looking around for evidence of its econometric connotations!

departments[46] - but Arthur refers (p.58), to substantiate this framework and its successes and desirability to two of his own writings and Sargent's 'Ryde Lectures'. This is both ungenerous and disingenious.

The fourth section is rich with interesting and feasible strategies for enriching the curricula and teaching strategies of graduate economics education. However, whether, even the sympathetic reader could concur that these suggested strategies emanate from Arthur's earlier discussion of the contents of the black box of cognition - 'benefit from the insights of cognitive science' (p.58) - is a moot point. I cannot disagree, even if I tried, and I doubt any sensible person, whether wedded to economic theory as conventionally taught or not, will disagree with the eminently commonsense prescription of the last section: that context is important in any problematic situation and methods for resolving such situations must invoke various associations, culled from historical experience, mediated and disciplined by theoretical frameworks and meliorated by sympathy and empathy for the actors and players in the drama that is created by the situation. I am very sceptical that the ungenerous and ahistorical waffling on the cognitive black box of the previous 11 pages was necessary for this perfectly reasonable prescription.

As I mentioned above, one of the fundamental metaphors of the Santa Fe vision of complexity, its definition, its genesis and its study is provided by the sandpile model, popularised in an illuminating and pedagogical way in Per Bak's classic text: **How Nature Works** [9]. The next chapter in the book, titled 'Looking Backwards: Complexity Theory in 2028', by Frederick L. Pryor, takes an amusing, but believable dig at the apparent lack of modesty displayed by the Santa Fe visionaries, such as Bak, who seem to 'take all reality as their subject matter'. The editor should be warmly applauded for placing Pryor's measured irony towards the aims and achievements of a Santa Fe vision immediately after Arthur's untrammelled enthusiasm and confidence. The brief chapter (it is only seven pages long) is divided into four sections: Future of the Moment, Problems in Complexity Theory (further subdivided into two sub-sections titled, respectively, 'Complexity Theory as a Disposition' and 'The Narrowness of Complexity Theory), Application of Complexity Theory and A Brief Conclusion. The style is strongly reminiscent of Leijonhufvud's celebrated satirical piece on '*Life Among the Econ* [56]. Just as the latter was a satirized anthropological tale about the calisthenics on 'how economists do economics', this is about a similar anthropology of how the Santa Fe complexity theorists do complexity theory, and why they failed and

[46] It is becoming tiresome to keep on referring to Simon, something I will have to do throughout this essay, in connection with almost every chapter!

their style faltered and all traces of their existence disappeared within thirty years. Beneath the satirical anthropological narration there is an undercurrent of serious scepticism that is worthy of close attention.

Pryor identifies, correctly I believe, the current enthusiasm for complexity theory as 'a rallying cry for a variety of economists dissatisfied with neoclassical economics' for diverse reasons. On the positive side, he lists the issues, metaphors and methods developed and used by the three classes of complexity theorists he names, in the style of 'Life Among the Econ', as the 'Santa Fe clique', the 'esoterists' and the 'humble practitioners'. Thus he cites with approval their emphasis on positive feedback, adaptive processes of adjustment between a large collection of heterogeneous agents, the study of disequilibrium dynamics, the broadening towards an inclusive human psychology (presumably towards a step in the direction of a more realistic definition of rational behaviour), and so on. I was not surprised to note that Pryor seemed to take the view that these emphases and advances were achieved by 'taking advantage of the advances in mathematics and computing that allowed economists to tackle more complicated subjects' (p.65). He does not, however, elaborate what kind of 'advances in mathematics and computing' allowed this to happen and I for one would have been interested in a few lines on his thoughts on the subject. This is because, while agreeing with this view, I also think that it goes against the relentless attack against 'deductive formalism' that is almost another 'rallying cry' that unites the essays in this and its companion volume. The advances in mathematics and the advances in the theory underlying computation have not come about by means of using inductive or abductive methodologies; nor by pattern detection and statistical inference; nor even by experimental techniques. They have come about by utilizing the time-honoured and humble methods of deductive formalism. Surely, there is scope for another anthropological tale on this curious dissonance. My own metaphor for these complexity evangelists who seem to be nihilists on deductive methodology is that of the lofty meat-eater who condemns the lowly butcher and is passionately against vivisection.

Pryor makes, also, a convincing case on the narrowness of complexity theory by pointing out that organizational complexity is not seriously dealt with in the current complexity visions. But, then, surely in what may be called 'classical complexity visions' or, more accurately, 'vintage complexity visions', *that* was exactly the field spawned, nurtured and brought to supreme completion by Simon in a research program that began with his classic doctoral dissertation ([88][47] and did not stop till his death - just as this book and its companion volume were being published. Almost the whole of that vintage tradition was set against the backdrop of the decision conundrums of a large collection of agents in a complex organization and the economy as a network of such organizations. The devising of concepts such as 'bounded rationality', 'satisficing', the founding of the field of *behavioral economics* [89], the distinction between *procedural* and *substantive rationality*, and much else, in constant use by the 'rallying criers', emerged (sic!) from that tradition which was faithful to the organizational underpinnings of economic society and whose origins can even be traced in the fertile imaginings of that visionary of computing: Charles Babbage.

Two pungent points summarize Pryor's strictures against the ostensible results of complexity theory. Firstly, since 'the complexity approach can explain almost everything ... it can [therefore] explain nothing'. Secondly, 'proving that some weird event is possible does not really explain why it has occurred since such results can be obtained by other models as well'. Pryor does not advocate, therefore, that the complexity theorists should also provide us with a metatheory of model selection. He does end, with supreme irony, on one high note in favour of the complexity theorists. In the one instance where humility was advocated by the complexity theorists they failed miserably (p.68):

> Perhaps the most lasting contribution of all streams of complexity theory was the realization that a bit more modesty on the part of economists would be appropriate when they are making policy proposals because they might not fully understand the complexity of the situation. Since modesty never impressed policymakers, the influence of complexity theorists and practitioners began to wane.

Is Pryor, then, suggesting that the brimming confidence, bordering on the same lack of modesty of the complexity visionaries, of the Friedmanites and the Lucasians, buttressed by their primitive, non-complexity theoretic theoretical technologies and fictions, is sufficient to succeed in inculcating the same message in the minds of policymakers - as they so successfully seem to have done? After all, not many pages after Pryor's essay there is an Austrian claim, by Koppl, to this same mantle, with equal confidence and lack of modesty. So, in what sense can the complexity theorists, whether they be the Santa Fe clique, the esoterists or the humble practitioners, claim that complexity theory contributed anything novel? How many different ways must we

[47] Indeed, the subtitle of this book was (is): *Decision-Making Processes in Administrative Organizations*

be educated that more modesty is desirable before an economist ventures into the policy arena?

It is appropriate to turn to the next chapter at this very point because it deals with exactly that topic: *'Complexity and Policy'*, the title of the chapter by Brock and Colander and their conclusion on the interaction between the two is precisely that which was given the one positive nod by Pryor.

Brock and Colander mercifully inform the reader exactly which strand of the Santa Fe lineage they build on for their reasoning in this chapter:

> As emphasized throughout this volume there are many different strands of complexity research even when one focuses on the Santa Fe approach. In this article we will focus on three of these strands. (1) Urn process models to economic dynamics, especially in industrial evolution in increasing returns-type industries . . . ; (2) pattern formation dynamics . . . ; and (3) evolutionary dynamics and inductive theorizing in contrast to deductive theorizing . . . .

I begin with the same misgivings that perplexed my reading of the earlier essays (and that continued to perplex me throughout the rest of the reading of this book and its companion volume): all of the mathematics that the Santa Fe approach relies on is derived by deductive theorizing. So, what are we to make of it? Take, for example, the reference under (1) above to the paper by Dosi & Kaniovski ([22]). If a diligent reader of this chapter by Brock and Colander went back, as I did, in good faith, to [22], so as to be equipped with the necessary theoretical technologies, it is unlikely that such a reader would get beyond the first few lines of the first technical section of that paper. For there we find [22], p.99, stating that:

> In the general case one needs a specific mathematical machinery to describe this "set of zeros".

At this point [22] refer[48] , in turn, to 'Hill et.al (1980)' and 'Arthur et.al (1987b)' for the 'mathematical machinery'. This gullible reader, keeping in mind that Brock and Colander claim to rely on 'inductive theorizing in contrast to deductive theorizing', went further back, as indicated in [22] to Hill et.al and Arthur et.al and found that *the mathematical machinery* in both of these references is replete with *deductive theorizing*. I could tell a similar story about

[48] A similar melancholy story can be told with respect to the other reference in (1), 'Arthur, 1989'. In that paper, [5], we are redirected to the same two papers to which [22] redirect the poor reader for 'the mathematical machinery' (cf., [5], footnote 8, p.26)

going back to the references under (2), above. One cannot even begin to read the fascinating paper by Newell ([66])to which Brock and Colander refer (and the wonderful book by Whitham ([103] to which, Stein, in turn, refers) without encountering a 'deductively theorized mathematical machinery' underpinning an analysis of patterns and their detection. I shall not bore the reader with explicit examples of where in Newell (or Whitham) the 'deductively theorized mathematical machinery' is invoked and where pure induction is sufficient, simply because this task is a lifelong research project of its own[49]. The reader should get the message by now: have a large barrel of salt to pinch from when reading any admonishing of 'deductive formalism' in any writing emanating from the Santa Fe stables, particularly the economic wing of it.

If one can get beyond the first instructions and guidance, there is much that is sensible, and even interesting and wise, in this well written chapter.

The authors correctly point out that their characterization of a Santa Fe approach to *a* complex adaptive systems analysis had to be spelled out, in terms of the above three underpinnings, because there are other, rich and flourishing approaches to the same subject. They also point out, again with much truth I believe, that it is not possible to characterize and differentiate the various available approaches to the study of complex adaptive systems on the basis only of methodological considerations. Thus, they add to the above three-fold basis of the Santa Fe approach the caveat that their:

> [C]omplex adaptive systems approach is different from the conventional approach in that it takes as a first principle that complex adaptive systems do not yield to linear methods of analysis and tend to generate 'emergent structures' that come as a surprise to the analyst, whereas the conventional approach tends to assume that our economy is subject to linear methods of analysis ([18], p.73-4).

Apart from flogging that poor, dying[50], 'deductive formalistic' horse, the essays have this other pet strawperson: *linearity*. Brock and Colander say this even before the reader has had time to absorb an immediately earlier caveat that covers (2) above with the qualifying statement that the 'pattern formation dynamics' they consider are, apart from relying on the methods developed in Newell's fine paper also based on 'Krugman's 1996 use of Turing-type models'.

[49] As any constructive mathematician would confirm when disputing any claim by a classical mathematician who claims her result is constructive, without checking the numerous lemmas and corollaries, and their reliance, in turn on a host of assumptions and axioms, on which a final, allegedly, constructive, theorem depends.

[50] Of boredom?

But the Turing [95] model *is linear*! Indeed, the finesse, as always in Turing's work, was how the 'Turing bifurcation' was utilised so effectively, in a linear model, to generate 'emergent structures'. Either they or Krugman [54] (or, more likely, both) have not done their homework as diligently as they expect their readers to do[51].

This 23-page chapter is divided into nine sections in addition to an introductory few pages. This reviewer found section 2, titled 'How Complexity Changes Economists' Worldview' most illuminating. The authors list six ways in which a complexity vision brings about (or may or should bring about) a change in the worldview that is currently dominant in policy circles. The latter is given the imaginative representative name 'economic reporter', a person trained in one of the better and conventional graduate schools of economics and fully equipped with the tinted glasses that such an education unimaginatively provides: a worldview for policy that is underpinned by general equilibrium theory and game theory. These are the bright young things that go around the world seeing Nash equilibria and the two welfare theorems in the processes that economies generate, without the slightest clue as to how one can interpret actions, events and institutional pathologies during processes and their frequent paralysis.

Brock and Colander, as complexity theorists, aim to take away the intellectual props that these economic reporters carry as their fall-back position for policy recommendations: general equilibrium theory and the baggage that comes with it (although, curiously, they do not mention taking away that other prop: game theory, which, in my opinion, is far more sinister). With this aim in mind the six changes that a complexity vision may bring about in the worldview of the economic reporter are as follows:

- With a complexity vision the most important policy input is in *institutional design*.
- The complexity vision brings with it an attitude of *theoretical neutrality* to 'abstract debates about policy'.

[51] Indeed, Krugman's highly popular book is replete with the lack of modesty that Pryor, with exquisite irony, apportioned the Santa Fe visionaries. For example Krugman states, without the slightest hint of irony in the tone or content, that [54], p.7: *[H]e may be the only economist in [his] generation who has ever heard of [nonlinear business cycle theory]'* of the Hicks-Goodwin-Kaldor variety! I can think of at least two authors in this book who were thoroughly familiar with this tradition to the extent even of having contributed to its development and extension in fertile ways: Duncan Foley and Barkely Rosser, Jr

- The 'complexity-trained policy economist' will try to seek out the boundaries of the equivalent of the basins of attractions of dynamical systems - i.e., equipped with notions of criticality and their crucial role in providing adaptive flexibilities in the face of external disturbances, the complexity trained economist will not be complacent that any observable dynamics is that of an elementary, characterizable, attractor.

- There will be more focus on inductive process models than on abstract deductive models.

- Due to the paramount roles played by positive feedback underpinning path dependence and increasing returns in the complexity visioned economist, the attitude to policy will be honed towards a temporal dimension to it being given crucial roles to play.

- The complexity worldview makes policy recommendations less certain simply because pattern detection is a hazardous activity and patterns are ephemeral, eternally transient phenomena.

