

Response

Chaos and Complexity: Their Bearing on Social Policy Research

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'What fascinates me about these models is that besides creating theory, besides corroborating our theories with real-world data, besides coming up with new principles, we are also beginning to develop metaphors. And I have a very strong belief that science and thinking progresses not so much by theorems but by metaphors. I think this Institute is certainly in the business of formulating theory, principles, and doing experiments. But at a deeper level we are in the business of formulating the metaphors for this new science, metaphors that, with luck, will guide the way these sciences are done over the next fifty years or so.' (Arthur 1994a: 680 *Emphasis mine*)

With this statement, Professor Brian Arthur, one of the guiding lights of the Santa Fe Institute, brought a sudden clarity and consensus to a gathering of scientific luminaries who had assembled at the Institute to discuss Complex Adaptive Systems (CAS). Arthur's quiet and commonsense suggestion is that, ultimately, the idea of Complex Adaptive Systems was itself a master metaphor for the New Science of Complexity. The power of the complexity metaphor generally and of the CAS metaphor in particular is amply attested to in this special issue of *Social Issues*. Contributors were asked to address two of the most vexing issues now facing the social sciences: first, what is Complexity Theory, and what is its relevance to the current practice of the social sciences; and, second, what bearing does Complexity Science have for understanding the present state of policy studies.

The difficulty of both tasks is evidenced in the diverse range of answers the contributors to this set of essays provided. This diversity is emblematic, I feel, of two things: (1) the manifold of unsettled issues still marking the 'New Science of Complexity', and (2) the state of anomie existing in the field of policy formation and policy research being generated by the disorientation which globalization, the latest stage of imperialism, has caused in the industrial democracies as their political leaders renegotiated the social contracts of the Keynesian/Fordist synthesis which the state struck with its citizenry some six decades ago (Harvey 1989). In what follows, I will first examine the metaphor of complexity and its potential role in addressing the current crisis in policy formation and policy studies, and, second, I will synthesize and critically summarize the contributions of those in this volume who have tried to link Complexity Theory to the practical problems of policy research.

I. Of Chaos and Complexity

Two recurrent themes among those who assembled in Santa Fe in 1994 (see Cowan et al 1994) to discuss Complex Adaptive Systems were the feasibility of CAS serving as a general paradigm for studying non-linear systems, and the role the Santa Fe Institute should play in promoting and disseminating its version of Complexity Science. The second subordinate theme was what to do with the insights offered by so-called Deterministic Chaos. For the more orthodox, Deterministic Chaos Theory seemed a dead letter issue. They insisted on using a vocabulary which equated chaos with randomness and system breakdown, while continuing to claim that Complexity Science as developed at the Institute was working in that shadowy domain where non-linearity thrives, i.e. on the very 'edge of chaos'. Moreover, whenever the issues of *change via saltation* (i.e. an abrupt or sudden transformation) was raised, or the possibility that punctuated equilibria conceptions of evolution might be used as a model for understanding mechanisms of emergence, the idea was usually pooh-poohed and the exchange politely diverted to more commodious subjects. Similarly, when the crucial thesis that the stimulus for an emergent evolution of internal hierarchy within the CAS might lie in a radical

transformation of the environment, rather than indigenous learning and internal development of new 'schema' from within the CAS itself, the idea more often than not died for lack of interest.

This is not to say that there was no sympathy for those who entertained augmenting Complex Adaptive Systems theory with ideas commonly associated with Chaos Theory. Indeed, Brian Arthur mounted a passionate, clear-headed defense of punctuated equilibria in his paper, 'On the Evolution of Complexity' (1994b). Presenting a powerful analytic brief for the position that co evolutionary processes were at the heart of the development of innovation and emergent hierarchies within CAS, he noted that the emergence of new entities or structures within an evolving system was the product of:

.... two positive feedbacks - circular causalities - [are] inherent in this mechanism. The generation of new entities may enhance the generation of new entities, simply because there is new 'genetic material' in the system available for further 'adaptive radiation.' And the appearance of new entities provides niches for the appearance of further, new entities. In turn, these mean that where few new entities are being created, few new entities can appear; thus, few new niches will be created. And so the system will be largely quiescent. And where new entities are appearing rapidly, there will be a rapid increase in new niches, causing further generation of entities and further new niches. The system may then undergo a 'Cambrian explosion.' Hence, we would expect that such systems might lie dormant in long periods of relative quiescence but burst occasionally into periods of rapid increase in complexity. That is, we would expect them to experience punctuated equilibria.

