

# **The Economy as an Evolving Complex System II.**

W. Brian Arthur, Steven N. Durlauf, and David A. Lane, (Eds.)

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## **Review by Gerald Silverberg, Maastricht**

How does one instigate a scientific revolution, or more modestly, a shift of scientific paradigm? This must have been on the minds of the organizers of the two conferences "The Economy as an Evolving Complex System, I and II" and the research program in economics at the Santa Fe Institute documented in the present volume and its predecessor of ten years ago.(1)

Their strategy might be reconstructed as follows. First, the stranglehold of neoclassical economics on the Anglo-Saxon academic community since World War II is at least partly due to the ascendancy of mathematical rigor as the touchstone of serious economic theorizing. Thus if one could beat the prevailing paradigm at its own game one would immediately have a better footing in the community than the heretics, mostly from the left or one of the various 'institutional' camps, who had been sniping at it from the sidelines all the while but were never above the suspicion of not being mathematically up to comprehending it in the first place. Second, one could enlist both prominent representatives and path-breaking methods from the natural sciences to legitimize the introduction of (to economists) fresh and in some ways disturbing approaches to the subject. This was particularly the tack taken in 1987, where roughly equal numbers of scientists and economists were brought together in an extensive brain storming session. Physics has always been the role model for other aspiring 'hard' sciences, and physicists seem to have succeeded in institutional-

izing a 'permanent revolution' in their own methodology, i.e., they are relatively less dogmatic and willing to be more eclectic in the interests of getting results. The fact that, with the exception of a brief chapter by Philip Anderson in the present volume, physicists as representatives of their discipline are no longer present, presumably indicates that their services can now be dispensed with in this enterprise.(2)

Finally, one should sponsor research of the highest caliber, always laudable in itself, and make judicious use of key personalities. Care should also be taken that the work is of a form and style which, rather than explicitly provoking the profession, makes it appear as if it were the natural generalization of previous mainstream research and thus reasonably amenable to inclusion in the canon. This while tacitly encouraging and profiting from a wave of publicity in the popular media, a difficult line to tread if one does not want to appear frivolous and offend sensibilities. All of these sometimes conflicting considerations are necessary, it should be observed, because we are dealing with something about which the new 'Santa Fe' theory itself from its very inception has had something very relevant to say, namely lock-in to an inferior technology. For indeed the economics profession, particularly in the United States, displays many aspects of a system characterized by increasing returns to adoption, where it becomes increasingly impossible, more for sociological than for substantive reasons, to break out of a nar-

row mold enforced by a circular system of publication and promotion procedures. This had already been borne out by the experiences of both American and European iconoclasts of the 50s to the 80s in attempting to establish bounded rationality, increasing returns, evolutionary modeling, nonlinear dynamics, dissipative systems, synergetics and what have you in more than just marginal niches in the economics research agenda.(3)

Has the Santa Fe Institute succeeded where others have had only mixed success (but dared to tread)? This is not as simple a question to answer as it would appear, since there are many different criteria one could apply to measure success. In terms of popular consciousness the answer is certainly yes. It has succeeded in putting a certain style of reasoning into the media limelight,(4) but as I have noted, this can be a double-edged sword and thus a difficult act to balance with professional considerations, as the example of catastrophe theory has amply demonstrated. In terms of the mainstream, and in particular, the American mainstream (which may be almost synonymous), it has also succeeded in placing complexity onto some agendas, such as special sessions of the annual meetings of the American Economic Association, without becoming tainted by the patronizing odor previously associated with radical economics, Marxist economics, or possibly feminist economics, in such forums. In the forum that really counts, however--publication in leading mainstream journals--only now can one really identify a slow penetration, while more marginal journals still play a major role. And the Santa Fe Institute's own publication and working paper series should be recognized in themselves as significant contributions. More remarkable is the fact that another evolutionary school is already well established in the fast lane of the journal superhighway, namely evolutionary game theory. The reason may lie in its basically lesser radicalism, since it mainly resorts to radical new methods and bounded rationality to address an immanent game theoretic problem, that of equilibrium selection, and return the theoretical world to the comforts of uniqueness while opening a new mathematical playground to the suitably inclined.

The criteria I would finally like to apply are the internal and external substantive ones one would like to believe should ultimately guide scientific research (and in some long run probably do). Some of these are

enunciated with great lucidity in the editors' introduction and grouped under the following headings, which for convenience I will separate into two categories: I. dispersed interaction (plus: no global controller/cross-cutting hierarchical organization); cognitive foundations; structural foundations; II. continual adaptation; perpetual novelty; out-of-equilibrium dynamics; what counts as a problem and as a solution.