To this eminently reasonable list I would add another precept that was the backdrop against which Brock and Colander reasoned. The traditional closure of economic theory is in terms of *preferences*, *endowments* and *technology*; they would add institutions to this tripod, something with which I agree without reservation. Their reasoned motivation is impeccably admirable, and I believe North, Simon, Day, Nelson, Winter and every variety of the institutionalist school would also approve it. I also agree with the authors that this gives both a temporal dimension that is historical and dynamic that is sadly lacking in orthodox theory. As they put it (p.79):

> Much of deductive standard economic theory has been directed at providing a general theory based on first principles. Complexity theory suggests that question may have no answer and, at this point, the focus of abstract deductive theory should probably be on the smaller questions - accepting that the economy is in a particular position, and talking about how policy might influence the movement from that position. That makes a difference for policy in what one takes as given - it suggests that existing institutions should be incorporated in the models, and that policy suggestions should be made in reference to models incorporating such institutions ....

Was this not, after all, the message in the *General theory*? 'Accepting that the economy is in a particular position' of unemployment equilibrium and devising a theory for policy that would 'influence movement from that position' to a more desirable position. Such a vision implied, in the *General Theory*, an economy with multiple equilibria and, at the hands of a distinguished array of nonlinear Keynesians, also that other hobby horse of the Santa Fe visionaries: 'positive feedback' - in more conventional dynamic terms, locally unstable equilibria in a dynamical system subject to relaxation oscillations. In addition, in the early nonlinear Keynesian literature, when disaggregated macrodynamics was investigated, there were coupled markets, but the mathematics required to analyse coupled nonlinear differential equations was only in its infancy and these nonlinear Keynesians resorted to *ad hoc* numerical and geometric methods. So, we are in familiar territory, but not the terrain that is usually covered in the education of the economic reporters.

Were these precepts also not the credo of the pioneers of classical behavioural economics: Simon, Day, March and Co? Studying 'adjustment processes', eschewing the search for first principles, underpinning economic theoretic closures with institutional assumptions, enriching rationality postulates by setting agents in explicit institutional and problem-solving contexts, seeking algorithmic foundations for behaviour[52]. Was there ever an economic agent abstracted away from an institutional setting in any of Simon's writings? Were not all of Day's agents, in his dynamic economics, behaving adaptively?

So, these precepts have a noble pedigree, denied by Brock and Colander. But the problem with these precepts is that, by definition, the economic reporters are trained in so-called conventional economics[53]. At which graduate school would training in the complexity vision and its tools and methods be imparted to the 'economic reporter'? And would it be in addition to that which made the person an 'economic reporter' or as an alternative? If the

[52]Which, automatically, brings with it undecidabilities, uncomputabilities and other indeterminate problems that can only, always, be 'solved' *pro tempore*, aiming to determine the boundaries of basins of attraction numerically, and so on. A list that not only encompasses the Brock-Colander set of six-fold precepts but also one that is far richer in inductive content and retroductive realization.

[53]By now the reader would not be surprised to find Brock and Colander lumping together Herbert Simon, Richard Day, Richard Nelson and Sidney Winter with the general equilibrium theorists and other orthodox economists to define the class of 'conventional economists' (pp. 74-5). The reason given gets *curioser and curioser*(*pace* Alice): "We classify such works in evolutionary economics as 'conventional' because the themes they sounded were around before the advent of the use of techniques such as urn processes, pattern development theory, complex adaptive systems theory and artificial life techniques in the field of economics". All I can say about this almost unscholarly remark is that Brock and Colander must be talking about imaginary doubles of these pioneers! (p.74-5)

former, how many years of graduate study are the authors contemplating! If the latter, the person is, by definition, not an 'economic reporter'. Such conundrums can be added as caveats for each of the six precepts.

But that does not detract from the value of the observations. One can only hope that by osmosis or retraining after graduation, appealing to the wisdom that the confrontation with reality may have imparted to the intellectual fibre of the economic reporters, they may absorb these precepts, at least during their waking and working hours.

Readers of this stimulating chapter may be forgiven for coming away with some confusion, even after absorbing much enlightenment and entertainment. For, just as this reader is about to leave with some sympathy for the economic reporters who had been 'conned' by their less enlightened teachers, in their graduate schools, we read (pp.81-2):

> We want to emphasize that it is primarily the economic reporters' worldview that will be changed. Sophisticated economists have long ago given up a simplified general equilibrium worldview.

But only a few pages earlier we were told that the economic reporters were trained by misguided sophisticated economists and their stone age economics! I came away wondering where, in Lucasian writings, I will find an absconding from 'a simplified general equilibrium worldview'.[54]

In chapter 6, 'Policy Implications of Complexity: An Austrian Perspective', Roger Koppl promises to 'outline an "Austrian" approach to the policy implications of complexity'. He identifies, correctly I believe, the Austrian insistence on the 'knowledge problem' - the empirical[55] fact that at no level of an agent, whether as a government regulator, planning entity or even as a rational individual agent, can anyone have all of it that is necessary or sufficient in any decision theoretic environment whatsoever. This 21-page chapter is divided into seven sections and an appendix, together with the introductory page; some sections are also divided into subsections. In the end Koppl's aim is not just to 'outline an "Austrian" approach to the policy implications of complexity' but to suggest that the policy stand emanating from a Santa Fe

[54]The usual quota of infelicities mar the reference list in this chapter, too. The Marcet, McGrattan, Sargent paper is duplicated; the Economic Journal has become the *Economics* Journal in the Dosi reference; Sargent's *Ryde Lectures* seem to have been published by, both, OUP and the Clarendon Press and it would have been safer to refer to the University of Siena rather than incorrectly in Italian, and so on.

[55]I also believe theoretically sustainable with any theory of knowledge, its representations and its epistemological mechanisms, at least so far as we know at the research frontiers of the cognitive sciences, broadly conceived.

vision of complexity, outlined, for example, in the previous chapter by Brock and Colander, should be 'informed by Austrian economics'. I think this is a good idea and I also think Koppl succeeds admirably in the task he has set himself. One would, of course, wonder whether the Santa Fe visionaries would or should also be asked to be informed by Post-Keynesian Economics, Marxian Economics, Institutional Economics (of all varieties), Behavioural Economics (classic and bastard varieties), New Classical Macroeconomics, Evolutionary Economics (of the game theoretic and non-game theoretic varieties), Computable Economics, Experimental Economics, Structuralist Economics, and so on. This would leave little time for the Santa Fe visionaries to pursue their own agenda and it is better that they concentrate their energies on trying to disabuse the conventional theorists of their pernicious habits of thought, in particular because it is they who warp the minds of the young.

Now, Koppl confesses, quite candidly, that 'the Austrian economist [he] will draw on most is F.A.Hayek'. I, having sat at the feet of the Cambridge maestros of capital theory, wondered immediately whether Koppl would also 'draw on' that great Austrian economist's defense of Austrian capital theory? After all, the Hayek who, as Koppl correctly points out, dedicated[56] his *Roads to Serfdom* to 'The Socialists of All Parties', was also the author of *The Pure Theory of Capital*, *Prices and Production* and *The Monetary theory of the Trade Cycle*. Every single one of them is impeccably couched in general equilibrium terms in economies peopled by super-rational agents who can calculate and impute all sorts of intertemporal values and costs of any dimension whatsoever and Dr Pangloss and Maxwell's Demon would have their illusions amply realized. Now, I have no objections to schizophrenia but I am disturbed by memories of that nursery adage about 'sauce for the goose being sauce for the gander'. You cannot pick and choose areas where equilibrium is comfortable and others where imagination is allowed to run riot. Koppl is talking about informing the Santa Fe visionaries on 'Austrian Economics', to which also Lucas adheres himself when he constructed his equilibrium model of the business cycle([61], p.215). Are the Austrians to be allowed to assume super-human knowledge assumptions when they do capital theory but not when they indulge in nihilistic policy recommendations? I find this absurd and would like to remind these Austrians of that fine old Biblical admonishment before embarking on sanctimonious pronouncements: *Physician heal thy self*!

[56]Koppl writes that Hayek *'devoted'* this book to those to whom he had *dedicated* it! My reading of that impressive book did not leave me with the impression that it was 'devoted' to the 'Socialists of all parties'.

Leaving conceptual and philosophical aesthetics aside, Koppl has sensible things to say, not always clearly nor rigorously, but often with enough confidence to convince the reader. For example in section 2, 'The Austrian Critique of Socialism' , couches it - the critique - by interpreting, implicitly, socialism to be a planned system and the latter framed as a general equilibrium problem. Koppl then invokes a result on the intractability of a particular algorithm[57] - Scarf's algorithm - to compute a Walrasian equilibrium. This does not mean, as Koppl thinks and states, that 'general-equilibrium systems are intractable'; it simply means that a particular equilibrium concept and a particular algorithm to compute it are intractable in a very specific sense. Had the general equilibrium system been framed from alternative starting points and an alternative mathematics the results may have been the converse. In other words, the intractability results applying to the Scarf algorithm to compute a Walrasian equilibrium does not mean that, for example, Sraffa's algorithm to prove the existence of a standard commodity in his kind of classical equilibrium system is intractable.

But the kind of complexity implied in these intractability results - computational complexity - is not the kind that is under discussion in the Santa Fe vision. Indeed, the Santa Fe vision, except for lip service via references to evolutionary computation, pays no attention whatsoever to the computable model underlying their simulation frameworks. This is a huge lacunae in the Santa Fe vision and Koppl, via the Socialist Calculation debate and with the Austrian stand on it could have made a valuable contribution towards a constructive critique of policy feasibilities. But he skirts the issue with irrelevancies about intractable general equilibrium systems and misses a golden opportunity.

Koppl, finally, defines 'Big Players' as those who, due to the inevitable ambiguities in the language in which laws are framed, are able to exploit them due to their particular positions as enforcers, viz., as the guardians of the law, such as the courts and the legal system in general or discretionary public agencies. But, surely, the ambiguities in the language of the law and the legal system in general can and is exploited also by private agents. Witness the litigious nature of modern society as evidence for this. However that may be, Koppl, then, goes on to claim (p.107):

[57]Koppl does not state the result he invokes even with this amount of precision but intractability the way he states it refers to non-polynomial time computability by a Turing Machine or its equivalents by the Church-Turing Thesis. This must mean either that the general equilibrium system is framed, *ab initio*, in recursion theoretic terms or the algorithm to compute a classically proved equilibrium existence theorem is being considered. In fact, if the former formulation was opted for it is easy to prove the uncomputability of a Walrasian equilibrium and the intractability question does not even arise.

> My‘ counting argument’ is a simple combinatorial model to show that private actors subject to Big Player influence cannot formulate a complete model of the Big Player whose decisions they would predict.

The ‘Appendix’, pp.114-5, is devoted to an alleged proof of this statement, but it is hopelessly flawed, at least in the version given here. Moreover, complexity is defined so vaguely that the relevance of this result and the framework in which it is stated for the Santa Fe vision or as a stricture against discretion in policy discussions is hard to fathom. However, the other problem is that he now defines the ‘Big Player’ in terms of entirely different characteristics (p.114): such a player influences events in the market; s(h)e is ‘insensitive to profit and loss’; s(h)e ‘acts on the basis of discretion, not rules’. Obviously the first characteristic, in a market economy, is about non-price taking agents (on either blade of the Marshallian scissors); the second is about insensitiveness to market signals; and the third is about arbitrary exercise of power of some sort. None of this has anything to do with the ambiguity of the framing of laws, with which the main text was concerned when first defining the ‘Big Player’.

But even granting this infelicity, there is a flaw in the alleged ‘counting argument’ in a ‘combinatorial model’ to show ‘why it is impossible to form a complete model of the Big Player’ - unless it is assumed that the so-called indicators of behaviors of the Big Players form a recursively enumerable set that is not recursive. Contrary to what is stated in the footnote attached to the alleged proof, the relative ‘sizes’ of R and L are irrelevant to the ‘counting argument’. A Turing Machine model can be constructed to substantiate this last point. The statement, in the footnote, that a transfinite R, means, for his result to hold, an even greater L takes us into the treacherous terrain of comparing infinities. But no fear; he is, happily, wrong and we do not have to battle in any of Cantor’s paradises. Consider a Turing Machine with L states and a bi-infinite tape and the usual mechanisms to activate it. Given the ‘set of possible environmental states’, encode them in the usual way in the input tape. The rules, or the state transition matrix of the Turing Machine specify, for given environmental states read by the tape head of the Turing Machine and the machine state when reading, the R behaviors of the Big Player. Such a Turing Machine with L states can be simulated by a much smaller machine, say even by a 2-state machine.

Had Koppl couched the formalism of his model more rigorously, his intuition could have been substantiated in a stronger sense: it is not that ‘small players cannot create complete models of Big Players’; it is simply that no player can create complete models of any player, irrespective of size’, provided 'complete' is defined carefully. All one would need to use would be the famous result on *Halting problem for Turing Machines* to achieve a good variant of what Koppl's intuition is driving at, motivated by Austrian considerations on knowledge, its representations and its essential ambiguities - the undeniable incompleteness of decision procedures even in an entirely deterministic setup. It is a pity that he did not bolster his otherwise sound Austrian-theoretic intuitive arguments against unnecessary enthusiasm for policy activism with a more rigorous framework that would speak to the Santa Fe visionaries in their own language[58].

Incidentally, the reference to a nice point made by Adam Smith in the Theory of Moral Sentiments,, given on p.104, cannot be found on the page that Koppl assigns to it in that fine book; in the *Liberty Classics* edition to which Koppl refers, the text part of the book ends on p.342 and Koppl refers to the quote as bing on p.380-1!

Part three, 'Teaching the Complexity Vision in Economics: General' comprises four chapters, by Colander, Koppl, Stodder and Foley, respectively.

Colander's chapter on 'Complexity and the Teaching of Economics' deals, primarily, with the important issue of possible strategies for introducing, at the level of 'principles texts' in economics,'the Santa Fe approach'. This assumes, of course, that there is an unambiguous, clearly definable, 'Santa Fe approach' to economics. I doubt there is such a thing, even as evidenced by the disparate visions emerging from the views expressed in this and its companion volume being reviewed here. But for the sake of discussion, and *pro tempore*, let us grant the existence of this chameleonic creature.

