Socio-Economic Review (2008) **6**, 733–770
Advance Access publication August 13, 2008
doi:10.1093/ser/mwn015

DISCUSSION FORUM

On Rogers Hollingsworth and Karl H. Müller 'Transforming socio-economics with a new epistemology', *Socio-Economic Review*, **6** (2008), 395–426

Keywords: socio-economics, epistemology, evolution, complex networks, self-organization, power law, interdisciplinarity, multi-level analysis
JEL classification: D8 information, knowledge, uncertainty, D85 network formation and analysis, theory

The quest for theoretical foundations of socio-economics: epistemology, methodology or ontology?

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1. Introduction

The article under review has two sources of inspiration, one negative and one positive. On the negative side, the authors express their dissatisfaction with the current state of the socio-economic community with an implicit reference to the Society for the Advancement of Socio-Economics (SASE). Socio-economic research is characterized by strong empirical and comparative studies as well as a relentless criticism of the neoclassical theory but no relevant theorizing (395–396). On the positive side, Hollingsworth and Müller perceive a window of opportunity with the emergence of a new scientific paradigm that is able to transcend the divisions between the natural and the social sciences.

The reference to epistemology is clear evidence of the programmatic and ambitious objective of the wide survey of many disciplines undertaken in the article. The authors should be praised for daring to take on such an ambitious project and submitting it for the assessment and critique of the SASE community. Nevertheless, from a methodological point of view, it would have been interesting to discuss what the obstacles have been on the road towards theorizing socio-economics. Are they related to the absence of cumulativeness of the international comparisons and case studies? Did not neoclassical thought export its tools to quite all branches of social science at the very moment they were challenged within the economic profession? The diversity and occasional antagonism of alternative approaches have not promoted the emergence of a new paradigm in socio-economics. Furthermore, the lack of any canonical method that would unify the various strands of research has not favoured the formation of a strong research agenda among the SASE community.

This is precisely the strength of the proposition which calls for some unifying tools and possibly a general theory of socio-economics. It is thus a timely and relevant initiative, as will be discussed in Section 2. But the proposition can be interpreted along two different lines: either as re-foundation of socio-economics by the conjunction of five tools/concepts or as an implicit restriction to network analysis and its analytical consequences (see Section 3). Another ambiguity concerns the use and abuse of the notion of complex systems, since this can create quite contrasting visions and formalizations of the evolution of social systems (Section 4). At a quite abstract level, one may challenge the assumption that the breakthrough derives from a new epistemology, since the authors propose mainly to adopt the methodology and the tools that have been imported from statistical physics (Section 5). Lucidly, they perceive a major problem in transferring theoretical models across fields, but they adopt a quite optimistic and unwarranted stance: transfer is possible even when there is no strict isomorphism (Section 6). This leads to two final comments. Is it not paradoxical to try to build socio-economics without any clear definition of the phenomenon it is supposed to explain or its specific domain (Section 7)? Why not start from a lucid appraisal of the achievements and limits of past and various research programmes in socio-economics and try to look for the relevant tools generated by the current evolution of scientific methods (Section 8)?

2. A relevant and timely question

In their article, Hollingsworth and Müller argue that the present period is an unprecedentedly good opportunity for a new alliance between the natural and social sciences. Actually, research programmes have changed significantly during the last two decades.

Basically, the emergence of political economy and its transformation into modern economic analysis is somehow influenced by leading conceptions of scientific inquiry and tools. Thus, if science evades the hegemony of the Cartesian–Newtonian perspective in the direction of an evolutionary approach in terms of complex dynamic systems, this should have an impact upon socio-economics. One may challenge this mechanical vision, but it is a stimulating starting point from which the proposition for a new socio-economics can be supported by arguments that are less grand:

- First, the last two decades have experienced a paradoxical evolution. On the one hand, the standard paradigm of economic theory (rationality, representative agents, market equilibrium and rational expectations) has exhibited *decreasing returns* in the very domain of economic analysis. On the other hand, this paradigm has been exported successfully to other social disciplines (sociology, political science, law, history, etc.). Only recently have social scientists perceived the charms and limitations of this importation of the concepts and methodology of economists.
- Secondly, within the economic profession, creative individuals and innovators have searched for *alternative approaches* built upon the concern for bounded rationality, the major role of agent heterogeneity in the resilience of dynamic systems and the diversity of coordinating mechanisms other than typical market adjustments. The Santa Fe Institute is one of the melting pots where physicists and social scientists have gathered in order to develop new tools for understanding complex interactions among large populations.
- Thirdly, in the past, the concept of optimization and the search for optimality, which are at the core of neoclassical theory, are used to imply the convergence of socio-economic configurations towards the equivalent of one best way. Nowadays, *the diversity* of organizations, institutional configurations and of course capitalisms calls for alternative paradigms that could explain such variety. This is one of the recurring themes of past socio-economic research, and such variety might be explained by the models put forward by Hollingsworth and Müller.
- Fourthly, the notion of equilibrium makes it difficult to account for the evolutions observed since the breaking down of the Golden Age. It is, for example, difficult to explain the shift from full employment and rapid growth to mass unemployment and slow growth with the hypothesis of a continuous evolution of the same macroeconomic equilibrium, submitted to a series of marginal shocks. In contrast, *dynamic systems* with significant non-linear aspects may present the possibility that a marginal shock may trigger the shift from one equilibrium to another quite distinct one.

- Fifthly, various innovative studies on the relative importance of strong versus weak ties, the diffusion of social norms and the impact of mimetism on financial markets have triggered new formalizations that do share some *common features*: the importance of horizontal interactions among heterogeneous agents, strong sensitivity to small variations in the initial conditions, and evolutions out of equilibria. Actually, the related models share common patterns with the formalization elaborated by physicists.

In a sense, the two authors have perceived this epochal change and interpreted it as a paradigmatic shift from Science I to Science II. But what is the precise meaning of this aggiornamento for socio-economics?

3. Re-interpreting the proposition: networks as preferential attachment or a more general and integrated theory?

The article under review is structured along the presentation of five concepts or methods, labelled as self-organizing processes, complex networks, power-law distributions, the general binding problem and multilevel analysis. Actually, a careful reading suggests that these scattered notions belong to two different research programmes, even if one could imagine bridging them (Figure 1).