This mechanism, whereby complexity increases via the generation of new niches, is familiar to most of us who study complex systems. Certainly, Stuart Kaufman has written extensively on various examples of self-reinforcing diversity. Yet strangely it is hard to find discussion of it in the traditional biological literature.'

(Arthur 1994b 68-69)

This implicit defense of Niles Eldredges (1985, 1985a) and Stephen J. Gould's ideas in this passage, as well as his not-so-oblique reference to Gould's (1989) masterful account of the Burgess Shale, is unmistakable. Moreover, his evocative image of a dialectical dance of entity and niche engaged in mutual self-production seethes with an energy that verges on the poetic.

Compare Arthur's description of coevolution with a quite competent summing up of the properties and dynamics of Complex Adaptive Systems and the contemplative relation such systems have to their immediate environment as they go about learning and developing an internal hierarchy of complexity. Here is Professor Ben Martin of Stanford University's Psychology Department giving a terse definition of the kind of Complexity Theory promoted by the Santa Fe Institute:

'The Santa Fe Institute exists to promote the study of a variety of natural and artificial systems that defy traditional mathematical and empirical methods of analysis. As a rule, these systems are *adaptive*; changes in their internal states occur in response to the environment. They are also *complex*; changes in their behavior are not linearly related to changes in their surroundings. By bringing together researchers with diverse interests, the institute encourages explorations of the general properties of complex adaptation. Many complex adaptive systems develop a representation of environmental information; their internal structure comes to reflect external conditions of the world. As a result, behavior arising out of plans or predictions based on internal states can lead to successful action. Successful action yields high

evolutionary fitness in systems under adaptive pressures, or high performance in natural or artificial goal-directed systems.' (Cowan et. al. 1994, 263-264)

While I have no intention of suggesting some deep seated split within the Santa Fe Institute, or to claim that some fatal flaw exists in Complexity Theory itself, I do want to underscore the fact that any strict attempt to segregate Complexity Theory and its Santa Fe variant from the Deterministic Chaos Theory has developed in the last two decades by Ilya Prigogine (1984) and his Brussels School, or by those like Eldredge and Gould who have handsomely mounted their work in punctuated equilibria on principles consistent with Deterministic Chaos, will end in failure. This should be immediately apparent to anyone who, in fact, picks up George Cowan's splendid volume *Complexity: Metaphors, Models and Reality* (1994) and examines the papers and the transcribed exchanges found therein. The same lesson is driven home when one reads several of the essays in this special edition of *Social Issues*, for while the authors ostensibly have written on Complexity Theory, they almost invariably use a vocabulary associated with Deterministic Chaos Theory and its study of time-dependent, dissipative systems.

This tendency toward the elision of the two approaches to complexity lies in the fact that Chaos Theory and the Complexity Theory both have a common ontological field of investigation: nonlinear systems and their evolutionary elaboration over time. They differ in that the Complexity Theory of the Santa Fe Institute is currently concentrating its energies on mathematically modeling the inner structuration or *internal subsystem* of complex systems, while Chaos Theory as articulated by Ilya Prigogine and the Brussels School have used models from statistical, non-equilibrium thermodynamics to study the *external system* of complex systems. To the extent this characterization holds, the two can be seen as holding complementary positions in the way they tackle the problems of nonlinear systems analysis. Moreover, there are even grounds for arguing that, insofar as both approaches are expansive in their research agendas, their perspectives might even be convergent.

In noting the possible complementarity of Chaos Theory and Complex Adaptive Systems research and their possible convergence, we must not lose sight of the profound differences separating the two. For example, the subtitle of Mitchell Waldrop's (1992) best selling volume *Complexity: The Emerging Science at the Edge of Order and Chaos*, signifies his desire to distance Complexity Theory from Chaos Theory. And, indeed, there is a tendency among some members of the Santa Fe Institute even today to conflate the terms 'chaos' and 'randomness' in a way that denies the basic validity of Chaos Theory. Indeed, I would venture that the insistence on the part of many associated with the Institute that their research works 'at the edge of chaos' is a shibboleth that has generated much unneeded confusion.

More than nominal differences, however, separate the two approaches. If they share the same ontological object - i.e., nonlinear systems - there are profound differences in the scientific and social agendas they pursue, as well as the style of scientific praxis to which each is committed. For example, complexity science in the Santa Fe mold is largely an adjunct of standard science. As such, it has been framed in the mode of institutionalized science, what some have derisively called 'Big Science.' Complexity science from this perspective is a mere 'technical rounding out', so speak, of time-tested methods which must now be extended into the domain of non-classical, nonlinear systems. By contrast, Chaos Theory, though itself as unmistakably a creature of Big Science as the Santa Fe Institute, has nonetheless put forth an agenda of reform for the entire scientific enterprise. It seeks to create a unified science by 'humanizing the physical sciences', while the more or less technocratic credo of the Santa Fe Institute seeks a unified science by 'scientizing the arts and the humanities'. Perhaps we can sum up the difference between the two approaches by saying that Complexity Theory has sought to *reform* the standard science model while Chaos Theory has sought to *revolutionize* science and learning in general.