Colander is of the view that 'there are essentially two ways in which the Santa Fe vision can be introduced into economics texts': the first way is to write a text, *ab initio*, logically developing the central ideas, concepts, methods and tools of the Santa Fe approach; the second is to 'knead the Santa Fe complexity ideas into simple mantras and conceptual pictures that are sufficiently compatible with existing models and conceptual pictures so that these ideas are integratable into existing texts'. It is in this latter context that Colander's '15% rule' comes into play: 'a textbook can differ from its previous edition by 15 percent each time' (p.127). This rule takes into account teacher competency, willingness, market tolerance and a few other identifiable elements characterizing the textbook market. Much of the discussion surrounding the 'derivation' of the rule would make sense to most teachers of principle and intermediate level economics and I have no doubt many will sympathise and

[58]Or, rather, in the language of the logic that should have been natural for the Santa Fe visionaries: recursion or model theory.

agree with Colander on some such rule-of-thumb as a strategy for writing texts at such levels. A rapid rolling over of editions and a simple calculation shows, on 'stationary' assumptions of developments of orthodoxy and the Santa Fe vision, that the 15% rule would, by the 6th edition, have a content of well over 50% reflecting the latter approach and more than two thirds by the 8th edition - if neither obsolescence nor dominance of one sort or another had not overtaken the whole issue by then. Assuming a rolling over of editions every two years, this means, within a decade or so, an increasing number of economics majors would be increasingly receptive to thoughts and tools reflecting the Santa Fe vision and, more importantly, a growing number of teachers may be less unwilling to teach such things. There should, therefore, be some kind of cutoff point at which the 15% rule could become higher and, in fact, one could work out[59] an 'endogenous' rate of growth of kneading of orthodox textbook contents.

I have, deliberately, spelled out the rosy scenario; but I do not believe a word of this. Let me explain. Colander, for all that he may have his own, individual, 'learning curve', culled out of his considerable teaching and textbook writing experience, must be living in a kind of cloud cuckoo land of student capabilities at the principles and intermediate texts. Secondly, for frontier topics, however basic, an alternative scenario of absorption can be given that is historically sustainable, even if unpalatable for those in a hurry and who think they possess the 'true vision' - as the Santa Fe visionaries seem to think. Let me expand upon these two themes.

Does Colander seriously believe that a principles or intermediate economic text can include material on 'the central ideas of iterative dynamic processes, . . . , Mandelbrot and Julia sets' and, if we are to believe the message in Brock's earlier chapter, Bootstrapping, ARCH and GARCH models and methods, the idea of stable distributions and power laws, the pattern detection methods underlying the dynamics discussed in Newell's paper, the Turing Model of Morphogenesis, Urn processes, and so on? Surely, these are the stuff of which graduate texts are made and his seemingly sensible 15% rule should be eminently applicable at this advanced level rather than at the lower levels at which one encounters students mortified even by the simplest of mathematical notation, let alone fractal dynamics and dynamic patterns in convective fluids. I agree with Colander that 'textbooks direct the mindset of future economists and, in doing so, influence the research the profession does' (p.122). However,

[59] Using the 'Experience' or 'Learning' curve discussed by Colander in this chapter and Rothschild in the last chapter one should be able to calculate the rate at which teachers are learning new techniques, methods and concepts and increase the 15% accordingly!

the 'research the profession does' begins at the graduate level and the contents of the graduate texts determine the tools, methods and concepts that a budding Samuelson would learn for a dissertation. Moreover, graduate students in economics or management come from a variety of backgrounds, with many hardly ever touched - mercifully! - by the hand of the conventional and boring neoclassical principles or intermediate text. It is, therefore, how the 15% rule can be applied at the level of a graduate text that would determine how many bright young things would pursue a line of research that may lead to a cumulative increase in the number of future teachers who may be amenable to adopting an alternative to the arid neoclassical vision.

Let me illustrate this with a series of leading graduate macroeconomic textbooks by an undisputed pioneer in theory, concepts, methods and pedagogy, Tom Sargent, and a particular research topic, Search Theory, as a kind of 'counter-example' case study to the Colander undergraduate, 15% vision.

Sargent has authored three highly successful graduate macroeconomic textbooks: *Macroeconomic Theory* (1st edition, 1979; 2nd edition, 1987), *Dynamic Macroeconomic Theory* (1987) and *Recursive Macroeconomic Theory* (2000). I have taught from each one of them, to graduate classes in Macroeconoics at UCLA, at the People's University in Beijing, at Aalborg and Copenhagen Universities in Denmark and at the National University of Ireland in Galway. I am no card-carrying New Classical, in fact, if anything, quite the opposite.

The first textbook did not have anything about search theory. The second book, immediately after an excellent pedagogical chapter on dynamic programming, included as the first 'economic' chapter and topic, Search Theory[60]. The third book, again after four chapters of introductions to the theoretical technologies, had as the first substantial economic topic Search Theory and

[60]This book was published the same year as Models of Business Cycles, the Yrjö Jahnsson Lectures Lucas had given in Helsinki in May, 1985 . In these lectures Lucas paid handsome tribute to McCall's search model, but underlined its mathematical underpinnings ([62], pp.54-5; italics in the original): "An analysis of unemployment as an *activity* was initiated by John McCall in a paper that integrated Stigler's ideas on the economics of search with the sequential analysis of Wald and Bellman. McCall's contribution is well-known and justly celebrated, but I would like to celebrate it a little more ..." . Lucas had, of course, integrated the six elements of labour supply via search theory, the natural rate hypothesis, the 'island paradigm' to drive a wedge between local and global information, rational expectations, olg and human captital to derive an equilibrium model of the business cycle in a research program that initiated the newclassical tradition 15 years before that. But it took another decade and a half before both Wald-type sequential analysis became, together with dynamic programming and (Kalman) filtering, part of the staple repertoire of graduate courses in macroeconomic theory. These are supplements to dynamical systems theory, which has had its own evolutionary history as part of the staple diet of macroeconomics education ever since Roy Allen's series of textbooks.

also the last chapter of the book, perhaps as an indication of a frontier subject for research students to choose thesis topics, was on search Theory. The three books included an increasing array of mathematical techniques, all pedagogically well motivated and clearly presented with examples, problems and accompanying solutions (in some cases). The most recent textbook has the qualifying term 'Recursive' added to 'Macroeconomic Theory' because Sargent wished to highlight the fact that the mathematical techniques he was using for formalizing and theorizing macroeconomic topics were (Wald's) sequential analysis, (Bellman's) dynamic programming and Kalman filtering - all of them to be implemented on time-series data and models.

John McCall, in personal conversations extending over a decade and a half, has confirmed that it was his understanding of Wald's approach to sequential analysis and a familiarity with dynamic programming, learned during his years at Rand in the late 50s and early 60s - where he shared an office with Phelps - that gave him the analytical handle with which to implement his own research program on optimal search and a search theoretic analysis of labour market dynamics. This research entered the frontier material quite early, for example via the contributions by Holt and Mortenson in the influential 'Phelps volume' ([73]) and became part of the constituents of the microfoundations drive in macroeconomics.

Why, then, has it taken almost 40 years, since its initiation at the hands of Stigler and McCall in the early 60s, for search theory to enter as a staple, stable topic, with all the mathematics necessary for its modelling and implementation empirically, packaged in a pedagogical format within the covers of a textbook, by a leading theorist and proponent of a particular vision of macroeconomics in particular, and economics in general? Is this a typical scenario and if so what were the intermediate steps that were taken in the intervening 40 years? Most importantly, when will the subject become part of the staple contents of intermediate and, then, principles texts?

Now, Colander is suggesting the converse strategy: to go from the bottom-up, so to speak, whereas search theory filtered from top-down. I believe there is a pattern to be discovered and codified here, but only after a detailed study of theses on search theory over, say, the thirty years between the late 60s and the late 90s, an investigation of the changing profile of the contents of graduate macroeconomic curricula, the equally varying profile of graduate economics faculty and their specialisations, and so on. I do not think there are short-cuts for success in such ventures. Applying those theories that underpin a Santa Fe approach to self-organisation and the emergence of spontaneous order to the emergence of sub-disciplines, say in Macroeconomics, it must be clear to a proponent of that vision like Colander that there cannot be a blueprint for success, but there may well be patterns to discover, from which strategies for modest movements in one or another pedagogical directions might be considered feasible.

Colander's chapter contains, in the section under the heading 'The Santa Fe Vision', a clear delineation of this vision by contrasting it with what can only be called 'vulgar' neoclassical economics. That the Santa fe vision is about 'emergence and process' in contrast to the neoclassical emphasis on states of equilibria; that the former is underpinned by inductive rationality whereas the latter is, allegedly, based on deductive rationality; that the emphasis is on the competitive process and not the competitive state in the Santa fe vision and the converse in the neoclassical approach; that both visions admit that the economy is complex, but the one emphasises its adaptive nature and the other its final state; the one looks for the sources of complexity in simple iterative dynamic laws and the other assumes static, linear structures[61], and so on. This is, by now, a familiar list and as implications of these two alternative visions of the economy, its agents and its workings, its conceptual characterization will be in terms of the Santa fe hobby horses of path dependency, lock-in and increasing returns in one, unified, indivisible package, multiple equilibria and the need for a kind of sequential decision making and an exploration of the solution space by simulation I subscribe, unreservedly, to all of this.

In the section on 'The Balancing of Induction and Deduction' (pp.128-9), Colander, quite suddenly and out of the blue, appeals to abduction as another conceptual prop for the Santa Fe epistemological vision:

> A Santa Fe compatible book would explore the induction/deduction distinction more carefully .... It would explain how economics, and every science, has always involved a balancing act of induction and deduction, perhaps arriving at what Charles Peirce calls abduction[62].

Does Colander seriously contemplate the possibility of including, in a principles or intermediate text, references to Peirce's definition of abduction and its possible use as a methodological principle, to justify Santa Fe practices and visions? I will have more to say about Peircean notions of abduction below,

[61]This, however, is difficult to digest, since we have been told by the Santa Fe visionaries,, almost *as nauseam*, that linear models are inadequate to generate complexity, despite Turing's demonstration to the contrary.

[62]As a matter of fact Charles Peirce eventually preferred the word *Retroduction* as his translation of Aristotle's *apagogy* ($\alpha\pi\alpha\gamma\omega\gamma\eta$); but see below, my discussion of Hoover's chapter for more details.

when discussing Hoover's chapter and shall only make a few brief and general comments here.

Methodology is a serious and full time topic, as Colander should know. It will be hard enough for students at the principles and intermediate levels to absorb the new techniques and methods that are required to implement models that reflect the Santa Fe vision. To add to these arduous requirements also a methodological baggage, particularly around the nebulous and dangerously little understood concept of abduction, must border on the reckless. If it is an evolutionary approach that Colander wishes to advocate, and this seems to be evident from the way the discussion in this chapter is structured, then - if constrained also by the 15% rule - he will have to be ruthlessly selective. If the aim is to seamlessly introduce a methodological innovation into a principles or intermediate text, then it must be to replace that tiresome dichotomy between positive and normative science that forms the pocket-size diet of current textbook nod to this serious subject. Is it worth using up the scarce ration of the 15% rule on such a replacement, on the first go? Would it not be wiser to simply delete the tiresome positive-normative dichotomy and wait for a later edition - say the fifth edition! - to make a gentle mention of the fertile possibilities of an abductive methodological underpinning for a simulation dominated approach to economic theory construction?

In the section on 'Production Possibility Frontier, Decision Trees, and Sequential Decision Making', (pp.132-3) Colander tackles, ambitiously or recklessly, depending on the way one chooses to look at it, the important issue of a formalized presentation of production theory at the undergraduate textbook level. His desiderata, so that Santa Fe ideas can be satisfied, are that production models should incorporate 'sequential decision making, increasing returns, non-linearities and learning by doing' - noble but daunting requirements for principles and intermediate level texts! At a geometric level, to sugar the above technically daunting pills, he suggests that the graphical presentation should present the production possibility frontier as an evolving curve over a non-convex set. There are, of course, several ways of doing this and almost all of them have been suggested in the literature at some time or the other. The simplest is to follow Scarf's fertile ideas of handling increasing returns to scale via indivisibilities and one of the ways of formalizing such a model is via graphs (cf. [84], [85],[86]). Another way is to extend Romer's recent ideas to formalize the production of goods and ideas 'digitally'. A natural formalization of such a suggestion is to use Turing Machine formalisms (cf. [77], [97], pp. 152-160 and [104]), which will give students immediate access to simulation models without the *ad hockeries* of starting with curves, whether smooth or not. Non-linearities, increasing returns to scale, learning by doing and sequential decision making get incorporated *ab initio*.

But just imagine the kind of preparatory knowledge that a student must have? A bit of graph theory; some recursion theory; some modicum of non-linear mathematics; and the formalisms to encapsulate 'learning by doing'. However naturally intuitive their geometric representations are, useful economic implications cannot be derived in a formal vacuum. I cannot but think Colander's students must be super-human beings; in 30 years of teaching in four continents at every possible level I have never met a class of students at any level who can absorb all this even in a whole course - let alone as one insignificant part of one aspect of one chapter of an intermediate or principles text.

Colander concludes his chapter by trying to make a case for the inclusion, in a Santa Fe dominated curriculum, of the *experience* or *learning curve* (a topic emphasised by Rotschild in the last chapter of this book, where I shall deal with it in greater detail). The surprise in these two sections, titled 'The Experience Curve' and 'The Aggregate Learning Curve', respectively, is that Colander does not refer to Arrow's classic ([2]), nor to Kaldor's *technical progress function* ([42, 43, 44]. After all, Arrow did refer to T.P.Wright's work on the 'learning', 'progress' or 'experience curve'quite handsomely in his classic paper (op.cit, p.156) and, indeed, did go on to state, quite explicitly, that he was going to(ibid; italics added):

> [A]dvance the hypothesis here that technical change in general can be ascribed to *experience*, that it is the very activity of production which gives rise to problems for which *favorable responses are selected over time.*

Why it took a further 20 years or so before these insights were incorporated into the literature on endogenous growth theory forms a chapter in the same section of the investigation I suggested for the way search theory has become a part of the staple content of graduate macroeconomics. But, of course, an avenue for the Santa Fe visionaries to include some version of the 'experience' or 'learning' curve in the envisaged textbook, maintaining the 15% rule, is via a subsection in the building up towards a model of endogenous growth theory or, even better, an endogenous evolutionary growth theory, an entirely feasible venture along Nelson-Winter lines.