- The bulk of the proposal focuses upon a central tool: *network analysis*. The merit of this approach is the ability to cross a large spectrum of physical, natural and social phenomena and to build upon many of empirical investigations which analyse various interactions among entities. The now well known 'small world models' show that the connectivity of many social

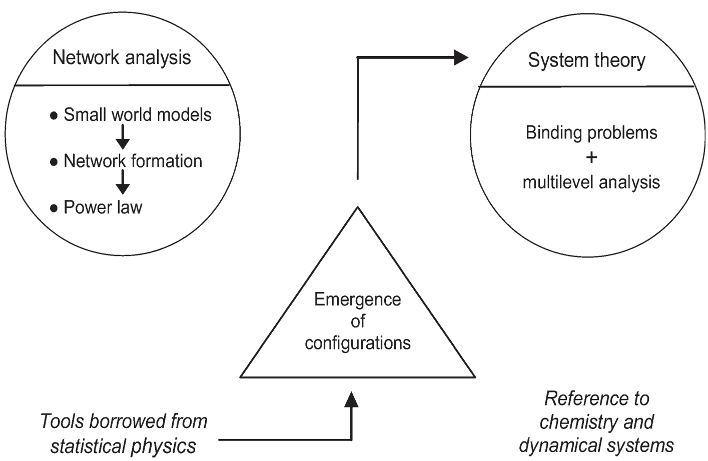

Figure 1 Not one but two research programs.

networks is quite high compared with what one would expect from a purely stochastic network. The power-law distributions capture the originality of these networks, in contrast with the conventional normal distribution.

- But the two other concepts, i.e. the *binding problem* and *multilevel analysis*, do not belong to the same paradigm. The former tries to explain the cohesiveness of two entities, with an explicit reference to the formation of molecules by the strengths of interactions among atoms. It is quite difficult to extend such mechanisms, even if metaphorically, to the issue of institutional complementarity or isomorphism between organizations and institutions. These are two themes of very active research programs in contemporary socio-economics. Similarly, multilevel analysis in socio-economics is very different from the methods of natural sciences that construct the atom from elementary particles, the molecule from component atoms, the cell from the mix of specific molecules, the organ from the interactions of specialized cells, and so on. When economists or political scientists study the contemporary world, they want to understand the mutual interactions between the local, regional, national, continental and international levels, and the genesis of these social constructs do not follow the same patterns or logics as atoms and molecules. This is the second research agenda proposed, but the novelty of the strategy is far less convincing.

Of course, the models borrowed from statistical physics do provide examples of the emergence of macro configurations out of the interactions of entities at the micro level, and they can be extended to the diffusion of innovations, social norms, and behaviour on financial markets (Table 1).

Nevertheless, there is no equivalent to the rich and complex interactions observed in contemporary socio-economic configurations, especially the *embeddedness* of some orders into other ones, or even more the *nestedness* of various orders with bottom-up as well as top-down influences among different entities.

Table 1 Macro regularities as emergent properties of heterogeneous micro behaviours

Micro behaviour	Emerging aggregate pattern	Theory
Rational choice	Market equilibrium	General equilibrium theory
Imitation/innovation	Growth regime	Evolutionary/Schumpeterian models
Mimetism	Polarization of bear and bull markets	Behavioural finance

Thus, the proposition is far from coherent, since it juxtaposes a rather well defined framework – network analysis – with a quite loose approach to the cohesiveness of socio-economic systems.

4. Simple mechanisms, complex evolutions: away from the conventional concept of complexity

Basically, network analysis might help socio-economists in detecting and analysing the diversity of social, institutional and economic configurations. But a second area of research aims at accounting for the processes of change that affect these configurations. The introduction of the time dimension calls for a different approach. This is probably why Hollingsworth and Müller refer so intensively to *complex systems* theory. Their presentation gives the reader the impression that complex systems are so complicated that they challenge conventional scientific methods and that complex dynamics can only be the result of highly interdependent, rich and heterogeneous systems. These two intuitions might be misleading: is not the very purpose of scientific analysis to explain with parsimonious hypotheses the largest possible number of phenomena?

- First, very *simple determinist systems* with homogeneous agents usually generate monotonous evolutions at the macro level, but mathematical economists have shown that the conjunction of a series of rational individual behaviours may generate *chaotic properties* at the macro level. Conversely, *highly complex systems* may generate *monotonous evolutions*, even if the probability of bifurcations and chaotic evolutions becomes higher in these systems than in simple systems (Table 2).
- Secondly, the authors attribute a major role to the opposition between *deterministic* and *stochastic* models. But it would be erroneous to think that only

Table 2 Do not confuse the complexity of systems and the complexity of their dynamics

	System			
	Simple		Complex	
Dynamic	Homogenous agents	Heterogeneous agents	Several sub-systems	Numerous and nested systems
Monotonous	Typical	Possible (law of large numbers)	Possible	Problematic
Bifurcations	Rare	Typical	Likely	Likely
Chaotic	Rare	Possible	Likely	Likely

complex stochastic systems display complex dynamics. It has been proved that very simple non-linear dynamic models can easily generate complex dynamics such as bifurcation, chaotic behaviour and the existence of limit cycles (Table 3). Furthermore, when individual behaviours are not influenced horizontally by one another, their heterogeneity and the fact that they might be partially stochastic may quite on the contrary generate simple dynamics or distributions at the macro level. Therefore, complex dynamics are not necessarily the norm, and they may or may not imply stochastic elements on top of determinism.

Consequently, social scientists should use with care the concepts of complex systems, the meaning of which might differ between physical and social systems. The everyday meaning of complexity should not be confused with the analytical one, which is much more restricted and difficult to implement rigorously in socio-economics.

5. Visions, theories, methods and models

This is an invitation to assess the epistemological status of 'Transforming socio-economics with a new epistemology'. Epistemology is usually defined as the critical analysis of the origin, logic, value and consequences of scientific activity. Historically, philosophers, impressed by the scientific advances they were observing, have tried to delineate what it is at the core of science and how it distinguishes itself from other human reflexive activities. So doing, they do not pretend to be active in the field of scientific research itself, because they rarely possess the dual competences of the philosopher and the scientist in the related domain. Thus, Hollingsworth and Müller propose a quite new and daring strategy to redesign socio-economics: they detect a general scientific revolution; they perceive its emergence in various social sciences, and they intend to speed up the use of the related concepts in the domain of socio-economic analysis. But

Table 3 Complex dynamics may occur in determinist as well as stochastic models

	Model	
Dynamic	**Simple (a few simple determinist equations)**	**Complex (numerous stochastic equations)**
Simple	Conventional case	Possible if law of large numbers with heterogeneity (e.g. law of aggregate demand)
Complex	Possible with non-linear relation	Typical of this kind of system

a cursory retrospective of scientific evolution (for instance in the domain of political economy) suggests that the actors themselves recurrently find new domains, concepts, methods and results—and afterwards the related community redefines its implicit epistemology, quite apart and independently from the ex-post appraisal of the philosophers who study the history of their science. The authors' wager is that this sequence is not the only one available and that a form of *epistemological reflexivity* is able to promote an intellectual aggiornamento among the socio-economist community.