The contrasting agendas of Chaos Theory and the Complexity Research carried out under the auspices of the Santa Fe Institute, can be seen in the direction CAS theory seeks to take complexity science. CAS as it now stands starts from a mechanical view of the world and seeks through the use of modeling and computer simulation to extend this powerful mechanistic metaphor into the organic domain of the life sciences, into studies of human agency, and, eventually into the humanist domains of history and the social sciences. For all its talk of 'modeling emergence' and simulating hierarchical feedback, however, those currently leading CAS research are still sunk in methodological

individualism and a reactive conception of agency, human or otherwise. No matter how many emergent levels CAS modeling efforts have been able to simulate or spontaneously generate, this perspective has yet to effectively produce a systemic model in which a whole/part interaction based upon either negative feedback or positive feedback (or preferably both) can generate a holistic conception of a self-regulating or far-from equilibrium system capable of saltational possibilities.

Indeed, the forte of CAS research seems to be in describing and simulating *lower level adaptive systems*: its effectiveness approaches a rate of diminishing scientific returns, however, with its simulation of a psychological mechanism of learning and its conception of agency. With collective learning at the institutional level and the social construction of reality, the proper domain of sociology and history, CAS research admittedly encounters a barrier it has yet to broach. And, it may well be that the barrier to completing the Santa Fe Institute's agenda of constructing a unified science of complexity spanning both the sciences and the humanities may be impaired by its methodological commitments - i.e., by a definition of science predicated, thus far upon a bedrock of mathematical modeling and computer simulation techniques. This commitment to mathematical modeling is amply communicated by David Pines when he discusses what the Institute's agenda has of value to offer the social sciences:

.... I think there are a whole range of problems, however, that aren't going to be handled analytically. And this has everything to do with the social sciences, and probably a great deal to do with biology, ecology, etc. And there I think our responsibility is to really define what we mean, if we write 'computational social science'. Not try to avoid the word, but take advantage of the opportunity to define it and to make it as respectable a branch of science as computational physics is. I think a lot of the people in this room are capable of contributing to developing this kind of approach. And, were I to try to pick out one sort of niche for the Santa Fe Institute in which it could play a really substantive role, given the relatively small resources that we have available on the one hand, and on the other hand our extraordinary convening power, I would say it's in trying to do a proper job with computational ecology, computational social science, computational economics, and so on. I think we're capable of doing it, and I hope very much we'll take advantage of this niche. [T]his is, I believe, the future of social science: To do carefully designed numerical experiments that can be reproduced by anyone else. I think it enables one then to handle the problems that are before us. It's going to take a very long time, and only the very tiniest first steps have been taken.' (Pines and Cowan in Cowan, et. al. 1994: 674)

While Pines frank advocacy of a computational social science may not go down well with the humanists or the Postmodernists amongst us, it is a terse, admirable, and perceptive mapping of the Sant Fe Institute's future promise. That future is, more or less, a linear extrapolation of its original mission statement and advocates an agenda which can be of immense value to more postivistically inclined social scientists. Indeed, Murray Gell-Mann in another context expresses the same ecumenical sense of mission when he speculates upon the future requirements of those who would build a social science of complexity:

'I continue to maintain we need to pay much more attention to society, to social science, to try to find those few social scientists who don't suffer from crippling math phobia but are, nevertheless, not the kinds who trivialize social problems by mathematizing them. We have to keep searching for such people, and I think the economics effort needs to be informed by being embedded more in the study of society in general, and culture in general.' (Gell-Mann in Cowan, et. al., 1994: 661-662)

There is, of course, built into Gell-Mann's offering of the olive branch to the social sciences a set of self-serving assumptions and self-inoculating justifications. It assumes the main barrier to the social sciences adopting the CAS perspective and methodology is the 'crippling math phobia' from which social scientists suffer. And as a bona fide mathphobe myself (I attend weekly meetings of Numbers Anonymous) there is a measure of truth in his statement. At the same time, by taking this Procrustean stance, Gell-Mann finds a convenient way to ignore the possibility that the assumptions and modeling methods employed by CAS researchers may be severely limited when it comes to studying human agency, society, and history.