In passing it may be worth mentioning that, if the 'experience curve', an empirically determined engineering cost curve, can play a role in a Santa Fe curriculum, its production counterpart, the *engineering production function* may also advantageously be put to use to bolster the appeal of a Santa fe vision.

This, too, has a respectable pedigree in economics, going back, in modern times, at least to Chenery's work in the late 40s ([14] and, intermittently, by many others, but never to the extent of becoming part of the standard economic curriculum.

But where Colander surprised me even more was in his appeal to an 'aggregate learning curve'. I was surprised in two senses; Colander paying no attention to the fallacy of composition (the chapter by Prasch in the next section is particularly enlightening on this issue) given that he subscribes to a Santa Fe vision of complexity and in his total negligence of Kaldor's technical progress function, first introduced in his growth model of 1957 but fully elaborated in the famous Kaldor-Mirrlees model of 1962. That a Santa Fe inspired economist can justify his appeal to the existence of an 'aggregate learning curve' on a woolly principle of aggregation is nothing short of a minor scandal and smacks of *ad hockery* and opportunism (p.134):

> All discussions of the learning curve that I have seen have centered on individual industries. But since the aggregate economy is a complex agglomeration of industries, there should be a corresponding aggregate learning curve for the entire economy.

Aggregation by any other name - call it 'complex agglomeration' if you will - 'smells as sweet' (*pace* Romeo). To be told by a Santa Fe pen that the whole is a complex sum of the parts and mirrors the parts is hardly edifying to the whole enterprise of arguing against reductionism, bottom-up research strategies and so on. But even if we let that pass, I was seriously disturbed that a scholar of economic thought and theory is able to state, without even a mild nod at Kaldor ([42], pp.264-6):

> This aggregate learning curve relates aggregate output with the growth in long-run productivity and thus provides a bridge between the long run and the short run in the same way that it did in the industry curve. If there is an aggregate experience curve, long-run growth depends on short-run expansion and it is impossible to separate out the two.

I suggest that Colander goes back to the drawing board and re-reads both Kaldor and Arrow so that his 'attempts to introduce Santa Fe ideas into macro' (p.134) is better and more historically grounded for his students to get a picture of an approach that is squarely in a noble, if unorthodox, tradition. But, then, I am not sure that Kaldor is a name that will resonate with anything the principles or intermediate economics student has encountered in his ordinary life as an economics major. So, Colander might as well carry on as if Kaldor's model of growth was never devised as an alternative to the neoclassical model. The student will, eventually, meet Kaldor as the originator of the phrase and conceptual underpinning behind 'stylized facts' and at that point the abductive pattern detector that is Colander, the Santa Fe visionary, can attempt to ignore another noble link with the methodology of growth economics advocated by Kaldor and practised by all and sundry in current growth theory. Pryor's next reflection, from the vantage point of fifty years hence, as a Ph.D dissertation on the origins of the Santa fe vision, may unearth these Kaldorian conceptual innovations in growth theory, as those in the companion volume to this one find evidence to place Frisch and Marshall, Babbage and Hayek and others as complexity theorist before their time, to place Kaldor in the same noble pigeonhole.

The next chapter, by Koppl, has as its theme the teaching of complexity with an 'Austrian Perspective'. I shall have to remind the reader, at the outset, of my disquiet, expressed in my discussion of Koppl's earlier chapter above, at any Austrian claim to allegiance with a Santa Fe vision without also explaining how that can square with an equilibrium approach to core issues in macroeconomics: monetary theory, trade cycle theory and, above all, capital theory.

Koppl's starting point is Colander's dilemma on the problem of how best to include a Santa Fe agenda in textbooks at the principles and intermediate level: kneading Santa Fe ideas under the constraint of the 15% rule or wholly new texts with all the risks this involve. Koppl claims that he has a 'better way' to suggest: 'The better way is an Austrian approach' (p.137) and he gives four reasons as why this is a better way:

- The Austrian approach is evolutionary and hence can be incorporated into any textbook seamlessly as 'a soft undercurrent or the main flow' (ibid);
- It - the Austrian approach - allows Santa Fe ideas to be placed at the center;
- It allows the placing of 'Santa Fe materials as supplements';
- Finally, 'it is intellectually correct';

Is there any school of thought that does not believe that its approach is 'intellectually correct'? Is it impossible to place 'Santa Fe materials as supplements' in a Marxian, Post-Keynesian, Nelson-Winter type evolutionary growth theoretical, Computable, Experimental, Institutional, Structuralist and many other economics? Can any of these be denied the status of being at least partly 'evolutionary'? Cannot the 'Santa Fe ideas' be placed at the center of the economics of most, if not all, of these schools of thought?

However, Koppl does have interesting things to say about a way to view Austrian economics that synchronises well with many aspects of a Santa Fe vision: the emergence, spontaneously, of order; the cognitive underpinnings that can be discerned from Hayek's seminal book on *The Sensory Order*; and so on.

The chapter is also liberally and disturbingly sprinkled with *non sequiturs* and other infelicities. I am compelled to highlight a few of them, because their existence in the paper makes it difficult for me to lend credence to his other, more particular, claims for Austrian economics.

First of all, it is simply not true that 'rational choice models depict people as perfect computing machines who maximize their utility functions with the greatest of mathematical ease'. As a matter of fact, if the 'standard definition' did 'depict people as perfect computing machines', then such entities would only be able to maximize utility functions that are computable. The class of functions considered in standard definitions does not work under such an enlightened constraint. It is likely that, long before any agent, depicted as a 'perfect computing machine', reaches a decision as to whether the function has being maximized, death, boredom or a fate worse than that which befell Buridan's ass would have intervened or a suboptimal choice would have been made.

Secondly, Koppl's students, faced with the 'simple exercise after having read Menger' (at the principles and intermediate levels?) would, perhaps, be still passing the bag of black and white balls to each other if they were to follow the rule described here (p.143). Koppl claims that the exercise he asks his students to undertake in class is to show how a 'network externality favours convergence to a common medium of exchange'. But his rule for the urn process is emphatically *not* a '*non-linear Polya urn process*'(p.143; italics added):

> Take a bag filled in equal parts with black and white balls. Give it to a student who draws a ball. This student replaces the ball drawn, adds another of the same color, and *removes one of the opposite color*. The first student passes the bag to the next student who follows the same procedure.

This is such an elementary misunderstanding of urn process mechanisms that I am surprised Koppl's students did not take him to task for wasting their time.

Koppl quotes, with apparent approval, Arthur's claim that 'Marshall ... did'nt have the mathematical tools to do much with' increasing returns. This approval comes in a paragraph with the opening line where Koppl thinks it might be useful to 'recall some facts of the history of our discipline'. Perhaps he should do his recalling after re-reading carefully the precise sections in the Principles to which Arthur refers whenever he invokes the Marshallian heritage on increasing returns and the advantages to the first entrant in such markets. I have quoted the complete footnote, above, in my discussion of first of the two chapters by Arthur, in this volume. The point Marshall was making about the study of increasing returns to scale industries is similar to the case once made by George Temple about the study of nonlinear differential equations: a general theory is (almost) impossible; for the moment we must cultivate each particular, interesting, nonlinear equation and study them individually with care and in a class of them in a Linnean way. To claim, as Koppl does, that the Santa Fe vision 'supplies the missing math' to 'drive us to Marshall's Mecca of economic biology' is to subscribe to the view that, given enough mathematics, any kind of difficult problem can be formalized and solved. This is pure nonsense. Not even Hilbert would have subscribed to this kind of fantasy - and for it to come from the pen of an economist claiming allegiance to the Austrian approach borders on the reckless.

The infelicities in this chapter are not only technical. There are irritatingly trivial ones as well. Poor Walras is renamed August Léon Walras (without the accent, of course - thus a transmogrification of Antoine August and Marie Éspirit Léon (p.138)); Wieser becomes Weiser (p.139); the reference to the Gode-Sunder paper is given in terms of Shyam Sunder's first name; and the reference to the Marshallian quote on p.138 should be to p.xiv and not to p.xii).

James Stodder in the next chapter, borrowing Leamer's famous use of the word 'con', puts it to good and interesting use by making a case for the 'the con of suppressed induction'. Stodder believes that a healthy dose of skepticism in ourselves as economists, when presented as experimental counterexamples to orthodox models, may reawaken the inspirations that drive many imaginative students to choose economics in the first place. He makes a good case by discussing three examples, within frameworks that can experimentally be implementable, to cast possible doubts on the sensibility of standard approaches to them. The examples are chosen based on what and how they stand as props to the most basic orthodox approaches to *efficiency*:

- in a competitive market
- equating marginal products rather than average products

- the evolution of the institution of money as an efficient solution to the 'double coincidence of wants'

The first counterexample is discussed and described using a variant of a standard experimental economic, double-auction, setup. By now most students of economics would have some familiarity with experimental economics, especially since the laudable most recent Nobel awards to Vernon Smith and Daniel Kahneman. From personal experience of teaching and residing in an economics department closely allied to a '*Computable and Experimental Economics Laboratory*', I can understand and agree wholeheartedly with Stodder's attempts, aims and potential successes. I have never found a single bored student in such classes, but whether the winnowing was done at an earlier stage, I do not know. The key here is that formalization and experimentation can go hand in hand and students can even begin to see a motivation for the mathematization of the subject as they go along with examples and experiments that develop subtle recesses of intuition. The fertile interplay between induction and deduction can also be developed and, paradoxically, a case can even be made of using the orthodox model as a kind of benchmark, as Vernon Smith, Charlie Plott and others have done in imaginative ways.

Stodder's main achievement, however, is not in parroting the tiresome adage of 'induction vs. deduction', extolling the virtues of the former and decrying the vices of the latter, as many others in these volumes have done. But his is a much more laudable and achievable aim of 'inducing the discovery of efficiency in the standard competitive market model' (p.154). This is not only a finessed way of honing the powers of induction in a natural way; it is also a prime example of illustrating the use of idealizations in theory construction. The usual way of appealing to ideal concepts and models by referring to frictionless pendulums, billiard balls and smooth tables are here replaced in the fertile way a good teacher of probability theory can use Feller's first volume with a simple coin and a couple of well sculptured dies to illustrate almost all the subtle concepts and results of that theory.

But Stodder can go even further, and does do so. An experimental framework is implemented in real time. Participants and those who devise experiments see and are a part of a process that takes place in real time and observe the iteration of prices to possible limit points or other, more complicated, limit sets. By varying the experimental set up with meaningful alternative calibrations the observable variations in outcomes happen in real time - even if the real time is virtually realized in computerized equivalents of, say, the double-auction markets. Such real time experimental participation telescope time and suggest an interpretation of the experimental background as an institution and its evolution, of which the competitive market is one possible limit. Other limits, identified with the way the iteration of an experiment seems to be tending could suggest other market institutions or other institutions that are alternative to the market. At this point real, historical, examples of institutions that have evolved, ruled, decayed and even re-emerged can be given as illustrations to give the students a flavour of the historical underpinnings of the experiment. Concepts of justice, fairness and ways of achieving them that depend on history and institutions emerge naturally and the pedagogical task of, say, introducing Santa Fe concepts become a pleasure and almost an imperative. Stodder has intelligent and wise comments on all these aspects and I, for one, found these strategies most felicitous - partly because I, too, have been practising them in that well-known way in which Monsieur Jourdain had been talking prose, without knowing it, all his life.

But two sceptical thoughts did surface while reading this chapter. In describing and explaining the elementary double-auction experimental set up, Stodder observes, quite correctly, that in a consideration of the realistic case of discrete units of traded goods there will be problems of uniqueness of the competitive equilibrium. He could have added more: such considerations lead to serious problems of interpreting the processes in any kind of efficient terms and, more disturbingly, makes nonsense of marginal analysis altogether. I have, in my own classes, at this point, introduced the students to the Clower-Howitt ([16]) paper on the 'transaction theory of the demand for money', by starting from the standard Baumol-Tobin inventory theoretic approach. This is a strategy that enables me to develop a bridge between micro and macro, between money and prices, between individual rationality and market constraints and, above all, provides a neat way to introduce ideas about integer programming in particular and combinatorial optimization in general. These frameworks emerge as a natural outcome of entirely normal economic preoccupations, exactly as the travelling salesperson problem is an intuitive way of introducing the complexities residing in deceptively simple and absolutely ordinary, everyday, life situations - just imagine the country postman's tasks and illustrate its mathematical formulation as a combinatorial optimization problem. Not a drop of marginality will be involved in any of the discussion but one is led to deep concepts of alternative institutional design and, correspondingly, alternative price mechanisms. These are fertile media for the introduction of all kinds of Santa Fe techniques and concepts - lock-ins, non-linearities, evolutionary emergence of orders, adaptation, selection, interaction of multiple simple elementary units, alternative concepts of rationality, and much else.

Unfortunately, I did not find Stodder's description,discussion and explanation of the 'melioration'model in the next section equally felicitous. In fact Stodder's description of the Herrnstein-Prelec melioration model (cf., for example, [36])is not quite accurate at essential points. For example figure 9.3 in Stodder's chapter (p.155) refers to average values for sandwiches and pizzas; but in [36] (from which it is adopted; cf. p140, figure 2, in [36]) these are absolute, hedonic, values, from which an ad hoc average utility curve is constructed - without any experimental motivation.