But before applying these substantive categories it is useful to see what kinds of contributions make up this volume. The first category consists of surveys and research contributions of a more technical variety by some of the principal people involved in or related to the Santa Fe enterprise. These in turn can be classified into primarily analytical vs. computational modeling exercises, and form the core of what one would expect from such a conference proceedings. The second consists of the chapters by Lane and Maxfield and by Geanakoplos, which are intriguing mixtures of business history storytelling and verbal theorizing in the former, heavier analytics in the latter. The third is what, for lack of a better phrase, I would call 'observations by distinguished outsiders': papers by North, Leijonhufvud, and Anderson that discourse freestyle on concrete economic topics related to the Santa Fe research agenda, but neither themselves employ any of the new methodological innovations nor show any great readiness to connect to the relevant existing literature in order to suggest a step in that direction.

How do the contributions in the first category measure up to the ambitious goals outlined in the introduction? In terms of the structural considerations I think there can be no doubt about the originality of the enterprise. With the exception of Arthur et al. (stock market double auction, but with heterogeneous, boundedly rational agents), Shubik (game theory) and Geanakoplos (general equilibrium), many authors do investigate a wide range of (nonstandard, i.e., non star shaped, non full strategic game theoretic, and in the meantime non random matching) interaction structures. This is particularly the case in the chapters by Durlauf, Ioannides, Blume, and Kirman. Endogenously evolved network structures seem to be the way to go for many applications, and this book is an excellent resource to find out which mathematical methods look promising and what the literature already offers in terms of results.

Bounded rationality is another piece of the puzzle to which more than lip service is being rendered, receiving considerable modeling attention and gradually finding its place in the methodological puzzle. This is mostly due to a loose consensus about the applicability of certain computational methods, particularly genetic algorithms and classifier systems, to the problem of choice, behavior, and (social) learning. A number of chapters have learning algorithms and social interaction as their primary focus. To my mind the two most valuable ones are Arthur et al. and Lindgren, for not only do they address central economic and social problems (the stock market in the former, the evolution of cooperation in the stylized form of the iterated Prisoner's Dilemma in the latter), but they also most explicitly take up the challenge of the second half of the editors' catalog. First, boundedly rational behavior is coded in high dimensional and sparsely populated spaces. In fact, one of Lindgren's models operates in an unbounded space through the use of a clever genetic operator: the 'chromosomes' coding for the next move on the basis of a finite history of previous moves can double in length by self-duplication. This should be contrasted with the analytic literature on evolutionary games, where the strategy space is usually finite and small (often consisting of only two strategies), and full support is assumed (all strategies are initially represented in the population, i.e., there is no novelty). At one fell swoop the promised land of continual adaptation, perpetual novelty, and perennial out-of-equilibrium dynamics comes into view.

Second, the emphasis on computational analysis opens another fruitful can of worms--the question of what constitutes an analysis and a 'solution.' Take the example of Arthur et al.'s artificial stock market. There agents can formulate a huge variety of trading rules which are activated with a probability proportional to their previous success. The evolution of the market is then the result of the continuing interaction of this 'ecology of rules' bidding and trading with each other, against the background of another dynamic governing the injection of new rules and the elimination of old ones. What we get out of the model, varying with the parameter determining these relative time scales, on the one hand is an artificial time series which can be compared with the predictions derived from a rational expectations equilibrium of an efficient markets model.

On the other it is the realization that agents' expectations remain heterogeneous and that the ecology of rules, for high values of this parameter, does not appear to converge to some long-period statistical equilibrium, i.e., it may not be stationary. While the time-series properties of empirical asset prices have been intensely studied and are still controversial (and the authors could certainly go further in benchmarking robust features of their model against such features as fat tails, GARCH properties, long memory, self-similarity, etc.), the conclusions about expectations and nonstationarity (in terms of market 'moods,' herd effects, technical trading) are still only amenable to 'anecdotal' verification and thus may not make much of an impression on believers in efficient markets. Serious thought still has to be given to how such models can be tied to reality and what statistical data have to be gathered to make their conclusions plausible, a problem that plagues to an even greater extent the more remote abstractions of other chapters.

What can we take away from the other computational contributions? Alas, much will be internal to questions of modeling, I am afraid, and less about verifiable aspects of the real world, but at this stage in the game I do not think this is necessarily a damning criticism of a young and rapidly developing field. Lindgren's treatment of the iterated Prisoner's Dilemma is remarkable as well for its nonstationarity conclusions: rules may come to dominate the system for long periods, but they are usually overthrown eventually. The representation of the strategy space (e.g., memory look-up rules vs. finite state machines) and the imposition of spatial structure can make a huge difference in outcomes.