Be that as it may, while we cannot treat CAS simulation and Santa Fe Complexity theory as identical, by treating the CAS perspective as an ideal type of complexity science done in the fictive 'Santa Fe Style', we have a means for contrasting the perspectives of Complexity Science and its antipode, Chaos Science. One need merely peruse Prigogine and Stenger's (1984) landmark statement, *Order out of Chaos*, or Prigogine's later work with Gregoire Nicolis, *Exploring Complexity* (1989) to recognize the difference between the CAS agenda and Chaos-based forms of inquiry. First, the subtitle of Prigogine and Stenger's volume, when translated into English is '*Mans New Dialogue with Nature*', a phrase signifying an expected sea-shift in how intellectual life generally, not just the physical sciences, would be done in the future. Indeed the ecumenical spirit of Prigogine's vision of chaos theory is amply manifested in many of the works of the Brussels' School published during his era. Nowhere is this better seen than in a volume entitled *Time, Rhythms, and Chaos in the New Dialogue with Nature* (1991) edited by George P. Scott, who was at the time a research fellow at the Ilya Prigogine Center, University of Texas. The essays contained therein are written by scholars drawn from a wide range of academic disciplines. Their task was to show how the metaphors of Chaos Theory might be usefully applied to their respective fields. And while, with a few exceptions, these essays were either too arcane or too undistinguished for general use, each communicates the intellectual excitement and liberating effect the chaos perspective can have.

The point of these comparisons is not to pillory CAS modeling for its 'narrow, technocratic vision of complexity science when compared to the broad humanist impulse of Chaos Theory. Nor is it my intention to promote the chaos paradigm of Complexity Science to the detriment of its Santa Fe version. Far from it. Instead, I wish to argue, first, that we in the social sciences are in no position to jettison arbitrarily any approach merely because it offends our methodological or ideological biases. Indeed, I want to argue for the reconciliation of the complexity and chaos paradigms on ontological grounds - i.e., grounds that are already apparent in the 'confused usage' and mixing up of metaphors that so often occurs whenever we are asked to explain to someone in the profession what is so special about nonlinear dissipative systems and why we, as social scientists, should care, much less bother. In that complex dynamic systems are functionally and structurally differentiated into internally replicative and externally oriented adaptive subsystems, both chaotic and complexity moments of the New Science are required if we are to grasp the full, totalizing behavior nonlinear systems involve. The CAS perspective has shown itself to be imminently qualified to deal with the fundamental modular mechanisms by which a complex system is recursively constructed internally, from the bottom-up. Likewise, the chaos perspective has shown itself to be a promising approach to grasping molar, totalizing processes by which holistic mechanisms regulate or advance, through either positive or negative feedback mechanisms, evolutionary transformation.

The intellectual errors leading into scientific *culs de sac* have thus far been committed by those who have chosen to ignore one or the other approaches to the study of nonlinear systematics. I have in mind two instances: first, the debacle of Catastrophe Theory in which sound mathematical models of social phenomena were constructed without giving due attention to the limitations social factors placed on naturalistic modeling. Second, there is the recent *Social Texts* scandal (Sokal 2000; Sokal and Bricmont 1998) in which scholars employing the art of hermeneutic interpretation, a necessary adjunct to social science research, saw fit to ignore the objective nature of the world which is natural science's subject matter, and sought, instead, to read the writings of physical scientists as merely one among many alternative 'Texts.' In each case, a partial perspective moved to claim a universalizing totalized knowledge which was ontologically and epistemologically impossible to deliver given the current state of the arts and the sciences [\[1\]](#)

One path by which the two approaches to the study of nonlinear or dissipative systems might be reconciled without undue bias or damage accruing to either perspective is the punctuated equilibria

framework for analyzing evolutionary processes - both biological and social - that has been developed by Niles Eldredge and Stephen J. Gould.. While space does not allow us to develop this argument fully, we will merely note that the theory of biological evolution embodied in the punctuated equilibria framework fully accommodates the internal/external division of dissipative systems theory and has shown its capacity to be applied heuristically and metaphorically to large-scale social and cultural systems. Such an integration of convergent perspectives to form a single coherent theory of nonlinear evolution via chaos/complexity is entirely plausible. How it would be achieved is a project that should rank near the top of the New Science's agenda. We do know this, however, Brian Arthur (see above) and evolutionary biologists like Stuart Kauffman have already opened up the possibility for such a dialogue to begin. And, from the ranks of Chaos Theory itself, we find Ilya Prigogine (1996) now writing:

'... there exists a multiplicity of evolutions, which are particularly evident in the field of biology. As stated by Stephen J. Gould, bacteria have remained basically the same since the Precambrian era, while other species have evolved dramatically, often over short time scales. It would therefore be a mistake to consider a simple one-dimensional evolution. Some two hundred million years ago, certain reptiles started to fly, while others remained on earth.... Similarly certain apes evolved into humanoids, while others did not.'