But the title of the article is probably misleading, since it actually suggests, more modestly, a new *vision* and various *tools* in order to formalize some of the *processes* that are currently investigated by socio-economists (Figure 2). The *vision*, i.e. what the authors call cognitive structure, focuses upon complex systems, be they social networks or multilevel configurations; but this should not be confused with a fully fledged *theory* that would derive from a few basic principles or axioms a series of falsifiable propositions in various domains of socio-economic research. *De facto* the authors put forward a tool kit, i.e. 'new methods, models, concepts and other tools in what they labelled as a Science II perspective' (p. 399). Does it help in building a socio-economic theory? Maybe if, and only if, the entities, problems and domains of this discipline are defined with precision. Unfortunately, the authors only give examples and mention specific models, and never with the generality required for a founding epistemological breakthrough.

Pragmatically, the article proposes instead how some class of models inspired by statistical physics, atomic chemistry or biology may help in formalizing some problems typical of socio-economics. By the way, it is not clear that 'requirements

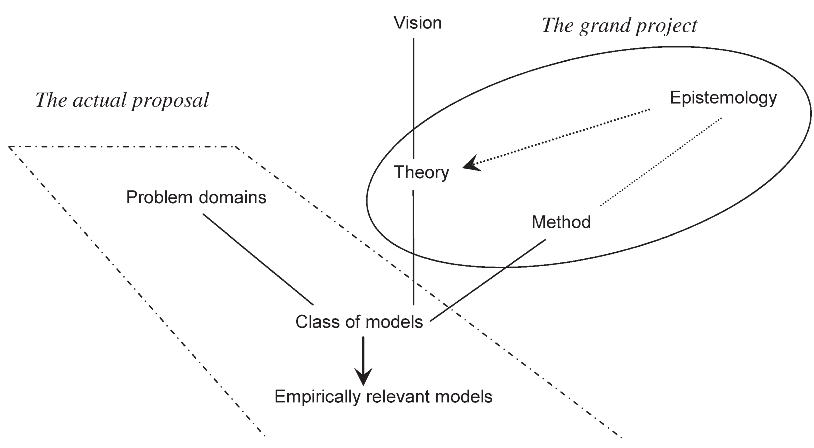

Figure 2 Clarifying the concepts and levels of socio-economics.

for scientific explanation are much weaker than with the Descartes–Newtonian paradigm' (p. 399). In 'small world models', for instance, the predictions derived from repeated simulations are confronted with the observations of the underlying physical or social systems, in conformity with the purpose of Science I. It might be misleading to let the reader think that the results are as fuzzy as those derived from qualitative/comparative analyses in social science.

Just as building a crane with Mecano or Lego components is not a contribution to mechanics as a physical theory, importing small world models does not provide evidence for a general advancement in socio-economics theorizing, however useful and stimulating this cross-fertilization might be. Thus, the proposition is not interesting as an epistemological or theoretical breakthrough since it simply points out a class of models that are common across various domains and scientific disciplines.

6. Homology across various social areas, but not an omnipotent and universal model

Hollingsworth and Müller are conscious of the difficulty just mentioned. Their concluding observations recognize that complex networks 'are common to socio-economists and many colleagues in the social and natural sciences' (p. 417). But sharing models does not necessarily mean transferring theories from one discipline to another. The authors then try to respond to Renate Mayntz's statement: 'Theory transfer in a strict sense presupposes—and assumes—*isomorphism* between the empirical phenomena to be described and explained' (1992, p. 30; my emphasis). In the light of the most recent developments in socio-economics, Hollingsworth and Müller argue that 'there is high potential for transferring *theoretical models* about complex networks across fields, even when there is no strict isomorphism among the empirical phenomena to be explained' (p. 417; emphasis in the original).

In this respect, the present author is on the side of Renate Mayntz. Not only is she right in theory, but the very models mobilized by Hollingsworth and Müller exhibit the same basic abstract mechanisms: the behaviour of an individual is positively correlated with those of the other individuals belonging to the same group. Within network analysis, this is equivalent to *preferential attachment*. In the choice of technologies this is called increasing returns to adoption. In behavioural finance, uncertainty favours mimetism. In scientific communities, reputation effects generate power-law distribution (Table 4). Actually, all these problems belong to the same structural model: the probability of adopting a given strategy increases with the observed frequency of current strategies (Figure 3). Of course, the parameters of the model vary from one domain to the next, but the same structure is present.

Table 4 The same mechanisms across various domains and social sciences: an example

The same structure		
Hypothesis	**Mechanisms**	**Outcome for configuration**
1. A standard/technique is adopted according to the number of users	Non-ergodicity, irreversibility	An inferior technology may persist due to the replication of the same technology
2. Learning by doing	Path dependency	Domination of one technology
3. Mimetism in response to uncertainty in financial markets	Permanent deviation from fundamental value	Succession of bear and bull markets
4. Existence of social norms	Diffusion of new practices and behaviors	Diversity of social configurations
5. Quotation of scientific papers	Reputation effects	Power law distribution

Two important conclusions are derived from this finding:

- The transposition of any theoretical model from one domain to another, from one discipline to another, is justified if and only if there is *a strict homology*, i.e. a one-to-one correspondence between elements and relationships.
- Consequently, the convergence of *empirically relevant models* has to be checked carefully case by case. *A priori* this cannot be an avenue for a general re-foundation of socio-economics, since these models cover only a limited number of the phenomena it is supposed to explain.

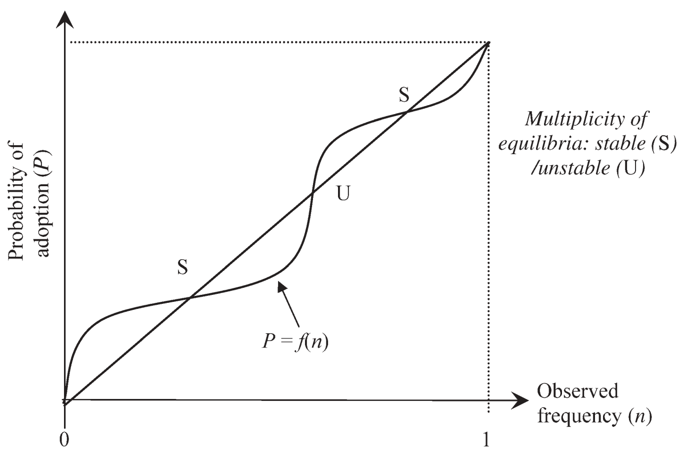

Figure 3 The general model for the examples in Table 4.