I do not think it makes much sense, therefore, to present that model in a vacuum. It is more useful within a prospect theoretic model, which becomes an alternative, behavioural economics, framework, out of which orthodoxy does not emerge as a limit or ideal case. An analysis and a description of melioration models requires a thorough understanding and motivation for the concept of *mental accounting* or *value accounting*. If this is done in the context of a discussion about cardinal and ordinal utilities and coupled to a critique of the underpinnings of indifference curve analysis, then there might be a case for this experimental set up. The fact that there is an element of *a priori ad hockery* in the assumption of the existence of *value functions* gives it the same unrealistic flavour as the assumption of utility functions. The choice of weights to parametrise value functions is as arbitrary as choosing convenient functional forms for utility functions and does not add anything to intuition or to the unearthing of suppressed inductive propensities.

The contrived nature of the explanation and presentation of the melioration model becomes clear when one realizes that Stodder is not describing it as an example of the way he has been using it in teaching, but as a desirable model that can be used to illustrate possible anomalies between intuitively reasonable behaviour and the normative precepts of orthodox theory.

The third experimental setup described in Stodder aims to explain the evolution of monetary exchange institutions to solve the problem of the 'double coincidence of wants'. The experiment itself is simple and easy to execute: divide a class of any finite number of student into various subgroups and distribute cards that are to be marked in a certain way by each group. Each group is to shuffle the initial allocation and deal the cards out and then they are asked to try to reestablish the initial allocation. Apparently, Stodder's student groups have had no difficulty in arriving at the 'Walrasian clearing prices'.

Although I agree that the experimental set up is simple I do not think the drawing of implications such as arriving at 'Walrasian clearing prices', or lessons on the 'luck' of history, multilateral gift exchanges as 'efficient solutions' to the problem of the 'double coincidence of wants, etc., are warranted. The experimental set up is too loose to warrant the drawing of such implications, without either preparing the students to 'play the game' with a theory of some sort as their contextual background or guiding them during the game, with instructions on the theory that one wants to highlight. It is not clear whether such a game is to hone intuition on the feasibility of alternative exchange mechanisms or whether it is to highlight the evolutionary emergence of a particular institution, whether efficient or not - i.e, lock-in within one institutional framework due to sensitive dependence on initial conditions, thus 'history matters'. The counterintuitive set up of engaging students to try to reestablish the initial allocation is partly to blame for this ambiguity. However a minor variant of such a game, played against a theoretical background (the way the double-auction game is played), could achieve the aims set forth by Stodder. I have in mind the kind of theoretical set up considered in Alfred Norman's 'theory of monetary exchange ([70], through which one can introduce very orthodox monetary theory and its mathematical and combinatorial complexities, by playing variants of such games. I have tried to use this kind of set up to introduce concepts of computational complexities and, then, at a second stage introduce a model of computation that must underpin a theory of monetary exchange. But, then, my purpose was to make a case for 'Computable Economics' which, in my way of doing things, contains Santa Fe visions as particular subsets. Stodder's aim is to show that his attempts to teach the emergence of institutions that solve the problem of the 'double coincidence of wants' leads to fertile synergies with and for a Santa Fe approach. The latter vision and approach, at least as presented by most of the authors in these two volumes, hardly ever touch the idea of a model of computation or its complex theoretic implications in the form of the theory of algorithmic complexity or theories of computational complexity, natural frameworks in which to set and theorise about the institutions of monetary exchange.

Stodder ends his interesting chapter with a peculiar claim (p.164):

> It is significant that much of the earliest and best experiments on anomalies of choice were done in business schools ....

Surely, Stodder's sense of history cannot have forgotten the anomalies discovered by Allais, Wold, Ellsberg, ...! Mercifully, there are only a couple of stylistic infelicities in this chapter: the reference to 'Figure 9.1' at the bottom of p.160 should be to 'Figure 9.4' and the second Herrnstein-Prelec item in the reference list is superfluous.

Let me end my reflections and comments on this chapter by setting the record straight on a much maligned story, twice re-told in this volume: first by Arthur[63] in his second chapter in this book and, then, by Stodder in this chapter. Both of these authors refer to the alleged Planck-Russell story on the difficulties or otherwise of doing economics. The story begins with a footnote in the famous Keynes biographical piece on Marshall in the *Economic Journal* of 1924 ([47], pp191-2):

> Professor Planck, of Berlin, the famous originator of the Quantum Theory, once remarked to me that in early life he had thought of studying economics, but had found it too difficult. Professor Planck could easily master the whole corpus of mathematical economics in a few days. He did not mean that! But the amalgam of logic and intuition and the wide knowledge of facts, most of which are not precise, which is required for economic interpretation in its highest form is, quite truly, overwhelmingly difficult for those whose gift mainly consists in the power to imagine and pursue to their furthest points the implications and prior conditions of comparatively simple facts which are known with a high degree of precision.

The second, Russell, part of the story is narrated in Harrod's biography of Keynes ([33], p.137; italics added):

> I happened to sit next to Keynes at the High Table of King's College a day or two after Planck had made this observation, and Keynes told me of it[64]. Lowes Dickinson was sitting opposite. "That's funny," he said, "because Bertrand Russell once told me that in early life he had thought of studying economics, but had found it too easy!" Keynes did not reply. It was unlikely that Russell's remark was to be taken with the seriousness that Lowes Dickinson seemed naïvely disposed to attribute to it.

Foley's chapter on 'Complexity and Economic Education' should be read in conjunction with his highly pedagogical 'Introduction' to Peter Albin's pioneering 'Essays on Econoomic Complexity and Dynamics in Interactive Systems' ([26]) and his forthcoming 'Schumpeter Lectures' ([27]). Foley's message for the pedagogically minded complexity theorist is clear and straightforward:

[63] Arthur states that:'In his autobiography Bertrand Russell tells us he dropped his interest in economics after half a year's study because he thought it was too simple. Max Planck dropped his involvement with economics because he thought it was too difficult'. Such are the ways for myths to emerge!

[64] Harrod adds in a footnote, at this point: '[Keynes] had just returned from Berlin, where he had been advising on the depreciation of the mark in November 1922

'viewing the economy as an adaptive, complex' self-organising system 'implies fundamental changes in the way we teach the use of mathematical tools for economists ... leading towards a more inductive analysis that tries to draw general conclusions from the simulation of a wide variety of specific models.' But he warns that induction and simulation come with their own dangers:

- '[T]he uncertain power of generalization and extrapolation that dogs all inductive arguments ...; Induction can err as easily as deduction. (p.167, 170);
- '[T]here is no generally agreed upon methodology disciplining computer simulation and no generally accepted protocol for drawing information from simulation that transcends the simulator's priors'(p.171);

I suspect this is addressed as much to the general economic theorist as to the Santa Fe visionaries who, in their enthusiasms, seem to have been carried away by the seeming intuitive simplicity of induction and the easy availability of powerful simulation techniques to forget that thinking and theorising are more complicated than appears on the surface. As Foley himself perceptively notes (p.172):

> Epistemology is a very strange subject.

And so is *Methodology*!

Foley refers to Rosser's claim in chapter 14 of this book that 'nonlinearity is a necessary, but not sufficient condition for the emergence of complex dynamics' (p.168). But, then, Turing's *linear* model of morphogenesis generates, via the *Turing bifurcation*, an emergent, complex, structure. It will be necessary to define 'complex dynamics' with sufficient generality to substantiate Rosser's intuitively reasonable claim.

Foley's main message seems to be that 'computer simulation ... will ultimately dominate an economic pedagogy that takes complexity seriously.' One would, therefore, hope that in addition to devising methodologies to 'discipline computer simulation', students would also be taught that underpinning the use of the computer there is also *numerical analysis* which has to be coupled to the mathematics of the computer itself, *recursion theory.* It is implicit in Foley's chapter that this pedagogy, through which students can approach the modelling and study of complex adaptive systems, would be based on the three sub-disciplines of dynamical systems theory, numerical analysis and recursion theory. This is quite a different scenario from Brock's vision of statistics and probability as the underpinning disciplines in the study of complexity. I favour, unreservedly, the former.

Part four comprises five chapters, the theme is the same as part three: 'Teaching the Complexity Vision in Economics'. However, the theme is expanded via the specifics of macroeconomics, development economics, mathematical economics and statistics & econometrics, authored by Prasch, Hoover, Ramaswamy, Rosser and Matthews, respectively.

Prasch in his chapter on 'Integrating Complexity into the Principles of Macroeconomics' carries on in the same refreshing vein of the previous two chapters, by Stodder and Foley, by concentrating on a couple of fundamental principles that characterize - or should characterize - *any* kind of macroeconomics, but particularly to a macroeconomics amenable to complexity theoretic interpretations: the *fallacy of composition* and, for want of a better name I have always called variations on the theme of the '*banana parable*' ([48], pp.176-8). On these two principles Prasch notes, perceptively (p.179):

> Taken together, these two fundamental principles, principles that have been virtually banished from our textbooks, can create room for concepts that are consistent with a complex understanding of the world.

There is, however, a third macroeconomic precept, not quite at the level of a 'principle', quite universally adopted by all practitioners of macroeconomics,but particularly the growth theorists: the idea of '*stylized facts*'. In anticipation of the structure of the arguments in the next chapter by Hoover, it may be apposite to bring up the use of the idea of 'stylized facts' in macroeconomics, in particular in growth and cycle theories.

Much has been made of pattern detection and Peircean abduction as part of the methodological credo of a Santa Fe vision. One of the many ways Peirce tried to give concrete content to the idea of retroduction[65], the word he finally chose to use, instead of abduction, as a translation of Aristotle's concept of apagogy ($\alpha\pi\alpha\gamma\omega\gamma\eta$) was as follows ([72], p.142; italics added):

> [In Retroductive reasoning] *not only is there no definite probability to the conclusion*, but *no definite probability attaches even to the mode of inference.* We can only say that the Economy of Research prescribes that we should at a given stage of our inquiry try a given hypothesis, and we are to hold to it provisionally as long as the facts will permit. *There is no probability about it.* It is a mere suggestion which we tentatively adopt. For example, in the first steps that were made toward the

[65] As noted by Peirce: ' $\ldots\alpha\pi\alpha\gamma\omega\gamma\eta$ should be translated not by the word abduction, as the custom of the translators is, but rather by reduction or retroduction.'[72], p.141

> reading of the cuneiform inscriptions, it was necessary to take up hypotheses which nobody could have expected would turn out true, - for no hypothesis positively likely to be true could be made. But they had to be provisionally adopted, - yes, and clung to with some degree of tenacity too, - as long as the facts did not absolutely refute them. For this was the system by which in the long run such problems would quickest find their solutions.

It would be clear to any serious methodologist, among the Santa Fe visionaries, where Perice-type retroduction[66] differs fundamentally from induction. But even the non-methodologist macroeconomist, familiar with the modern classics in growth theory and recent forays into methodology, even by some of the leading newclassical economists, will recognize that Peircean retroduction has been practised with finesse and fertility, albeit not calling it so - once again in that famous manner of Molière's Monsieur Jourdain, who had, of course, been talking prose all his life without knowing it. I refer all interested readers to Kaldor's classic paper which introduced the concept of '*stylized facts*' ([43], particularly pp.177-9), Romer's felicitous resurrection of it in his pioneering paper on Endogenous Growth Theory ([76],pp.53-5) and Kydland-Prescott ([55], pp.3-4) for the newclassical practice of retroduction when interpreting 'business cycle facts' - in all three cases without probabilities[67]. In other words, the starting point is not that raw facts are to be interpreted within a probabilistic model which is characterised by a set of parameters to be estimated statistically and econometrically. The retroductive approach advocated by Colander, Hoover and Matthews, in this volume, therefore, pulls the rug from under a Santa Fe vision that suggests that the methodological mecca for those who begin their journeys in New Mexico is in 'statistics and probability'.

[66] This caveat is necessary because there are other serious philosophers of science, from the great Bolzano to Norwood Russell Hanson and, among economists, Herbert Simon, who have interpreted *apagogy* with differing and subtle variations that do not correspond to Peircean views.

[67] It may be worth quoting Kydland and Prescott on this point just to reinforce this particular difference between induction and retroduction: 'Economists, [Koopmans] argues, should first hypothesize that the aggregate time series under consideration are generated by some probability model, which the economists must then estimate and test. Koopmans convinced the economics profession that to do otherwise is unscientific. On this point we strongly disagree with Koopmans: we think he did economics a grave disservice, because the reporting of facts - *without assuming the data are generated by some probability model* - is an important scientific activity.' (ibid, p.3; italics added) and, then, after listing the 'stylized facts'- called 'growth facts' by Kydland and Prescott - that are of interest for their particular inquiry, they go on even more emphatically to state (ibid, pp.3-4; italics added): 'These facts are neither estimates nor measures of anything: they are obtained *without first hypothesizing that the time series are generated by a probability model belonging to some class.*'

Thus, I would strengthen, by adding to the 'two fundamental principles' the third precept, Prasche's entirely reasonable claim that:

> Taken together, these two fundamental principles [and the third precept], principles [and precept] that have virtually been banished from our textbooks, can create room for concepts that are consistent with a complex understanding of the world.

Whether the creation of such room will violate Colander's 15% rule and if it does how one can find a compromise, I do not know. But, unburdened by the imperatives of textbook dependence in my own teaching, I am freer to roam than the conventional North American and the increasingly conventional UK economics teacher. Thus, the 15% rule is not something I worry about, although I suspect I belong to a distinct non-worrying minority on this point.

I am in full agreement, also, with Prasch's well substantiated observation, referring to *path dependence* and *positive feedback*, in particular, but he could easily have added *nonlinearity* and *adaptive behaviour* as well, that:

> [I]ntegrating some of the ideas associated with path dependence into the principles course is less a task requiring originality, as much as it is a task of retrieval. History shows us that we have been there before. ... However, ..., we need to build the 'complexity vision' into the very bedrock of the principles of economics.