At the conclusion of this chapter, it is appropriate to cite Gould's definition of the historical character of life:

'To understand the events and generalities of life's pathway, we must go beyond principles of evolutionary theory to a paleontological examination of the contingent pattern of life's history on our planet - the simple actualized version among millions of plausible alternatives that happened not to occur. Such a view of life's history is highly contrary both to conventional deterministic models of Western science and to the deepest social traditions and psychological hopes of Western cultures for a history culminating in humans as life's highest expression and intended planetary steward.

We are in a world of multiple fluctuations, some of which have evolved, while others have regressed. This is in complete accord with the results of far-from-equilibrium thermodynamics. But we can now go even farther. These fluctuations are the macroscopic manifestations of fundamental properties of fluctuations arising on the microscopic level of unstable dynamical systems. The difficulties emphasized by Gould are no longer present in our statistical formulation of the laws of nature. Irreversibility, and therefore the flow of time, starts at the dynamical level. It is amplified at the macroscopic level, then at the level of life, and finally at the level of human activity. What drove these transitions from one level to the next remains largely unknown, but at least we have achieved a noncontradictory description of nature rooted in dynamical instability. The descriptions of nature as presented by biology and physics now begin to converge.' (Prigogine 1994, 161-162)

Since we have been speculating about possibilities for the last page or so, this seems to be a proper place to end our discussion about creating a synthetic science of complexity. We have let Prigogine have the last word, to be sure, but we could just as easily ended with Kauffman or Arthur.

Either way, the point would be the same. Today, social scientists are working in exciting intellectual times. The conscious formulation of metaphors by physicists and biologists trying to grasp the nature of nonlinear systems and their dynamics is a boon for the social sciences. We have never been given richer materials with which to work and to apply to our own studies. The greatest dangers for the social sciences in this age of conceptual riches and rising expectations, however, is that we over-literalize or reify metaphors so as to appear to be on an academic par with the physical sciences. Hopefully, we are past that period of reifying the social in order to satisfy a desire that some wags

have called 'physics envy.' And, after the *Social Text* debacle, we have been cured, for the moment at least, of treating the metaphors as though they were mere 'free-floating signifiers' that can be interpreted as our personal circumstances dictate.

II. The Nonlinear Foundations of Social Policy

The essays in this volume offer the reader a fairly exhaustive picture of the focal concerns of current policy studies, the diversity of problems currently facing scholars in that field, and a varied list of ways in which Complexity Theory might have a bearing on critiquing and fine tuning future policy research and programs. On the basis of this collection of essays, I would hazard the following molar characterization of the policy sciences. First, and foremost, the goals of the policy sciences and their researches are inherently nonlinear in their formal structure and paradigmatic goals. That is, policy research seeks to discover ameliorative solutions to social problems in which small changes in the initial conditions of the life course of a person, a community, or an institution will produce great changes in the final outcome. This is a paraphrase, of course, drawn from Henri Poincaré's famous discussion of nonlinear processes in which he states:

A very small cause which escapes our notice determines a considerable effect that we cannot fail to see, and then we say that the effect is due to chance. If we knew exactly the laws of nature and the situation of the universe at the initial moment, we could predict exactly the situation of that same universe at a succeeding moment. But even if it were the case that the natural laws had no longer any secret for us, we could still only know the initial situation *approximately*. If that enabled us to predict the succeeding situation with the *same approximation*, that is all we require, and we should say that the phenomenon had been predicted, that it is governed by laws. But it is not always so; it may happen that small differences in the initial conditions produce very great ones in the final phenomena. A small error in the former will produce an enormous error in the latter. Prediction becomes impossible, and we have the fortuitous phenomenon' (Cited in Campbell and Rose 1983,vii).

Whether the focus of policy research be upon discovering ways of empowering local groups, the diremption of individual cases and the establishing of the way in which they embody major social forces, restoring run-down neighborhoods, defining and combating homelessness, understanding sudden shifts in university curricula, or in reforming policy formation methodology, Poincaré's definition seems to capture the essence of policy studies.