In other words, enlightening *metaphors* should not be confused with socio-economic *theorizing*. Furthermore, the contribution of Hollingsworth and Müller concerns more *methodology* that *epistemology*.

7. A black hole: definition and basic concepts of socio-economics

The proposition suffers from a strange understatement: what are the basic phenomena that socio-economics should analyse and how do they relate to complex network analysis? Paradoxically, the reader has to wait until the section on the general binding problem (Section 4.4, pp. 410–414) for a discussion of the *entities* that socio-economics is supposed to analyse. Generally speaking, it is the issue of *governance structures*, but they are quite diverse: markets, hierarchies, networks, associations, State, communities, clans (p. 413).

On this issue, the authors face a dilemma:

- Either they consider that the analysis of complex interactions, typical of networks, can also be applied to *all other governance structures*/coordinating mechanisms (For instance, one could consider that networking is at the origin of market formation, as well as State organization. But this opens a huge and uncertain research agenda since one may doubt that networks are such an overarching mechanism.)
- Or, networks are only one of the governance structures and thus an equivalent breakthrough has to be found for markets, hierarchies, State, etc.

If the latter is the case, Hollingsworth and Müller would only be proposing a sub-theory for socio-economics, far from the flamboyant title of their contribution.

Similarly, it seems quite difficult to accept that the only mechanism governing the relations between actors is basically mimetic, since this is the specific basis only for the building of networks by preferential attachment. What about the role of *competition*, the basic mechanism of the various branches of neoclassical economic theory? What about the impact of *trust* upon the viability of associations and the legitimacy of State? Has not the concept of *power* a role to play in network analysis, State formation or even the everyday operation of the firm? How do moral sentiments shape economic behaviour, and how do conceptions of justice inform the various socio-economic configurations?

At a more conceptual level, each discipline defines itself by a *domain of investigation*, which may evolve through time since disciplines tend to become more and more specialized. For instance, sociology aims at explaining society through social factors. Economics assumes that economic activity is generated by the specific rationality of homo oeconomicus. What is the purpose of socio-economics? Initially, it emerged out of the efforts to explain economic phenomena that could not be captured by typical neoclassical economic theory. Could moral foundations fulfil such an objective, or should explanations be extended to the impact of social and symbolic representations upon economic behaviour?

Thus, socio-economics could be presented as the *search for the social determinants of economic activities*, but it would then be in competition with a series of other paradigms.

- Some *mainstream economists* recognize the role of horizontal social relations, and they introduce them into their formalizations of economic behaviour, away from a pure utilitarian approach.
- *Economic sociology* is a quite active field of research that investigates network formation, i.e. the very topic privileged by the new epistemology proposed for socio-economics. But it also studies the role of State in the economy and the functioning of some specific markets, including financial ones.
- '*Culture matters*' is the motto of another community of research that also investigates the discrepancy between the behaviour of homo oeconomicus and the fact that individuals act and make decisions in conformity with their socialization in a given culture.
- The next phase of *experimental economics* may well overcome its present individualistic bias and explain how the social environment affects behaviours and strategies.

Quite difficult issues indeed! Nevertheless, any suggested transformation of socio-economics should address how it fits within the rather competitive arena of neighbouring social sciences and their current redeployment.

In addition to new *methodologies*, socio-economics needs to define itself in order to exist and thrive. It requires an *ontology*.

8. Survey, convert, recombine and hybridize current research programmes

For the sake of simplicity, let us assume that socio-economics is about the investigation of the origin, transformation and impact of *governance structures* in modern societies. The article by Hollingsworth and Müller mentions some achievements of socio-economic research in this respect, but only marginally. It does not try at all to link them with the 'new epistemology', probably because the proposal is driven by the methodology of complex network analysis; but this is only one of the many coordinating mechanisms that sustain contemporary economies.

Therefore, let us risk an *alternative research agenda* that would build upon the rich but scattered results of two decades of investigations.

- First, what have we learnt about the requisites, functioning and evolution of the diverse *institutional arrangements* that organize social, economic and political relations and exchanges?
- Secondly, such a *survey* should be directed towards a generally agreed upon *taxonomy*, and it could be the starting point of a tentative *axiomatization* of the logic and functioning of these institutional arrangements.
- Thirdly, this could be the starting point of *formalizations* and a class of applied models that could be tailored to various past and future case studies, while preserving the cumulativeness of the improvement of the tools thus generated.
- Fourthly, socio-economists should dare to face a daunting question. The strength—and simultaneously the limit—of neoclassical theory has been to provide a vision, and then a theory, showing under which conditions there exists an equilibrium for an economy that should only be coordinated by markets. For socio-economists, the equivalent task is to investigate what the mix of institutional arrangements that deliver *viable governance structures* at the macro level looks like.
- Fifthly, one should not forget that some of the building blocks of such a research agenda already exist. Is not isomorphism a possible mechanism? The *Comparative Institutional Analysis*, along with various tests of the *Institutional Complementarity Hypothesis*, have delivered interesting insights and elaborated some formalizations that fit with the very purpose of socio-economics.
- Sixthly, socio-economists should not study only institutional equilibria. Understanding the transformations of governance structures in *historical time* should be a specificity of socio-economics, building upon the hypotheses and mechanisms diagnosed by *Comparative Historical Institutional Analysis*: institutional arrangements are converted, layered and recombined. Thus, a series of piecemeal adjustments may trigger a complete transformation of the past socio-economic governance.
- Seventhly, all the previous propositions aim at renewing socio-economics as a scientific, positive theorizing. Nevertheless, researchers should think about one of the reasons for the success of neoclassical theory: besides a form of conceptual precision and sophisticated methods, it provides a *normative vision*: a 'good' economy allocates resources efficiently. This becomes quite crucial when the economist becomes an expert and is, for instance, an advisor of governments. Implicitly, what is a *good economy/society* for socio-economists?