The trouble is how to make the 'retrieval task' contextually meaningful to a generation of students brought up on a diet of antiseptic and ahistorical macroeconomic theory. Prasch delineates the strategy *he* has adopted to resolve this dilemma in the rest of the chapter. One crucial element in his teaching strategy seems to have been to try to disabuse his students from the pernicious habit of assuming that the dynamics of a macroeconomic system is fundamentally stable. This 'stability dogma'[68] has a long history in economics and is entrenched in conservative policy circles, bolstered by various types of

[68] A phrase I have borrowed from Samuelson's characteristically candid 'confession' on how he came to adopt this 'dogma' as a working hypothesis: 'In leaving Frisch's work of the 1930s on stochastic difference, differential and other functional equations, let me point out that a great man's work can, in its impact on lesser men, have bad as well as good effects. Thus, by 1940, Lloyd Metzler and I as graduate students at Harvard fell into the dogma .... that all economic business-cycle models should have damped roots .... what was so bad about the dogma? Well, it slowed down our recognition of the importance of non-linear autorelaxation models of van der Pol-Rayleigh type, with their characteristic amplitude features lacked by linear systems'. ([82],p.10)

neoclassical economics and all types of newclassical macroeconomics. Now, the one point where I disagree with Prasch is the following. He finds fault, I suspect half in jest, with whoever it is who chose to name the phenomenon underlying 'positive feedback' in that way:

> ... [W]hoever originally named this phenomenon [as positive feedback] was not a teacher.

But surely, Prasch can, as I routinely do in my classes at every level, use this supposedly inappropriate nomenclature in a 'positive' (sic!) way: taking the chance to associate 'instability' with positive feedback and 'stability' with negative feedback gives those of us who see the reality of perverse economic force for what they are a golden opportunity to exploit, for once, this manna from heaven felicitously[69]!

Kevin Hoover's reflections on 'Teaching Macroecoomics while Taking Complexity Seriously' is a perfect complement to the chapter by Prasch. Hoover, in a sense, in his teaching of macroeconomics takes Prasch's two fundamental principles and the additional precept I suggested and adjoins to them a fourth element that characterizes and sharply differentiates macro from micro, *national income accounting*. Clearly, one needs the disciplining framework of a proper accounting system to make sense of the 'banana parable'. The fallacy of composition, on the other hand, is basically about the pitfalls of aggregation and, on its flip side, a stricture against reductionism. Hoover has interesting observations on this point and his students, hopefully are disabused[70] of facile false parroting of claims that this or that macroeconomic model has microfoundations. Every such claim has to be false and the sooner students are made aware of this fact the better. Hoover, however, does adopt the recent practice in intermediate teaching of macroeconomics, where growth theory is almost the starting point. However, he 'de-emphsizes the self-adjusting mechanics of neoclassical growth' (p.195). Clearly that gives him an avenue, as it gave Prasch, at least to introduce ideas about positive feedback, lock-ins, hysteresis and other fertile 'Santa Fe concepts - depending on whether he allows himself to use nonlinear dynamics in some interesting way.

I think Hoover is completely correct in claiming that these elementary building locks are more than sufficient to illustrate lessons 'about garden-

[69] I have argued elsewhere, using a computable framework to formalize economic dynamics, that only unstable models are capable of interesting dynamics, in particular dynamics compatible with a vision of capitalism that endows it with the kind of evolution that history testifies as facts, [96].

[70] It is appropriate that I use this word in the context of a chapter that is innovative in such usage: there is a 'disconnection' (p.189) and even a 'disanalogy' (p.192)!

variety complexity' (p.194). Whether just this and the Santa Fe emphasis that 'even if we begin with simple rules, real situations become complex fast' exhaust what the complexity vision and theory has to offer to macroeconomics is something on which Hoover refuses to take a stand. I think this is a reasonable position to take, especially by someone like Hoover whose teaching of macroeconomics, like Prasch, seemed to be infused with enlightened insights into the interplay between facts, the process of theory formation and an informed historical perspective - both factual and doctrine historical.

I concur unreservedly with Hoover's willingness to go 'only half way' with the Santa Fe visionaries along 'the inductive rather than deductive' path. He opts for the Peircean abductive path and this, as I have discussed above, in the context of Prasch's chapter and economic dynamics, is a more sensible methodological option for macroeconomics. In a discipline where the fallacy of composition and the 'banana parable' are fundamental, there is no alternative but to adopt the abductive or retroductive methodological stance. I suspect that there is some deep seated methodological confusion or pure ignorance on the part of the Santa Fe visionaries who seem to think that induction and deduction exhaust the alternatives for scientific discipline. Hoover has done signal service by alerting them - at least some of them - to the existence of a third alternative.

I have, however, two minor reservations on the contents of the chapter. The first is a trivial point. Hoover states that (p.191):

> The animating spirit of of economics is not Keynes, but Walras, Debreu and Bourbakism. The movement in this direction began immediately on the publication of the General Theory in 1936 and is now complete.

That Keynes is no longer 'the animating spirit' of macroeconomics is quite clear, despite schools that ostentatiously call themselves New Keyneseans and the like. It may also be true that Walras and Debreu are 'the animating spirits', given what Hoover quite accurately calls Lucas's 'absurd hope' that the 'distinction between micro and macro will soon be erased' (p.191). But to bracket all these plausibilities with 'Bourbakism' might well be slightly disingenious (to be very Hooverian about it!). He might have been more accurate had he chosen the characterization 'formalism', rather than Bourbakism. Adherents of 'the contemporary *formalist* school of mathematics' ([21], p.viii; italics added) were the animating spirits behind the re-mathematization of economics that was taking place from about the late 20s and proceeding through the 30s with gathering momentum, undiminished during the war years - indeed accelerated, if anything - and reaching an initial crescendo by the mid- and late-50s, culminating in the publication of Debreu's classic text (ibid). Bourbakism, as a mathematical movement, had been embryonic in the early 30s and became codified during 1934 and 1935. By these years the mathematical philosophy that was to animate economic theory in its second wave of mathematization had taken a clear shape via von Neumann's growth and initial game theory papers and the work that Remark, Wald and others were doing in Germany and Austria. It is dangerously facile not to make a clear distinction between *Bourbakism* and *Formalism*, especially since the former is a waning spirit in contemporary mathematical philosophy and the latter is, if anything, a thriving movement, despite several setbacks to its foundational edifices in the years since Skolem, Gödel, Brouwer, Church, Post, Turing and others.

The second point where I disagree with Hoover is in his reconstruction of the state of economics in general and macroeconomics in particular 'by the 1930s', as he puts it. In talking about economic theory at this time he refers to it exhaustively as either 'French or English'. Surely, in the 30s it was more 'English or Austrian', if a dichotomy was to be chosen? As for macroeconomics - i.e., policy based economics - the Swedes and the Ausrians were leagues ahead of Keynes by the early 30s - both having Wicksell as their fountainhead. Lindahl, Frisch and Myrdal had claimed 'autonomy for macroeconomics'[71] even before the Treatise was published and much before the General Theory was even conceived. In particular, the theory of economic policy, which received its first formal formulation, in terms of aggregative national account categories, at the hands of Myrdal and Lindahl, and their lineage is a direct path from Wicksell, Public Finance, Taxation and Trade Cycle theory. Macroeconomics was a five-fold bud, the petals unequally developed: monetary theory, capital theory, trade cycle theory, public finance and taxation theory and value and distribution theory. The link with micro was most evident in the theories of capital and value & distribution theories. Wicksell had initiated the movement towards what became macroeconomics by his profound critique of the quantity theory at the turn of the previous century and adding to it also that famous wedge between a money rate of interest and the natural rate of profit, thereby linking monetary theory with capital theory - macro with micro. The Swedes, therefore, had a head start in developing a claim to autonomy for it. They were able to do so because they had developed the national accounting categories via their work on public finance and taxation. Unfortunately, the national accounting codification that became orthodox lost the public fi-

[71] Although Frisch and Kalecki had coined and used the word 'macrodynamics' from the early 30s, the earliest published reference to 'macroeconomics' that I have been able to unearth, in the precise aggregative sense it has come to mean, are in Tinbergen ([93], p.14) and Lindahl ([59], p.52).

nance underpinnings the Swedes sought for it and became infused with the Keynesian spirit for it - a story well told by Hicks in his piece in the Lindahl Festschrift ([37], chapter 18), which was also the 'animating spirit' behind the later development of French Keynesian - or Fix Price - macroeconomics.

Paradoxically, Hayek reacted away from 'aggregative economics', in the 30s, precisely as a 'response to complexity', not only in the 'most straight forward, ordinary language sense', but, if some of the other authors in this and its companion volume will have their way, in a precise Santa Fe sense as well. On the other hand the Scandinavian reaction, particularly by Lindahl, Myrdal, Frish and Lundberg, and to a lesser extent by Ohlin, was very much along the lines suggested by Hoover. They, to their credit, insisted on the primacy of national income accounting as the backdrop for policy frameworks and simulations for the study of economic dynamics because, in the latter case, they refused to compromise with the fundamental instabilities of a market economy and refrained from linearizing for mathematical convenience. That turn was left for the Anglo-Saxon school, soon to dominate all and sundry, to take and Metzler and Samuelson soon obliged. Kalecki was, of course, left in a different kind of wilderness, to be periodically resurrected along Robertsonian hunted-hare episodes.

This is my way of teaching macroeconomics but, then, I am freer to roam outside the constraints of formal textbook strait-jackets. As far as I am concerned there is no need to bring in the French at all - except, perhaps, a polite nod to Walras, a silent one in view of the beautiful but nihilistic Sonnenschein-Dereu-Mantel theorem on aggregation and excess demand functions!

There are minor infelicities in the reference list: the French title of Cournot's classic has some inaccuracies; the definite article in the tile of Keynes's 1936 classic is missing; and a hyphen in the title of the Lucas essays is also missing.

Sunder Ramaswamy, in the next chapter on 'Development Economics and Complexity' aims to 'point out that the ideas that the complexity approach raises, such as increasing returns, cumulative processes and path dependency, have been the preoccupations of scholars concerned with . . . [development economics] since the second world war'. Ramaswamy's subsidiary thesis appears to be that complexiy theory offers the mathematical tools to formalize these issues and thereby enable development economics to be 'integrated into standard economics'. If not, the discipline risks becoming an outlier that it seems to have been at various periods of its life, most conspicuously in the post-1960 period till its recent revival together with so-called New Growth Theory. More concretely, Ramaswamy concludes after an inevitably potted 'history' of post-WWII development economics, ideas of complexity, whether pertaining to this or any other discipline, if not 'embedded in simple models', 'will fade' away.

I do not subscribe to this view simply because there are any number of counter-examples, even in disciplines quite closely allied to development economics. Keynesian macroeconomics was embodied in teachable formal models and was spectacularly successful in its golden quarter century between 1947 and 1972. There were ample complexity theoretic concepts underpinning it - just to name two of them: multiple equilibria and positive feedback via the acceleration principle - and beautifully formalized using nonlinear mathematics. It has faded away, even from textbooks claiming to be written by New Keynesians.

Quite apart from this counter-example, there is a different kind of problem with Ramaswamy's 'Krugman version' of the story of the demise of development economics from its '*High Development Theory*' (HDT) period, as Krugman characterized it (cf. [53]) - roughly speaking the decade and a half from Rosenstein-Rodan in 1943 to Hirschman in 1958. That the work on development economics spanning these years did not 'take off', to use a phrase that was special to the field, at least according to Krugman (op.cit) and Ramaswamy, was the failure to formalize the rich and pregnant ideas that have become common currency in the complexity vision in simple mathematical formalisms. And this failure, in turn, was because the mathematics did not exist or the pioneers of this HDT period were not able to, or not willing to harness them, even if they existed.

But the story of the fading away of HDT can be given a different twist. Development economics was always highly contextualised by institutional considerations. By the late 50s dynamic programming and the maximum principle had become familiar tools for economists. Long before Pontrygin and his co-workers published his results in a book form ([74])and even before Bellman's textbook on dynamic programming was published in 1959 ([10]), the problem of economic development had been placed within the context of planning models for development and these, in turn, had become optimum problems. In this sense development economics never left the mainstream; it was simply another chapter in standard growth theory or a supplementary textbook to a textbook on growth theory. Witness the appearance of books like Sukhamoy Chakravarty's *Capital and Development Planning* ([13])and the hundreds of articles in all the leading Journals on optimum savings, turnpike theorems and so on. Tinbergen in 1956 ([94])and Goodwin in 1961 ([31]) had recast Ramsey's problem as a problem for a developing country to choose an optimum savings rate and it was allied, therefore, to capital theory and the thorny issue of choice of techniques. Another example, including a substantial part for which the current director of the economics division at SFI, Samuel Bowles, was responsible, was the Chenery edited volume on *Studies in Development Planning*. Let me simply single out just Larry Westphal's superpbly pedagogical and presciently numerical and applied chapter, the fourth in this excellent volume. It was titled *An Intertemporal Planning Model featuring Economies of Scale*.([15]. Right at the outset, Westphal observed[72](p.60):

> Economies of scale in interdependent sectors constitute an empirically important case in which market signals are inefficient instruments of investment planning, but only recently has the numerical solution of nonconvex models become practical. This study reports on a multisector, multiperiod optimizing model for South Korea in which economies of scale are specified in the petrochemicals and steel producing sectors are specified.

Ramaswamy 'recalls' that(p.204):

> [A]s a graduate student at the Delhi School of Economics ... the papers [they] were reading for the graduate course on development economics were not necessarily as elegant or tractable as in other courses, but were focused on the important questions about the human condition from an economic perspective - issues of poverty, demography, industrialization, and so on, for many of which we did not always have answers.

I have chosen, as examples, texts that I myself had as assigned or recommended reading when I was a student of development economics in the very early 70s. We had the formidable task of reading articles by Nemchinov, which casually referred the interested reader to the book by Pontryagin et.al., and coming to terms with the Kornai-Liptak methods[73]; the books by Chakravarty and Chenery were on the reading list (the former was also in the recommended list for the post-graduate course on 'Capital and Growth Theory'). These, too, dealt with 'the important question about the human condition from an economic perspective - ... '. Perhaps the key difference was the arrogance or the naïvety with which these planning models suggested that the problem of development had elegant answers.