Second, and perhaps more debatable, policy studies seem substantively oriented to dealing with: (1) the problems of industrially superfluous populations, (2) the management of institutions designed to serve and control those populations, and (3) designing and justifying giving aid to the concrete individual, even as the category defining them as problematic is reproduced within the context of capitalist social relations. While this last comment may seem somewhat jaundiced, it nonetheless underscores a growing 'double-bind' many policy researchers and those administering programs flowing from policy studies are forced to consider. Indeed, several essays in this collection make reference to the shifting political and institutional contexts and the problems arising from these shifting, external contingencies. In fact, many of the substantive difficulties confronted in these essays have to do with the dessication of funding sources and shifts in the basis and rationale for funding some programs and discontinuing others. Then there are the intra-organizational problems of the lack of fit between the local and individual needs of clients, on the one hand, and globally formulated or fixed planning protocols employing Procrustean evaluation standards, on the other. As we have seen in Darwin's and Blackman's essays, there are often attempts on the part of policy researchers to seek solutions to these problems by streamlining or reforming the bureaucracies administering such programs. Yet, there is just so much one can do towards improving interpersonal communication and group problem solving techniques - i.e. reshaping internal administrative mechanisms to approximate the metaphorical anti-authoritarian complexity of Darwin's "Rainforest," - before it becomes evident that the problems are exogenous to organizational style.

An even more instructive irony from the point of view of the social scientists is the way in which these 'substantive problems' of policy formation and implementation suddenly transmute or can be

translated into the serene issues of scientific method, theoretical readings, or even social ontologies - and vice versa. This is the subject of Haynes' methodological essay, "Complexity, Quantification and Research for the Management of Policy." There he shows how the ontological characteristics of nonlinear social systems often disallow the application of a wide range of operational measurements and multivariate testing and modeling. With equal force he moves on to policy formation considerations and suggests that these methodological conundrums can form the foundation for a critique of policy that is often administered from the top-down.

This is where the question, 'Does Complexity Science have anything to offer Policy Studies?', comes into play. The answer to this question is, to put it colloquially, a 'no-brainer.' Who can deny that society is and always has been an unstable, complex constellation of elements, and that it has always exhibited the attributes of a far-from-equilibrium system. Those complexly structured attributes have been consistently discussed at varying length in the essays by Medd, Byrne and Blackman. Taken together, these three articles are judicious enough, and, when taken together, exhaustive enough, that they show how complexity theory might well enrich social policy inquiry in particular and the social sciences as a whole. Byrne, for example, shows how a heuristic use of strange attractor icons such as the torus or the Lorenz Butterfly can help redefine or otherwise clarify a policy problem such as the resurrection of a failing school or tracing the shift in the structure and functioning of the modern welfare state as its Keynesian/Fordist class compromises give way to flexible accumulation and its neo-Liberal state. Medd, takes Byrne's argument one step further by exploring the "operational pitfalls" of doing complexity research. Underscoring the fact that metaphors and models are linguistic phenomena designed to give heuristic insights and understanding, and are not to be confused with the reality being modelled, he demonstrates the stratified reality of complex social phenomena, and exposes the role which the researcher's interests can play in constructing the object of complexity research. Hence, Medd's closely argued paper examines the bracketing of three welfare episodes and shows how these "randomly selected" empirical episodes must be qualified in terms of the intricate relationship between concrete observations, their selective extraction from the totality, and their final subsumption under a larger complexity framework that seeks to holistically describe a selected set of policy episodes. Finally, Blackman suggests how complexity theory and the conjugate concept of "fitness landscapes," and the social parameters that determine the relative ruggedness or permissiveness of such social landscapes, might be used to understand the concrete flows and cascading shifts that have recently marked such areas as higher education funding policies and neighbourhood efflorescence and decline. What is laudable in all three instances is the restraint demonstrated by the authors in applying complexity theory to their respective subject matters. In each case, the metaphors are judiciously applied and made to fit the empirical contours of the materials at hand, rather than falling into the kind of Procrustean scientism that has too often marred previous applications of Complexity Theory to the social sciences.

And, in that social policy pertains to the latest manifestations of Monopoly Capitalism's fundamental social contradictions, it must sooner or later embrace ideas of nonlinear change, sensitive dependence on initial conditions, historically contingent cycles of iterative reproduction, and should be given the latitude to employ such metaphors as 'strange attractors' in modeling local and global movements.

Perhaps even more important, though, the articles collected here have shown the need for those involved in policy studies to begin outlining in a non-Narcissistic way a *reflexive conception of complexity* in which the policy sciences themselves are treated as being an integral part of the unfolding manifold of objective, nonlinear feedbacks that mark the dynamical reproduction of modern capitalist systems, *per se*. After all, social policy studies clearly form part of the very objective complexity they are trying to grasp [\[2\]](#). Indeed, such a reflexive theory of social policy formation and *praxis* might well begin with a metaphorical tracing of its own contingent history, designating the evolving historical parameters and the putative bifurcation points by which the phase shifts of policy studies evolved.