9. Conclusion

Epistemology suggests that disciplines evolve under the pressure of two major forces. On the one side, *internally*, they have to solve the new problems generated by their own development and/or to respond to the anomalies that cannot be interpreted within the existing paradigm. On the other side, *externally*, some leading disciplines may experience paradigm changes and thus exert some influence on the redesign of other disciplines, for instance, in providing new visions, original tools and classes of models.

Hollingsworth and Müller argue that socio-economics should take this second route and that it should thus benefit from the cross-fertilization of theoretical models that recently emerged from advances in physical, biological and social science. They point out the potential of *complex network analysis*, since this research has already shown its relevance for socio-economics. But they overestimate its impact as a (if not *the*) major source of renewal and dynamization of socio-economic research. Theories cannot be imported from one discipline to another; only specific classes of models can. But doing so does not necessarily contribute to building the theory that is relevant for a precise domain of investigation.

The scope of this discipline is far larger since it studies a *whole spectrum of governance structures*. Therefore, socio-economics should mainly follow the first route, i.e. it should build on its own legacy, clarify its specific domain and develop the more relevant methodologies given its objective: explaining the *diversity* of governance structures and describing the forces that govern their *evolution*.

A biological perspective on complex networks for a new epistemology

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Hollingsworth and Müller draw many interesting parallels between social and biological systems and argue that a new class of explanations for these systems is ushering in a new scientific epistemology. I would like to explore that similarity a bit further in order to assess its validity and its significance. As a practising biologist, I will bring some expertise to the natural science side of the putative equation, and as an interested spectator of the social sciences, I will try to point out relevant features.

Much attention has been directed towards the network nature of biological systems. Ecosystems and food webs have long been treated in this fashion, from a theoretical (e.g. May, 1973) as well as empirical standpoint. The multilayered and multicomponent character of these systems was obviously complex. The difficulty of comprehending complex networks in ecology, whether empirically or theoretically, as soon as the system tried to encompass more than a few players also became apparent early on.

Another early entrant to the complexity circle was metabolism, which was recognized to have network properties as the number and interconnectedness of its pathways proliferated. Theoretical characterization of metabolism borrowed extensively from and relied heavily on control theory from engineering (e.g. Fell, 1997). However, the influence that these pathways have on each other and the difficulty of encompassing ever larger networks with more and more components have strained the capacity of error computation and feedback to account for the system's behaviour.

With technological advances that have expanded the scope of multicomponent data collection, networks in the brain have also become amenable to study. That there were such networks was an undeniable observation of a century of neuroanatomy, but until the signals from many neurons could be recorded at once, their analysis was impossible. Now it has become routine to assume network complexity in any discussion of the brain. A widely held view in this realm is that the activity patterns and outputs of these networks can eventually be described mathematically and modelled after (as well as by) computers (e.g. Sejnowski *et al.*, 1988). This viewpoint is, however, by far not universally accepted (e.g. Edelman, 1989).

The most recent inductee to the network club is the gene network—a more abstract entity than those discussed previously, owing to the fact that its members do not interact directly and physically with each other. Their existence has been inferred from studies of gene regulation, which have identified cascades of genes whose products control the activity of other genes, and from measurements of the coordinated changes in activity levels of all of the genes in an organism's genome under varying conditions. Perhaps, because of the indirect nature of communication in gene networks, accounting for and modelling network behaviour has lagged behind other fields. There have been models based on computer logic here as well (e.g. Istrail *et al.*, 2007), as well as dissenters (e.g. Greenspan, 2001).

Similarities and dissimilarities

As Hollingsworth and Müller point out, there are many similarities between social and biological networks. These include heterogeneity of elements, many elements, extensive interconnectedness and degeneracy. The latter, degeneracy, may also be characterized as under-determination. This is not necessarily the same as randomness, and is perhaps better described as probabilistic. This is one among several properties that most defy being captured by control theory or computer logic (cf. Edelman, 1987).

Dissimilarities include the fact that the connections in social networks, even though they are direct, must pass through a conceptual phase on their way to being realized, in contrast to the physical/chemical connections in biological networks. (The physical/chemical communication in biology applies equally to gene networks, despite their indirectness, because the players that carry out the interactions are the protein products of the genes.) In other words, social networks are mediated by language and are therefore inescapably abstract (Edelman, 2006) in comparison to chemical and physical networks. The versatility of language allows for a nearly unlimited number of possible bases for network formation and configuration. Moreover, the requirement for physical interaction may have been a limitation at previous times in history, but now that we have a vast, expansive and interconnected virtual world in which to communicate, such limitations are no longer relevant. Even neurons in the brain are relatively limited in the number of contacts they make, as opposed to the number of potential contacts they could make. A further distinction between the social and the biological pertains to the stability with which each can be transmitted and reproduced. Social networks require cultural transmission, whereas biological networks (at least those that exist inside the organism) rest on the more reproducible and stable foundation of genetic transmission.

Do these differences make any appreciable difference for the claim by Hollingsworth and Müller that there is an essential similarity in the social and biological sciences underscored by the emergence of a new epistemology? The answer to this question depends on the extent to which the two sciences share fundamental, underlying properties.

Selection and complexity

An issue that comes up repeatedly in discussions of the fundamental properties of biological networks is whether they operate according to engineering models (i.e. feedback, if–then logic, and so on) or not. If so, then the way is clear to understanding them; all that is needed is adequate data and equations. Moreover, the success of these models may then point the way towards successful analysis of social networks. On the other hand, if engineering models are not up to the task, then the question arise: what is an appropriate model? This question has been dealt with most extensively and in the greatest depth in the realm of brain theory (Edelman, 1987), where the contrast is drawn between 'instructionist' and 'selectionist' frameworks.

Instructionism refers to the engineering perspective—specifically, the reliance on if—then computer logic. The selectionist viewpoint is based on Darwinian theory as originally developed to account for the evolution of new species. It was subsequently applied to the process of the generation of antigen-specific responses by T-cells and the antibody-producing B-cells of the immune system.

In both examples, the key criteria are a large population harbouring extensive diversity, criteria for singling out a subset of the population and preferential survival of this subset of the population. The unit of selection (i.e. the entity that is the object of selection) for species evolution is the individual, but the population of individuals is the relevant level on which the process of evolution occurs. The unit of selection for the immune system is the T- or B-cell. In the brain, the unit of selection is proposed to be neuronal 'groups' as opposed to individual neurons, which are best understood as the functionally relevant circuits that mediate a given response or task. Here, selection does not result in preferential survival of the actual cells, they all keep on living (at least for the time course of this phenomenon), but rather the persistence of that particular ensemble of neuronal activities that emerged from the repertoire of possible ensembles.