The contributions of Darley and Kauffman; Padgett; Kollman, Miller, and Page; and Tesfatsion (who should win a prize for the insolence of her title alone, although the full connotations of the pun may be obscure to non-Americans) take us into often surprising applications and some technical thickets. What may be an obstacle to the wider acceptance of this style of modeling is that these applications, while dealing with highly stylized and abstract versions of their subject, are still too particular and idiosyncratic to lend themselves to easy generalization. Therefore the investment in the modeling effort may appear excessive compared to the robustness of the results, which lack the canoni-

cal appeal of such an obviously simplified representation of reality as Prisoner's Dilemma. This may be the Modeler's Dilemma: whether to go for a real-world problem in all its specific gory detail, or find a highly stylized problem which is transparent and can be analyzed in detail but remains only a suggestive metaphor for the real world. The fate of anything in between may simply be to get lost in the literature of high sunk cost, one-off models.

Thus Darley and Kauffman propose replacing 'rational' agents by adaptive agents, and show that complex dynamics can result from their particular setup. But will any of this structure be applicable to another economic context, i.e., will it form part of a growing evolutionary/adaptive toolkit? I doubt it, although the conclusion is well taken. Padgett, and Kollman et al. explore problems a bit far from the interests of a majority of economists, I suspect, and I am not in a position to judge what lasting value these initiatives will have in their domains, as technically interesting as these models are. Tesfatsion certainly addresses those interests more closely in her chapter on trading networks. The value of what she discusses, as she herself recognizes, however, lies more in the demonstration effect of showing how such problems can be attacked and what difficulties arise in reaching specific results, rather than in establishing robust conclusions. These may well be in the offing with some combination of simulational and analytical efforts, but it looks like a substantial technological thicket still has to be traversed before we will come into that clearing.

I have already talked a bit about the more mathematically formulated papers in terms of structural foundations. As to specific methods, the volume presents a rich mixture of overviews and elaborations. Lane returns to Santa Fe's inspirational source--the Polya urn and increasing returns--to explore aspects of the information contagion model and the tensions between individual and social choice. Durlauf and Krugman provide masterful overviews of statistical mechanics approaches to interaction, and nonlinear dynamics in economic geography, respectively. While Durlauf displays a firm command of the technical jargon and notational complexity, Krugman's chapter is a model of nontechnical and lucid exposition, and it is a matter of taste as to which style one prefers. In one respect they both suffer from the 'not invented here' syndrome,

however, where 'here' refers perhaps less to the USA than publication in mainstream economic journals.(5)

The chapters by Ioannides, Blume, and Kirman also deliver high quality reviews of interaction models, both evolutionary game theoretic and otherwise. What is paradoxical about all these analytical approaches to evolution, however, is that they always seem to come back to a stochastic or systems dynamic concept of equilibrium, and thus fall short in this respect of the editors' vision of open endedness (although Blume does devote a section to the question "How long is the long run?" and the practical relevance of asymptotic results).

High theory naturally leads to high econometrics to make the connection with data. This book provides two examples. Brock contributes a comprehensive guide to the time-series empirics of asset prices as well as a tie-in to various complex dynamics models, and as usual this review is about as definitively state-of-the-art as we will probably ever get. Manski's chapter on "Identification of Anonymous Endogenous Environments" is a highly abstract approach to this topic, but I for one would have been well served by a concrete example of an application to real data.

But this volume also treats us to two unusual--for an economics treatise--chapters taken from the book of life itself, so to speak, and among all of this high-level theory, econometrics, and ALife they are a breath of fresh air. At the same time, they illustrate in two rather different ways just how problematic the scientific enterprise may really be, and that for all the fun and games of modeling a certain skepticism should be brought to the question of its real, everyday decision-making relevance. First there is Geanakoplos' reflections as an academic economic theorist on his five years as a practitioner on Wall Street. This personal memoir on what this turbulent experience meant should be applauded for its self-critical openness, culminating in the question (p. 291): what good did Kidder Peabody do, and how can it be that in a world of rational investors it is possible to make money on the sell side of a market, and then on the buy side of the same market?

Yet Geanakoplos then proceeds to formulate a general equilibrium model of his problem in which, at least to my way of thinking, all of this turbulence and out-of-equilibrium behavior (in the sense of the Arthur et al. asset model) get thrown out the theoretical win-

dow. Evidently one cannot teach an old horse, however brilliant, new tricks, although the model may be a general equilibrium tour de force.