In fine, I believe the case can be easily made that social policy studies, along with all genuine social sciences, have an integral affinity for complexity theory. But this having been said, there are issues raised in these essays concerning the nature of Complexity Science, complex systems, and the

metaphors guiding our understanding of both, that remain unresolved. Let me end my commentary by listing some of the pressing issues in the form of questions:

1. *When do attributions of putative institutional self-organization lapse into reified understanding?* As long as the metaphorical use of the concept of self-organization is restricted to referring to the 'spontaneous' capacity of matter to evolve into new hierarchical and qualitatively new *natural forms or hierarchical constellations*, it is both a provocative and productive metaphor for bridging the classical Cartesian dualism of an active cogito confronting an essentially lifeless, mechanical natural world. When transported to the social sciences, however, the idea of groups and institutions being 'spontaneously organized' is a half-truth that leaves the social sciences vulnerable to charges of reifying the world in a manner against which Lukacs (1971) warned. Such reification directs attention away from the fact that institutional reproduction and operation is governed not only by the historical logic of institutional formations, they are equally the product of human agency - i.e., of human conduct that is characterized by abstract self-reflection, intentionality, and historically evolving modes of reproduction. Any use of the term 'self-organization' must thus retain the dialectical character Bhaskar (1994, 89-114; [1979]/1989, 25-64) maps out in his discussion of human agency. That is, we must never lose sight of the fact that human actors are both socially-determined, productions of historically situated social structures, on the one hand, and productive free agents whose very complexity hinges on internally-generated sources of unpredictable conduct. To the extent that CAS theorists claim to have metaphorically modelled some form of 'agency-based' conduct, they (and we) must be careful to specify which of these two moments of 'agency' is being discussed.

2. *What aspects of predictable knowledge become problematic in Complexity Science?* Do the protocols of Complexity Science deny the possibility of all forms of prediction that work from a knowledge of a system's initial conditions to its present state, or does this proviso discourage only long-term forms of prediction in which the parameters of the system and the phase space in which it evolves are themselves being so rapidly transformed, being constantly added to or subtracted from, that linear models of prediction fail?

3. *Can the history of human societies and the characteristics of human agency be fully grasped using the general model of Complex Adaptive Systems?* I have already touched on this issue more than once in this essay, but the question still remains, 'Is there room in CAS models, as presently conceived, for the evolutionary leap to human agency to be made, as well as the all-too-human tendency of many of us to act or change our conduct for reasons not immediately related to environmental learning and environmental fitness (these latter seem to be the alpha and omega of CAS modeling)? Alternatively, is CAS modeling capable of reconstructing an agent's continual productive incursions into its environment, using exploitive actions and fresh gambits to alter the very objects impinging upon its world and ostensibly directing its 'learning?' Or, are CAS modelers constructing metaphors of agency that assume a largely passive actor - i.e., a mere recipient and captive of whatever information the environment 'chooses' to present? It seems to me there is a vast difference between behavior that is capable of conforming to and of manipulating rules (Kant's schema), and the conduct of complex human agents who, on the spur of the moment, produce new rules (Kant's maxims) and choose to apply them or not to apply them as they choose.

4. *Must we still differentiate between random behavior and chaotic behaviour?* There is a tendency among many in the Complexity Sciences these days to equate deterministic chaos and 'randomness.' What is lost when the two are conflated? Formally, is there a difference between, say, a logistic equation's tendency, under sufficiently high control parameters, to fill the entire phase space, and that of a strange attractor to fill a strictly delimited volume of dense points? The answer to such questions involves more than mere nominalist conventions. In the social sciences this may be a crucial distinction. For example take the case of deviance: Is personal deviance purely willy-nilly, random behavior, or can it be seen as unfolding in a complex, dynamic system of interrelated trajectories traced out in a normatively defined and delimited phase space? The latter description might well describe Robert K. Merton's ([1949]/1968, 185-248) theory of anomic deviance. In that theory, you will recall, the various 'anomic adaptations,' are seen as action-induced reproductive errors of conforming norms which, under recursive reproduction over time leads to a well-ordered, socially-supported *deviant career* - i.e., one that carries the actor into untested, shadowy sectors of normative phase space. Indeed, taking this metaphor a step further, one of those careers - *rebellion* - entails an actual restructuring and possible evolution of the phase space itself, establishing new

normative protocols of conformity and deviance and the restructuring of cultural basins of attractions themselves. And, in a 'twice-removed' metaphor, if you will, Merton tells us that the 'control variable' that regulates the possibility and probability a person will drift into deviant career space is a ubiquitous class-based, unequal distribution of assets and accepted means for achieving success. In sum, is social deviance best grasped metaphorically as mere random reactance of psychologically-driven misfits and degenerates, or can it be seen as a complexly-structured social system whose trajectories have a common sociologically defined, referential attractor (conformity) from which and around which they periodically oscillate [\[3\]](#).