The most important distinction between these concepts is that instructionism requires an instructor, whereas selectionism does not and instead allows the response to emerge from the pool of potential responses based on the relevant, adaptive criteria. The process is not random, but rather is biased by the selectional criteria which, in turn, have been inherited. Selectional systems are particularly well adapted to responding to novel conditions, which are ever present in the biological world.

Selectional features may also apply to social networks. It is certainly hard to see how instructionism could. Direct analysis of such networks with these issues in mind will be required to confirm the similarities. Viewed from the outside, it seems reasonably likely that the common features of the two kinds of network reflect some common underlying mechanisms.

If a system is selectional, does that necessarily make it complex? In principle, the answer may be no. In practice, however, a selectional system must have a substantially large repertoire in order to be capable of degenerate solutions, and thus to work well (Edelman, 1987). It is probably safe to say that any large, heterogeneous, network-like system, rife with degenerate responses to any situation, is complex.

Is it a new epistemology?

The idea that a new scientific framework may be emerging that encompasses both social and biological systems is compelling. If this new framework is based on selectional, as opposed to instructional, principles, then it will certainly be novel.

In his 1925 Lowell Lectures, the philosopher Alfred North Whitehead traced the evolution of Western world views in terms of a sequence of different scientific paradigms (Whitehead, 1925). He might have been tickled at the idea that in the twenty-first century, we would embrace a pluralistic, non-instructional, incompletely deterministic philosophical perspective that originated with Darwin, if indeed selectionist complexity does actually become the dominant paradigm of our current century.

Funding

R.J.G. is the Dorothy and Lewis B. Cullman Fellow in Experimental Neurobiology at the Neurosciences Institute, which is supported by the Neurosciences Research Foundation.

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Networks and self-organization: dissecting the model of 'complex networks'

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More than 30 years after Todd LaPorte's valiant attempt to make social scientists think more pointedly about complexity (LaPorte, 1975), Hollingsworth and Müller take up the challenge for socio-economics, arguing that the conceptual models of what they call Science II should be used by socio-economists as a much needed alternative to the theoretical paradigm still dominating the field. Science II 'is primarily concerned with an effort to comprehend both the natural and the social world in terms of evolution and complex adaptive systems which tend to be self-organizing' (p. 397); self-organization is the dominant process in Science II (p. 398). These definitions suggest that Science II is closely related to complexity theory, a rather formal and at times heavily mathematical branch of systems theory. In complex systems, the interaction of system parts is governed by non-linear dynamics and involves phase changes, tipping, and bifurcation phenomena. Interaction effects are difficult, if not impossible to predict. The complex systems approach has been familiar in the analysis of ecological and meteorological systems, but the approach, if not the mathematics, is also used in sociology and political science, for instance in research on sustainable development, organizational management and large technical systems. However, though Hollingsworth and Müller emphasize complexity, non-linearity and unpredictability as hallmarks of Science II, they do not refer explicitly to complexity theory; in fact they appear to subsume its approach under the mechanistic Science I paradigm (see p. 397).

The concepts and theoretical models that the authors mention in their discussion of Science II refer partly to structural properties of systems (i.e. network, bonding, multi-level), partly to processes and process characteristics (i.e. self-organization, bifurcation, path dependence, evolution, emergence). These concepts do not form a coherent paradigm; they have in common that they have become popular more recently, and have been used in many different disciplines. Of course, the authors do not claim that the theoretical paradigm of Science II is coherent. But the heterogeneity of the elements assembled under this label does create a conceptual tension when the authors proceed to build an analytical model of complex networks from five of the concepts identified with Science II, offering it to socio-economics as a promising new approach. The five concepts—self-organization, complex networks, power-law distribution, binding and multi-level analysis—'intersect' with each other in the analysis of complex networks (p. 417): networks imply 'bonding', are presumably generated by self-organization, have a multi-level structure and produce law distributions. Choosing complex networks rather than complex systems—a much wider concept—as focus of an analytical frame, presumably offering a fruitful perspective for socio-economics, has the advantage of narrowing the task to manageable proportions.[1]

[1] All of the elements of the complex network perspective can be features of complex systems, and though complexity theory often works with system properties and their interdependencies expressed in terms of abstract variables, complex systems can also be represented in terms of system parts and concrete interactions. Vice versa, though basically networks as well as self-organization processes are based on relations and interactions between concrete parts, they, too, can be represented in the form of variables and their interdependencies.

Hollingsworth has been one of the first to call attention to the fact that not only hierarchies and markets, but also networks are a wide-spread and important social form in the economy. In contrast to the sophisticated methods of network analysis, network theory is still underdeveloped and could well gain if extended in the direction suggested by the authors, for instance, by paying more attention to bonding through preferential attachment, to the different ways in which networks develop and to their vertical differentiation. However, the claim that complex networks can serve as the basis of a new and fruitful perspective in socio-economics also raises a number of questions. Is this really a new perspective for socio-economics? To what extent is socio-economics still dominated by what the authors call the mechanistic perspective of Science I? Are complex networks as elaborated by the authors a characteristic feature of the economy? And finally, do the five presumably connected concepts singled out by the authors constitute a coherent paradigm? Being neither an economist nor a socio-economist, I shall only deal with the last question. My answer is, in brief, that the complex network framework as developed by the authors is rooted in two different perspectives, one connected with the concept network, the other with the concept self-organization and the paradigm of non-linear dynamics. These perspectives do not neatly fit together; there is tension between them, but this tension could be fruitful.

The concept of self-organization is crucial in the framework of non-linear dynamics; this framework is characterized by a syndrome of related concepts, including, in addition to self-organization, path-dependence, bifurcation, evolution, emergence and punctuated equilibrium. These concepts have already been stimulating social scientists for some time and are mentioned by Hollingsworth and Müller as hallmarks of Science II. There is no doubt that phenomena to which the concept of self-organization and the concepts related to it refer can be observed, and indeed have been observed in the economy. The concept of self-organization, introduced by the natural scientist/philosopher von Foerster (1960), refers to the spontaneous emergence of macro properties in systems containing a multiplicity of like parts (e.g. atoms, nerve cells, human beings) that behave autonomously, yet are interdependent in their behaviour. Ideal-typical market processes are an instance of self-organization in this sense. It is therefore not surprising that characteristic features as well as characteristic outcomes of self-organization can be observed in the economy.