[72]Sections and subsections had titles such as: '*Behavior of a model with increasing returns*', '*Consequence of Alternative Patterns: The Complex Sectors*', and so on.

[73]My copy of Pontryagin et.al., is dated 27 December, 1972; that was the term I did the 3rd year undergraduate course on development economics in the economics department at the University of Lund.

I can give equally valid examples, from the development planning literature, of the seamless incorporation of nonlinear methods, simulation-rich examples and other Santa Fe characteristics. But the point I wish to make is the following. The return of HDT themes is not only because there may well be mathematical tools to make them work or formal frameworks in which to encapsulate them effectively. It is also due to a change in perspective and an ideological shift: the demise or the dethronement of the supremacy of planning models; the end of 'Licence Raj' regimes; the return, with a vengeance, of free market ideologies and the concomitant rise of market-based models of growth and development.

Development economics never faded away; it just took on a different guise, lived under an alternative ideology, and adapted and adopted the mathematical tools appropriate for that outlook and its aims. But no doubt Ramaswamy is right when he notes:

> Dusting off the ideas already found on the intellectual shelves - ...- to expand our understanding of the complex phenomena of growth and development is a fascinating and extremely important research agenda.

But why dust collected in the first place is itself a *complex phenomenon* which, in turn, 'is a fascinating and extremely important research agenda'. It is certainly not a one-dimensional saga à la Krugman-Ramaswamy of missing mathematical tools.

Rosser, in chapter 14 titled, 'Integrating the Complexity Vision into Mathematical Economics', wonders 'how the idea of economic complexity can be introduced into the teaching of economics'(p.209). To make concrete his thoughts on this, Rosser tries a working definition of complexity, following Richard Day [20], as dynamics with erratic, endogenous, oscillations, whose basins of attractions are not the elementary limit sets - i.e., limit points or limit cycles. Rosser points out, then, that dynamical systems satisfying such a criterion encapsulates some form of nonlinearity and that this is 'necessary but not sufficient condition for such complex behaviour'. Now, there are two inadequacies with such a definition. First of all, where do we place the ingenious Turing Model of Morphogenesis in such a scheme? Secondly, what of complexities that are generated in dynamics that are not formalized as differential or difference equations; or complexities, say, emerging in models of computation or in models, say, of integer or other combinatorial programming problems? He contrast this Day-inspired definition of dynamic complexity with a loose characterization that a Santa Fe vision sees as minimally 'associated with truly complex systems':

- dispersed interaction of a large collection of elemental units;
- no global controller in the sense that not all profitable opportunities in the system as a whole are exploitable by any one unit - as I see it, this means a continual violation of the no arbitrage principle and hence of the orthodox rationality postulates for individuals and the collective;
- such systems tend to be hierarchically organized with decision nodes and flows proceeding 'haphazardly' (in a definable sense?);
- the individual units that make up the system continually adapt and evolve (in response to systemic and exogenous signals)
- the system as a whole generates new orders - the emergence of order;
- out-of-equilibrium dynamics of the system as a whole where the possibility of multiple equilibria is, of course, entertained - but also 'there possibly being no equilibrium at all';

This is all very fine and properly vague enough to be all encompassing but not concrete enough to be a list that can guide the search for pedagogical precepts for choosing topics and strategies in a mathematical economics textbook. In particular, a statement about the non-existence of equilibria in a dynamical system will face the same formal difficulties as those faced by recursion theorists who had to define, formally, the notion of effective computability before being able to talk about non-computability. To not bother about the existence or not of equilibria is one thing; to work with a hypothesis that the system does not possess an equilibrium is quite a different thing.

Furthermore, the alternatives are not simply the existence or not of one or more solutions. There is also the case of the undecidability of solutions and their limit sets. To add a further twist, there is the still more basic case of the computability of even solutions that are 'proved' to exist. Most of these proofs are solidly non-constructive. For example, many of the limit sets commonly invoked in a Santa Fe inspired complexity vision are provably undecidable. What would be the meaning attached to using dynamical systems with provably undecidable limit sets for simulation purposes. These comments may have been more appropriate in the context of the Brock or the Brock-Colander chapter but Rosser is more explicit about basing his definition of a relevant complexity in terms of dynamical systems.

Next,after a discursive tour of some of the more standard and elementary books that aim to be advanced undergraduate mathematical economics texts, Rosser seems to come to the following conclusion (p.220):

> It may well be that in the longer run, evolutionary dynamic game theory will provide an entry for using computer simulation techniques to demonstrate various kinds of complex dynamics in mathematical economic textbooks.

But, surely, this depends on what kind of economics a mathematical economics textbook takes as its base. If the economics is general equilibrium theory then one of Nikaido's classic textbooks could be the ideal one. Almost the only way any kind of Santa Fe theme can enter such a book would be via considerations of computable general equilibrium theory, but it would be the marginal theme of computational complexity theory. Rosser would be right in his conjecture if the economic core is either some form of game theory or modern IO.

On the other hand, any textbook on growth and cycle theories can incorporate a reasonably full menu of the Santa Fe themes and there is no reason why such aspects of economics cannot aspire to become part of the core of mathematical economics. After all, that's one of the two pillars on which von Neumann erected the basis for modern mathematical economics! I don't think Rosser is averse to such a strategy. In the concluding section he does include, as leading candidates for an infusion of complex dynamical elements: increasing returns in sections on production theory, multiple equilibria in sections on equilibrium, cobweb models and multiplier-accelerator models.

There is another direct way to introduce core complexity themes into either an auxiliary mathematical economics text or even directly into a textbook on general equilibrium theory. This is to redo elementary choice theory, *ab initio*, and formalize some version of bounded rationality and make such an agent face *decision problems* in the strict recursion theoretic sense. This, coupled to an alternative, algorithmic or combinatorial production theory, would give the chance for the textbook writer to break the traditional mould and take even the advanced undergraduate economics student with some mathematical sophistication towards a border with a Santa Fe vision. But who will write such a textbook - even if there may well be some readers, and even a few courageous teachers who might teach from such a book? I have in mind the fate of one of the finest textbooks in mathematical economics, written by an outstanding mathematician, that fell like water on a duck's back: *Lectures on the Mathematical Method in Analytical Economics* by Jacob Schwartz ([87]. The fate of that book is a salutary lesson for those of us who might want to break Colander's 15% rule and remain within the citadel of economic theory. Incorporating complexity visions into a modernized version of that outstanding book by Schwartz would be the easiest thing to do; finding a publisher - with or withot the 15% rule - would be next to impossible.

If a reader can skip the the long first two paragraphs of the next chapter, by Peter Hans Matthews, then some sensible and interesting thoughts on ideas 'Toward the Complexification of Statistics and Econometric Curricula' can be gleaned. On the other hand a reader has to be ever vigilant, lest the author's unwarranted claims leads to the proverbial throwing away of the baby with the bath water. For example, right at the outset we read that(p.232):

> [T]he 'complexification' of the statistics/econometric curriculum starts with a subtle epistemological shift: students learn to understand (even) traditional inference as part of a broader concern with 'pattern detection' or *what Hoover calls*[74] *'abduction'.*

And, a page later (italics in the original):

> *[T]he pursuit of pattern and order, and the consequent attention to abductive methods, are the* sine qua non *of a 'complexified' statistics and econometrics curriculum'*

'Traditional inference' is precisely what 'abduction' is *not.* As I have pointed out, with references to chapter and verse by Peirce, at least his kind of abduction, which is what Hoover invokes, has nothing to do with 'traditional inference', replete as it is with notions of various kinds of probabilities. Either Matthews has in mind a different definition of abductive methods or has not understood the Peircean notion; I rather suspect the latter.

But if we abstract away from such unwarranted and exaggerated claims, the section on 'Patterns, Nonlinearities and Cognition'[75] has some extremely valuable pedagogical insights, apparently gleaned from classroom experience. What Matthews has to say about student exercises in pattern detection is as sensible as anything the experimental economists have been doing and even more basic. As he correctly observes, simple curve, graph and table constructions, from raw data - the kind of exercise advocated by Kydland and Prescott, as I pointed out when discussing Stodder's chapter, Peircean abduction and 'stylized facts' - enables students to discover Okun's law, the short-run Phillips curve, Klein's 'five great ratios', and so on. This is an exercise in Peircean abduction par excellence, provided the students are, at this point, taught to give a theoretical form to the patterns they have discovered.

[74]At this point Matthews refers to 'Hoover (1999), which is missing in the referencee list; but extrapolating from Brock(1999) I think I am justified in inferring that he means the Hoover chapter in this book!

[75]I cannot see why 'cognition' is used in the heading to this section, but there is much that is unfathomable in the choice of words and phrases in this chapter.

Matthews also makes the eminently sensible case for the students to be encouraged to explore non-economic data, as a small gateway toward discovering Santa Fe emphasized patterns. It is not unlikely that the more imaginative students may discover self-similar structures and power laws in economic, financial and other data and that would give the more enterprising teachers a chance to introduce a few of the Santa Fe models and techniques, such as the sandpile model. From these small steps to a discussion of criticality and self-organization, even before introducing any statistical or probabilistic structure on the data, would be almost natural, as Matthews quite correctly observes.

Matthews makes the valid point that 'a Santa Fe-inspired econometrics course should, at the least, underscore the detection of (even mild) non-linearities in economic data' (p.239) and I think he is absolutely to the point when he suggests either the repealing or amendment of Franklin Fisher's 'Iron Law of Non-Linear Econometrics - Don't Do It' (ibid). However, for students to make sense of detecting non-linearities in raw data presupposes that they would have had some instruction on the role of non-linear economic theory of a kind that is amenable to Santa Fe interpretation. But as many of the previous chapters have made clear, particularly the chapters by Stodder, Hoover and Rosser, economic theory of even the most conventional variety is replete with such possibilities.

I must confess to being unable to make any sense of the caveat:

> This said [i.e., repeal or amend Fisher's 'Iron Law'], if it seems obvious (to most of us, at least) that the 'laws of motion' of capitalist economies are often non-linear, it is also the case that the pursuit of non-linearities can be overzealous, as the 'overfitting' of Hendrick-Prescott[76] filters and some 'neural net' methods illustrate.

I have always understood the Hodrick-Prescott filter to be *linear*!

Matthews suggests that students could be introduced to 'the intuition, if not the technical foundations, for the BDS statistic'. Does he, then, envisage an econometrics course where students have had a prior course in reasonably advanced non-linear dynamics? For, if not, how are they to understand even something as elementary as an 'embedding dimension' that is crucial in making sense of the sensitivity of the BDS[77] statistic to it? I do not think this is a feasible suggestion to the kind of students he seems to have in mind, at least

[76] Clearly the author means 'Hodrick-Prescott, just as much as he means Durbin when he writes 'Durban' in referring to the classic paper by Brown, Durbin and Evans, a couple of pages earlier!

[77] Named after **B**rock, **D**echert and **S**cheinkman who first devised it in 1986.

as I can infer from the description of the exercises they have been asked to undertake.

In the same breath Matthews thinks that a reasonable variant of the Polya urn process will leave the very same students, who can handle the BDS statistic and understand the role and purpose of embedding dimensions, with difficulties in being 'able to sense, let alone prove, the [limit] result, so this, too, will require computer simulation.' Either Matthews does not quite understand how to present a reasonable variant of the Polya urn process or has no grasp of its formalism and combinatorial structure. I would advise him to present it in the simple way discussed in Feller's classic first volume, pp.119-121 ([25]). I speak here from experience at having had it taught in my probability class, as an undergraduate, more than 30 years ago (I still have the same, heavily marked, volume of Feller). While on this topic and referring to Feller I might as well add another point. It has become fashionable to refer to the Polya urn scheme and its relevance for discussing and modelling lock-ins, hysteresis, positive feedbacks and all sorts of related things. I think students should be presented alternative probability models that can encapsulate such phenomena, sometimes in a more intuitively acceptable way than urns and balls. I have in mind, in particular, the *Ehrenfest Model of heat exchange between two isolated bodies*, which is presented, in Feller, immediately after he discusses, in a characteristically lucid mode, the Polya urn process.

On the other hand I endorse wholeheartedly his suggestion that 'students should also become familiar with recursive estimation'. A familiarity with recursive estimation would mesh well with introductory instruction in iterative - recursive - dynamics, i.e., maps, so that elementary non-linear models can be presented in a unified way. But the one time I tried to teach recursive estimation, I did not get beyond linear methods even after a whole quarter! And all attempts at teaching non-linear dynamics, concentrating on maps, has never taken less that half a quarter of lectures and tutorials with the computer. I guess Matthews has brighter, better prepared, students, for without them even 'a three course sequence' to complexify the 'statistics and econometrics curriculum' along lines probed by Matthews will be too demanding. On the other hand, if complemented by allied courses in macroeconomics, microeconomics and mathematical economics, which take into account the contents of the projected 'complexified statistics and econometric curriculum', it might be a realisable project over a couple of years. I suspect that all this leaves Colander's 15% rule in tatters.

One final point. The author makes the valid observation, early in his chapter, that the 'remarkable renaissance' of (endogenous) growth theory, 'rooted in the perceived *empirical* failures . . . of the neoclassical parable, has produced so little persuasive econometric research' (p.232; italics in the original.). He goes on, thereafter, to discuss pattern detection using, presumably, Heston-Summers (PENN World Table) data. It is unfortunate, then, that he does not discuss, nor advocate a discussion for his students, on the methods used to generate this data. If he did so, he will find that the theory that underpins the construction of that data, essentially the use of the Geary-Khamis index, is violated in the numerical computation when producing the final users data sets! In any class that I teach, where estimation or simulation has pride of place, the first lessons are on the nature of the data: their origins, their scope, the methods used to prepare the for the final user, and so on - essentially old fashion index number theory; the kind that used to be common when teaching national income accounting.