5. *Is so-called communication theory sufficient to grasp the ontology of complex social systems?*

Since Claude Levi-Strauss's ingenious attempt to use the premises of structural linguistics to reduce all social structures to forms of reciprocal exchange, there have been numerous attempts to reduce all social relations to symbolic communication. Such abstractions, in that they deny both history and processes based on man's productive transformation of nature, have been uniformly rejected in most quarters as abstract and one-sided. Perry Anderson (1984) has given us the canonical rebuttal to such structuralist and post-structuralist pretensions when he writes:

'Kinship cannot be compared to language as a system of symbolic communication in which women and words are respectively 'exchanged,'... Still less does the terminology of 'exchange' warrant an elision to the economy: if speakers and families in most societies may be reckoned to have at least a rough equivalence of words and women between them, this is notoriously not true of goods. No economy, in other words, can be primarily defined in terms of exchange at all: production and property are always prior. Levi- Strauss's trinitarian formula operates in effect to screen out all the relations of power, exploitation and inequality which inhere not only in most primitive economies, let alone our civilization of capital, but also in every familial or sexual order known to us, in which conjugality is tied to property and femininity to subalternity.' (Anderson 1984, 43)

Given Anderson's hammer-like rebuttal and his materialist insistence, it would seem a dubious venture to abandon productive social ontologies when constructing a complexity paradigm of social systems, and to substitute, instead, communication metaphors. Similarly, one must ask what is lost in our understanding of modern social policy studies when communicative theories of social complexity are used to explore their current predicament in Late Capitalist social formations.

These questions, skewed as they are and intentionally stated to be provocative, are not offered up as dogma as much as they are meant to be a way of furthering the questioning process that these contributors to this special issue have so ably set in motion. Still, these issues should be addressed, positions accepted or rejected as the case might be, but in all instances they must be explicitly addressed as those social scientists engaged in policy science research go about settling accounts with Complexity Theory.

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Notes

[1]The fact that such a myopic error as occurred in the Social Text and Catastrophe Theory is an everyday problem for all of us is demonstrated in a passage in John Hollands *Hidden Order: How Adaptation Builds Complexity*. Holland, a model builder par excellence, and a member of the core group at the Santa Fe Institute developing the CAS paradigm, in writing of 'fitness landscapes' in economic systems has written:

"Few models exist that exhibit this implicit approach to fitness, even in quite simple situations. There is more of a mystery to the origin of the pin factory that Adam Smith (1776) discusses in his *Wealth of Nations* than is generally realized. This factory was one of the first examples of a production line: one craftsman drew the wire, another clipped it to size, another sharpened the point, and so on. The result was a tenfold increase in production over the efforts of the same number of craftsmen working individually. Smith and later commentators discuss relevant factors : specialization, more efficient learning, mass purchasing, and so on. But we do not have any models that demonstrate the transition that enables individual skilled craftsmen to organize into a factory. What actions and interactions between these individual agents produced an organized aggregate that persisted? What were the adaptive mechanisms that favored the emergence of this aggregate? It makes little sense, and it helps our understanding not at all, to assign a high a priori fitness to the pin factory. That fitness must emerge from the context" (Holland 1995, 97-98).

It would seem that much of the mystery over the a priori fitness or lack of fitness of the pin factory and the actions and interactions that 'enabled individual skilled craftsmen to organize into a factory' could be cleared up by consulting Parts IV and VIII of Capital, Volume I (312- 507; 713-774).

[2] In his own way, George Soros (1998, [1987]/1994) has used the subject/object dialectic to formulate, in albeit somewhat halting terms, a theory of complexity that is grounded in the interaction of the subjects self-reflexive evaluation and anticipated future states of the economy.

[3] If one is not a Mertonian (of which I am not), an even richer set of metaphors awaits those who see society as an ecology of class-based or ethnic subcultures struggling to gain hegemonic mastery over one another, or, perhaps, only seeking their own uncontested niche in a larger hegemonic order.