Networks, the core concept in the approach advocated by Hollingsworth and Müller, are ubiquitous, and they have also been frequently observed in the economy. Production networks, for instance, are an important area of research on economic globalization, international trade is represented in network form, and inter-firm networks continue to be studied. The network concept does not belong to the set of concepts characterizing non-linear dynamics, nor has the metaphor, in contrast to self-organization, been taken from some specific natural science theory. Network is an everyday concept, and it has been used wherever sets of interrelated parts are the object—in engineering, in biology, and last but not least in sociology. A similar argument can be made for bonding, a concept by definition connected to the network concept. Bonding/attraction has been a core social science concept for a long time. Though also used in nuclear physics and chemistry, bonding and attraction are not specific to the new paradigm of non-linear dynamics; in fact, one would associate them sooner with the mechanistic notions of Science I.[2] Power-law distributions are a concept familiar from statistics and again not specially connected with non-linear dynamics. Only the last of the five concepts, multi-level analysis, has links to both perspectives: on the one hand, polities as well as networks can have a multi-level structure (though this must be clearly distinguished from hierarchical relations!). On the other hand, the level-metaphor plays an important role in theories of emergence and reduction; emergence and reduction belong to the paradigm of self-organization (though the authors associate the issue of reduction with Science I).

The network perspective and the self-organization perspective do not contradict or exclude each other, but they do not come together in a single, integrated causal model. Networks and self-organization are distinct empirical phenomena that can be, but need not be, causally connected as it appears in the model of complex networks: How they are related is an open—and fruitful—*empirical* question. Networks, for instance, can be the result of self-organization, but self-organization does not necessarily and not even typically generate networks; instead of positing that networks are generated by self-organization, one could ask under what conditions this will or will not occur. Matthew effects, an instance of power-law distributions, can also be the outcome of a self-organization process, but self-organization does not necessarily produce power-law distributions, nor are Matthew effects necessarily produced by networks. Networks and self-organization processes can even be opposed to each other. In the economy, deliberately created network relations (i.e. cartels) are often used to *contain* spontaneous market processes following the logic of self-organization. It is thus the dialectic *interaction* of network structures with self-organization processes that could be a fruitful topic for socio-economics.

[2]This does not mean that bonding cannot display features of non-linear dynamics. Bonding and attraction should, incidentally, be distinguished from the concepts 'fit' and 'complementarity', used synonymously by the authors; complementarity refers to the mutual functional enhancement of institutions and does not necessarily presuppose direct (manifest) social relations ('bonds').

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Bargaining, not borrowing: on problem choice and problem space

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The attractiveness of evolutionary thinking and systems dynamics, of complexity sciences and network analysis continues unabated at a time when disciplinary boundaries are becoming porous and the frontier of science is rapidly expanding. For the first time, *frontier research* is funded at the EU level, based solely on the criteria of scientific excellence and a genuine bottom-up approach. Fourteen per cent of the total budget of the European Research Council has been allocated for inter- or transdisciplinary and high-risk projects, i.e. attempts to better understand problems that can productively be approached from perspectives that either cross disciplines within or across the domains of physical sciences and engineering, life sciences and social sciences and humanities. While it is too early to assess what kind of changes, let alone transformations, will follow for 'hot' topics or fields, it is encouraging to see that institutional conditions have been set up that invite and enable the kind of transformations that form the core of the important contribution made by Hollingsworth and Müller.

The paper's focus on epistemological issues presents five well-selected and useful concepts that arose in the framework of what the authors call Science II. We are warned in a footnote that the terminology of Science I and Science II is not to be confused with Mode 1 and Mode 2 knowledge production. This is certainly correct, but readers are well advised—and the authors will agree—to keep the institutional preconditions and actual scientific practices in mind that often precede, accompany and co-produce the transformative shifts that scientific inquiry can take when new ground is broken. In other words, we should not overlook the connection that exists between changes in the epistemological foundations and changes that arise in the reconfigurations that combine the various nested 'working knowledges' (Pickstone, 2007) in novel combinations. The way in which science is organized reflects as much as it co-produces the observed transformation. If this is so, there may be more to Science II than Darwin, Prigogine and their legacy, influential as it continues to be. We may have to delve deeper to find out in which way modelling and computer simulation combine with new research technologies and how 'digital' research methods feed upon and produce new kinds of data besides those collected in the traditional way. Above all, we may have to ask how problems come to be defined, and in which 'context of application' of knowledge production and where new contexts of application arise in the wider institutional landscape in which research is undertaken.

Put differently, we should ask the kind of questions that Hollingsworth has raised and answered so convincingly in his investigations of breakthrough developments in the past. Is a new epistemology like the one outlined in the paper discussed here linked to novel forms of knowledge production, and if so, to what degree and through which mechanisms? Instead of focusing on the analysis of the concepts and their ability to travel across disciplinary boundaries, I propose a discussion of two issues that may help to contextualize them in a somewhat different way. One is the issue of problem choice and the other is the issue of problem space.

Much has been written about the usefulness and the limitations of metaphors. Historians of science have devoted meticulous attention to reconstructing the unforeseeable pathways through which concepts 'in flux' travel to new destinations. Mayntz' warnings about the risks that come with borrowing methods, concepts, and models, let alone theories, from other disciplines remain valid, even if they may not have sufficiently been heeded in some quarters of the social sciences and humanities. The importance of technologies, computer modelling and simulation, writing software programmes that facilitate experimentation with digital evolution within very short time spans, has been widely acknowledged, and it continues to be a source of innovation. But this in itself does not make artificial life, game theory, network sciences or behavioural economics more attractive to those who are either ignorant about the opportunities provided by new methodological tools and cross-cutting concepts or who deliberately ignore them as irrelevant for their own questions. Borrowing, let alone imitation, is not sufficient as a strategy, if *transforming socio-economics with a new epistemology* is the declared objective, even if familiarity with different 'working knowledges', as Pickstone aptly calls them since they include epistemic practices, remains indispensable as a precondition. The reason why the use of metaphors or borrowing concepts cannot be sufficient is as simple as it is easy to overlook: every problem-choice by an individual researcher or a research group is embedded in a wider collective problem space that legitimates the problem choice in its openness towards the future while at the same time constraining it.