Lane and Maxfield's chapter is an equally valuable first-hand excursion into the world of business, this time the entrance of the Rolm Corporation into the nascent PBX telecommunications market of the 1970s. The first half of the chapter describes the evolution of the PBX market as a user/producer learning process in which no one had any entirely clear idea of what they were doing, but where in retrospect certain strategies proved eminently successful. This makes for a good read, since Maxfield, like Geanakoplos, was a key player with an inside view of mentalities and events. The second half attempts to derive 'lessons' from this story, such as Lesson 4 (p. 186):

The "window of predictability" for the attributional shifts and structural changes that characterize complex foresight horizons are very short--and virtually nonexistent outside the particular generative relationship from which they emerge.

Here, in their attempt at extracting some sort of generality from their particular narrative, the authors may have inadvertently hit on a deep problem in the complex systems perspective when it comes up against decision-making reality. For the danger may lie either in its dissolving into high-flown inanities of little or no practical utility (something I am afraid it probably shares with much of the management literature). Or it may wind up making such specialized predictions, under such restrictive but in practice unverifiable conditions, that one can never know when it will be applicable. Can we ever step into the same Heraclitian river twice?(6) And that is possibly why the two stories in this volume seem to stand head and shoulders above the theoretical exercises (formal or informal) their authors seem compelled to pair them with. In the recognition that perpetual novelty and out-of-equilibrium dynamics are the touchstones of the next modeling revolution, may we not be forced, whether we like it or not, to come full circle to the position of the German Historical School in the long-buried Methodenstreit--that each historical situation is unique, and thus only narrative is possible and not axiomatic theory à la physics?

I personally do not think the situation is quite as hopeless as this (although I am always in favor of a

good narrative, and the editors should be commended for the unusual experiment of including these two). On the one hand there are statistical regularities, such as Anderson mentions, which characterize large domains of the social sciences, and are still begging for explanation.(7) On the other there are a number of developmental patterns and (ir)regularities in the historical record of institutional and technological change crying out for systematization and a basic theory, to which North partly alludes. But it is remarkable to note that, for a book purporting to be on the economy as an evolving complex system, there is next to nothing on technical change as one of the fundamental driving forces of economic change. We have indeed come a long way from Nelson and Winter.

In summary, this book sets out an ambitious program to which it only partially lives up.(8) But even in failing to achieve all of its goals it presents a spectrum of attempts of the very highest order. Thus the first ten years of the Santa Fe Institute have amply demonstrated that economists can get ALife if they want to. The question that still remains to be answered, however, is whether there is real life after ALife.

## Notes

1. Anderson, P.W., Arrow, K.J. and Pines, D.(eds.), 1988, *The Economy as an Evolving Complex System*, Redwood City, CA: Addison-Wesley.

2. To be sure, computer scientists (including erstwhile physicists) and mathematical biologists have become more actively involved in the modeling effort. It should be noted that one of the greatest bones of contention in the first meeting revolved around what many physicists perceived as the economists' fetish of mathematical rigor at the expense of empirical relevance. Perhaps this was an additional argument against a repetition of the original exercise.

3. Names worth recalling in this context are Herbert Simon, Richard Day, Richard Nelson, Sidney Winter, Ilya Prigogine, Herman Haken, and Wolfgang Weidlich, to name only a few.

4. As evidenced by the success of Waldrop, M. M., 1992, *Complexity. The Emerging Science at the Edge of Order and Chaos*, New York: Simon & Schuster,

which, while a remarkably coherent popularization of 'complex' ideas, veers towards an almost romanticized personalization of how science is done, and exposure in other media.

5. Thus statistical mechanics methods and Ising-type models of exactly the type Durlauf discusses were developed and extensively applied to the social sciences starting in the 1970s not only by Föllmer--whom he cites--but also by the Stuttgart school of synergetics (Weidlich, W. and Haag, G., 1983, *Concepts and Models of a Quantitative Sociology*, Berlin: Springer Verlag; Weidlich, W., 1991, "Physics and Social

(eds.), *The Behavioral Foundations of Economic Theory*, special issue of *The Journal of Business*, 59: S427-434.

7. The virtual neglect of fractality and self-organized criticality (cf. Bak, P. and Chen, K., 1991, "Self-Organized Criticality", *Scientific American*, January 1991: 26-33) in this volume is surprising, considering how well established they have become in the complex systems literature.

8. A more prosaic goal also needs some attending to, namely, the quality of the copy editing.

Science—the Approach of Synergetics", *Physics Reports*, 204: 1-163). And much of the complex dynamics in spatial models Krugman discusses was also being intensively investigated in the 1970s by the Brussels school (e.g., Allen, P. M. and Sanglier, M., 1981, "Urban Evolution, Self-Organization, and Decision-making", *Environment and Planning A*, 13: 167-183).

6. Cf. Winter, S.G., 1986, "Comments on Arrow and on Lucas", in R.M. Hogarth and M.W. Reder