This chapter is liberally sprinkled with phrases in inverted commas, parentheses, italics, unwarranted caveats and irritating typos. I was not sure when a phrase in inverted commas was meant seriously and when to be taken with a pinch or a barrel of salt. Equally, the unwarranted caveats contained dangerous mistakes or inappropriate or incorrect analogies to such an extent that I found it difficult to take the overall message too seriously.

There was, first, Biometriks and the Biometrika; then, Econometrics and the Econometrica. So why not also Bioeconomics? Stephen Magee's ambitious chapter is about 'Bioeconomics: Lessons for Business, Nations and Life'.

The chapter is full of interesting analogies and useful metaphors, from the life sciences in general and biology, molecular biology and genetics in particular, for the slightly unorthodox economic analyst. There was a time when such analogies were rich between the physical sciences in general and classical mechanics and phenomenological thermodynamics in particular provided the analogies and metaphors. Then there was pure mathematics in general and classical real analysis, matrix theory and dynamical systems theory in particular providing fertile metaphors and analogies for economic theoretic formalizations. More recently, solid state physics and statistical mechanics have entered this crowded arena. Somehow, Chemistry seems to have missed the boat. Its time, too, will come - if I may naively extrapolate the economist's reckless habits.

Magee's justification for including his message in this book is that the 'animal kingdom is a rich laboratory in which to learn economic lessons about complex systems' (p.256). An earlier generation told us that 'the physical world was a rich laboratory in which to learn economic lessons about *equilibrium, deterministic*, systems'. Over a century of this latter lesson has left the subject in a limbo from which Santa Fe visionaries, among others, want to rescue us. Magee's latter day saviours, those who have winnowed that animal kingdom and reaped a rich harvest and will now shower the benefits to allied fields are the Sociobiologists. Magee bears their message with conviction to recast problems of the individual, the firm and the nation in biological terms.

The way he thinks his message is of relevance, concretely, for the purposes of this book, i.e., for 'complexity and the teaching of complexity'(p.236) is given rather loosely in terms of an unrigorous - bordering on the incorrect - interpretation or understanding of the claims of chaos theory and some loose analogies about positive feedback, path dependence, evolutionary selection and punctuated equilibria.

One starting point for Magee's approach is his characterization of behaviour in a social context(p.252):

> In all of nature, there are only four logically possible ways for organisms to interact with others. Behavior can be cooperative, selfish, altruistic or spiteful.

I wondered about the place of the *benevolent dictator* and *the impartial observer*, venerable ideals in our subject, and their place in Magee's characterization. I also wondered about alternative logics that make 'logically possible' a many splendoured world. My thoughts went back to that sage Rabbi Hillel and his exquisite rendering of the human dilemma in a social situation:

> If I am not for myself, then who is for me?
> And if I am not for others, then who am I?
> And if not now, when?

Magee uses his highly speculative characterization to draw implications for man's economic and political behaviour. They may or may not be true; most likely both, but that kind of implication is probably disallowed in the logic that Magee employs. He then goes on to finesse this characterization with a finer stratification in terms of an analogy between the behaviour of roaches (r) and cows (K) - r vs. K strategies. The former exhibit opportunistic, myopic, behavioural propensities; the latter, the opposite. This dichotomy is utilised most audaciously to assert that(p.259):

> The r-K strategy distinction is insightful in explaining economic behavior between developing and advanced countries.

There is no point in offering prizes for which way the analogies go! Homilies on hierarchies and dominance, specialization and reproduction, power and democracy, common law vs, civil law, even a prediction of the death of nations enrich and adorn this speculative, confident, non-empirical, non-theoretical chapter without a single simulation or experiment to substantiate any of the grand themes.

Magee represents those issues in the life sciences that he draws upon for drawing metaphorical and analogical lessons as if they are undisputed truths in their own right. As if the doctrine and the underpinnings of sociobiology are undisputed truths. For someone who has, for example, read Kitcher's outstanding critique of Sociobiology ([49] many of the analogies and metaphors, with serious policy implications in economics and politics, would appear reckless. Magee, his themes, his aims and the implications he draws for economic and political behaviour, at every level, do not suffer from any of the vices of modesty. Also, their relevance for the main two themes of this book, complexity and the teaching of complexity, are, in the best case, tenuous,

Michael Rothschild, in the last chapter of this book, aims to 'present [a] side of complexity ... which suggests that the economics that follows from the complexity vision can be more relevant to business and the real world than is standard economics' (p.285). By now any diligent reader would have realised that it is hard to pinpoint *the* complexity vision; but let that pass. Rothschild, in the chapter titled, 'Complexity, Business and Biological Metaphors' does not try to be rigorous or precise; he concentrates, basically, on the empirical case for one concept - *the learning curve* - and its relevance for enhancing a complexity vision of economics.

Rothschild begins by stating his credo: from his 'business person's perspective', competition in the economic sphere, is best viewed as a biological equilibrium outcome, rather than a 'mechanistic' one. This perspective, therefore, brings with it an evolutionary, adaptive outlook, with which the business person is comfortable. This biological perspective enriches and enables, in addition, a vision of the economy as an 'ecosystem', developing spontaneously and generating, in the process, new orders and increasing complexity. The difference, according to Rothschild, between a biological ecosystem and an economic one is in the time scales of evolution; eons for the former, years or decades for the latter. The reason for this difference is *learning* and this is the central theme of this brief, discursive, chapter. The backdrop for learning as the central theme is the following (p.288; italics added):

> At most levels of biological structure there is *no* learning that goes on. ... What is different about the economy is that there is conscious, endogenous learning.

I am not sure what the neurophysiologist, epidemiologists, kinesiologists and others will make of this astonishing thesis; but the peculiar point is that the kind of learning Rothschild wishes to emphasise - via *the learning curve* - is independent of this strong thesis.

The evolutionary metaphor he draws on is obviously the current dominant orthodoxy of the Neo-Darwinists. He is not concerned with non-linear dynamics and the possibilities of 'mechanistic equilibrium' being given non-Darwinian (neo- or otherwise), evolutionary interpretations from which economists can derive useful metaphors[78].

This overall vision he calls *Bionomics: The Economy as an Ecosystem* - the title of a 1990 book by Rothschild.

As far as Rothschild is concerned 'learning is what causes firms and economies to grow', and since growth is viewed from the evolutionary perspective of Neo-Darwinism, this leads to spontaneous generation of new orders and increasing complexity over time. Hence, an understanding of the learning process underpinning growth is the central analytical task of the Bionomist. As far as Rothschild is concerned this learning process is encapsulated in *the learning curve*[79].

Economists are as guilty as charged by Rothschild: 'What is amazing is that it is not part of the economics curriculum' (p.291). But, then, I can - and many others in this book have - list(ed) oodles of concepts that are 'not part of the economics curriculum' and are relevant not only for a complexity vision but even for many other kinds of visions and perspectives that lend veracity to the subject. So, for a reader to take Rothschild's thesis seriously he must make a case for the learning curve to be included in the economics curriculum in some persuasive way. He makes that case in the following way:

> Here is the problem the learning curve presents for the standard texts. There, it is presented that the supply curve depends upon marginal costs, and that the supply curve slopes upward in the relevant ranges of output. The learning curve suggests the opposite, and thus undermines the basic notion of the upward sloping supply curve.

By now even the sympathetic reader would be itching to see an example of a formal learning curve and its workings in concrete economic settings. Not to

[78] As a matter of fact none of the authors in this book envisage the possibilities of a non-Darwinian evolutionary world. The tacit understanding seems to be that this is the only vision of evolution.

[79] It may be useful to point out that the 'learning curve' is also referred to as the 'progress ratio', 'progress function', the 'experience curve', etc.

worry; the textbooks may abscond from their responsibilities but responsible economists, from Armen Alchian and Kenneth Arrow to Werner Hirsch and Laura Tyson, have not. But the story begins at the beginning: with that famous pin factory in the *Wealth of Nations*[80].

As Rothschild correctly points out (pp.288-9), the concept of the learning curve, as understood today, was introduced to the aircraft industry in 1936 by T. P. Wright of Curtiss-Wright Corporation with an article in the Journal of the Aeronautical Sciences in 1936[81]. Wright's working hypothesis was the simple and intuitive observation that repeating an operation in a well-defined production process results in less time or effort expended on that particular operation. Those of us reared on *The Wealth of Nations* and *On the Economy of Machinery and Manufactures*, even if we have never visited a pin, aeronautical or any other factory, may, once again think of poor Monsieur Jourdain, but would not feel the slightest discomfort in this eminently sensible observation by Wright. When the hypothesis is more specific, it is stated as something like: the direct man-hours necessary to complete a unit of production will decrease by a constant percentage each time the production quantity is *doubled*[82]. The general idea, however, is exactly as Adam Smith and, following him, Charles Babbage made, referring to pin making: the amount of time re-

[80]The earliest formal economic work on this concept seems to have been Armen Alchian's RAND memorandum of 1949, [1], closely followed by work done by Werner Hirsch, also at RAND, and reported at the Santa Monica Meeting of the Econometric Society in 1951 [38] and, then, Hirsch's more complete paper of 1956,[39], to which Arrow referred in his seminal paper of 1962, ([2], p.156). These, although they do not exhaust the references in the more standard economic literature, appear to be the pioneering ones. However, it has been widely used in industrial policy contexts, taking for granted that firms are aware of it, use it and decide on intra-firm planning strategies on working with it ([40], p.128-9). Rothschild does list some of these references, although puzzlingly, has no reference to Adam Smith or Charles Babbage.

[81]Rothschild refers to 'Curtiss' and to the 'Journal of Aeronautical Science'; cf. Arrow, op.cit for a precise reference to Wright's paper. The Curtiss Aeroplane and Motor Company had become the Curtiss-Wright Corporation on July 5, 1929. It is, surely, appropriate to point out, in the context of the themes discussed in this paper and the contents of the books being reviewed, that Theodore Paul Wright was the brother of Sewall Wright - the architect, together with R.A.Fisher and J.B.S.Haldane, of the synthesis between natural selection and Mendelian genetics that is the core of Neo-Darwinism. Serendipity supreme!

[82]This is a kind of converse of the 'half-life' concept, an analogous empirical concept, equally popular and fertile at the firm level. The half-life idea is to try to formalize the notion that there is always a gap between potential efficiency and actual efficiency in any production or organizational process and to identify the principal culprits responsible for the gap. Thus, if 80% of the defects responsible for the gap can be attributed to 20% of the processes then the time required to reduce by 50% the gap between the potential and the actual is the 'half-life'. This is, of course, a concept borrowed from radioactive decay and drug absorption. The reason I mention this here will soon become evident.

quired for the completion of an operation in a process producing a well-defined unit of an item (pins?), is a decreasing function of the number of times it is performed and the quantity of the item produced. A simple formalization of this hypothesis is given by:

$$y = \alpha x^{\beta} \tag{52}$$

where:
y: the cumulative average time (or cost) per unit;
x: the cumulative number of units produced;
$\alpha$ and $\beta$ are parameters with the former denoting the time (or cost) required to produce the first unit;

Rothschild would have us place, on this not very robust functional form which is to encapsulate 'learning by doing', the whole weight of substantiating learning processes in the economic system and thereby also justifying and underpinning a complexity vision for the subject as a whole! I am not sure whether he takes this stand with tongue in cheek or not. And any further comments will end up by making this part longer than the chapter being discussed. Therefore, I shall confine myself to a few critical observations while not unsympathetic with Rotschild's perplexity as to why this concept does not form part of the standard curriculum, say in the section on production theory.

First of all, it does form a part of the current standard curriculum in macroeconomics, even at the undergraduate level, via endogenous growth theory[83]. By now most students are taught some version of this kind of growth theory and are, usually, given at least a minor dose of Arrow's 'learning by doing' approach to, if you like, the 'experience curve'. Secondly, even though I believe Rothschild is absolutely correct that the experience curve became *one of the bases* of competitive strategy via its focus on the *positive feedback* loop between market share, cumulative experience and unit cost, it remains, when used that way, without either behavioural or technical foundations. One of the other bases was the 'half-life method' on defects. Rothschild's allusion to Sony leapfrogging (p.292) tells only half the story when it is told on the sole basis of the experience curve. I refer the reader to [71], especially pp.28-9 (but cf. also [52], pp.224-5). Thirdly, economists who are cavalier about taking production formalisms as one of closures of the core of neoclassical

[83]Thus, the second part of footnote 6 in this chapter (p.295) is a puzzle. On the other hand the fact that the telephone interviews reported are from 1988 may explain the puzzle. Endogenous growth theory was still embryonic at that time, at least from the textbook perspective.

economics, assuming away the process and engineering underpinnings of that concept and formalism, should pay some attention to Rothschild's plea; I read it as a plea to put the idea behind the learning or experience curve to replace the conventional formalism of production as one of the triumvirate defining the neoclassical closure. But, where and how would Rothschild place the dual of the learning or experience curve: the half-life 'curve'; for the one without the other does not even capture half the factors underlying learning in any organization. Fourthly, even although some kind of positive feedbacks can be discerned in the interaction between policy based on the experience and half-life curves and the capture of market shares, how these are underpinned by a Bionomics vision remains a large unwritten chapter. Nothing in any of the current formalism encapsulating these concepts have anything to do with a biological or a Bionomics perspectives.

On that count the message of this chapter is a complete failure and, hence, its place in this volume is dubious, to say the least. Rothschild ends his chapter by re-asserting his credo that he is a 'supporter of the complexity vision' because 'it takes the focus away from equilibrium conditions and places it back on process'. But there are many other 'visions' that take the focus away from 'equilibrium conditions and place it on process' - for example a formalism of even standard general equilibrium theory from the perspective of constructive or computable analysis will do the same. I suspect that Rothschild's case for the prosecution hangs by its own shoe laces (or, if you prefer, bootstraps).

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