The range and type of problems that are addressed is neither infinite, nor is their relevance equally distributed. Their scientific attractiveness, their 'sexiness' or 'technological sweetness' is linked to the wish to know, to the curiosity and the enjoyment of discovering how things work. Certainly, the reward system of science plays a role with its promise of big reputational rewards for those who succeed in defining and/or solving problems that are assessed as 'high risk, high gain'. The process of research remains open-ended and inherently uncertain. Surprises, in the guise of serendipity, may enter at any time. Problems, while having a scientific lineage which is often more influential than disciplinary history is ready to admit, do not simply follow a linear tradition, nor is novelty privileged as such. Problems are not a given, since Nature does not whisper into the ear of a scientist which problem to choose. Nor can funding agencies, their widespread conviction to the contrary, set the agenda for research questions, although they certainly influence it. Problem choice remains undervalued as a phenomenon and underresearched as practice, perhaps because it remains so firmly wedded to the belief in the autonomy of the scientific community and the high social value assigned to free scientific inquiry.

Progressive problem shifts that open up new territory or allow one to see the familiar in new light may lead to significant advances in new knowledge production, but they can be identified only after they have occurred. As with other forms of creative, transformative moments, they can be neither predicted nor prescribed. The fortunate circumstances that enabled them can be reconstructed over time by a comparative analysis, as Hollingsworth has brilliantly shown. But none of these preconditions for creativity can explain why one particularly fertile problem was initially chosen and then pursued through hard and persistent work. We may learn with the benefit of hindsight why a specific sub-group of a given scientific community chose to work on a particular problem which turned out to be a goldmine. Or, a competent rebel may emerge, who chooses to venture out into unknown territory, knowingly taking the risk that his or her research could lead over the cliff. In case of failure, such an individual performs a valuable service to the wider community by alerting it to which paths should be avoided, since they lead nowhere. As Gaston Bachelard knew so well, science progresses through errors. The pathways leading to errors as well as to the ways to overcome them begin with problem choice.

To put it more concretely: we can see how Science II, enabled through practices of knowledge production of Mode 2, has led to problem choices of individuals and research groups that focus on phenomena occurring in very different domains. The nature of problem choice has led researchers to cross disciplinary boundaries. The study of self-organization and networks, of complexity in its various forms and manifestations, the investigation of the general binding problem and of power-law distributions are the result of deliberate problem-choices. But—and this is the next important step to take if we want these and similar problem choices to become more widely institutionalized—the collective problem space in which they are to be embedded, contextualized and nurtured also needs to be reconfigured. In the social sciences there is still a strong defensiveness against what are frequently perceived to be neocolonial attempts by the natural sciences to expand at their expense. The collective problem space remains, with the exception of peripheral regions, largely closed. In the natural sciences, there is growing awareness of the importance of interfaces with the social and cultural world. But progress is often hampered by the lack of recognition that their study demands more serious and long-term engagement with the social sciences. It cannot be done with sleight of hand or a mere (minor or too crass) adjustment of the modelling assumptions.

The collectively organized problem space legitimates, accommodates, positions and provides the dynamic move forward that, taken together, constitutes the kind of transformational force that H&M argue for. They rightly point to the need to identify what constitutes a common problem of whose existence researchers in other fields may not be aware, so that mutual interaction and learning can take place. They also notice correctly that many of the most fascinating concepts are borrowed from or stem from conceptual issues that are high on the agenda of the life sciences. But what is a ‘common problem’ and what does it take to be perceived as such? One of the most interesting problems for the life sciences today is to explain, i.e. observe, measure, model and compare, the phenomenon of cooperation and its emergence, enforcement, and relation to conflict, its comparative range and variation. One would expect that this resonate deeply with social scientists for whom problems of social order, based on cooperation and conflict and a variety of mechanisms to regulate them, have formed an integral part of their research agenda since the beginning. Certainly, it helps when methods and models are shared. Game theory and its continued refinement in ever new experimental settings, with real subjects or with digital organisms, are a case in point. This is the reason why behavioural/experimental economics and artificial life flourish, as the vast literature on punishment and social norms, on retaliation and collaboration, shows (Fehr and Gächter, 2000, 2002). But how deeply does knowledge about cooperation that has arisen in the life sciences really infiltrate the collective problem space of the social sciences—either to be taken up in a spirit of interdisciplinary cooperation or to be contested in open and fair engagement?

Or, take the phenomenon of self-organization which is widely recognized today in its ubiquity. Again, what relevance has it been accorded in the collective problem space of urban studies? And if its relevance is recognized, how has it come about? One can also point to the growing number of studies, mostly initiated on the side of the life sciences and bio-medicine, that seek to differentiate genetic from environmental factors that include culture and behaviour on outcomes like individual and collective variations in stress resistance or susceptibility to diseases. If there is one area that cries out for interdisciplinarity, it is this interface where new knowledge, especially about epigenetics and the complex regulatory function of genes, invites social knowledge to move in (Nowotny and Testa, in press). But are the vast majority of social scientists even aware of the fact that they are addressed and needed? Another example: an important concept like that of degeneracy in biological systems, i.e. the ability of elements that are structurally different to perform the same function or yield the same output, cries out for comparison with the concept of functional equivalence in social systems, but also with similar concepts in technical systems, where redundancy and design play an important, but different role. Given the ubiquity of degeneracy in biological systems, it also forms part of human activities, specifically in language and communication, where metaphor and ambiguity may function in a positive and creative way (Edelman and Gally, 2001).

The lack of mutual awareness and familiarity with the working knowledge of other disciplines will be addressed only if overlapping problem spaces emerge. 'Trading zones' (Galison, 1997) must be established that invite encounters with different problem definitions, methods and concepts that can be transformed into bargaining items. Incentives must exist. The issue is not so much borrowing, but bargaining, which presupposes mutual interest in what the other side has to offer and hence some kind of reciprocity. *Transforming socio-economics with a new epistemology* sets out a neatly arranged and attractive package which hopefully will set into motion a network of bargaining exchanges. The mutual benefits and potential gains are certainly there.

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Science (2008) 'Special Section: Cities', **319**, 739–775.

Interdisciplinarity in socio-economics, mathematical analysis and the predictability of complex systems

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1. Introduction

The essay by Hollingworth and Müller (2008) on a new scientific framework (Science II) and on the key role of transfers across disciplines makes fascinating reading. As an active practitioner of several scientific fields (earthquake physics and geophysics, statistical physics, financial economics, and some incursions into biology and medicine), I witness every day first-hand the power obtained by the back-and-forth transfer of concepts, methods and models occurring in interdisciplinary work and thus applaud the formalization and synthesis offered by Hollingworth and Müller (2008). Section 2 presents a personal highlight of my own scientific path, which illustrates the power of interdisciplinarity as well as the unity of the mathematical description of natural and social processes

But my goal is not just to flatter Rogers and Karl and praise their efforts. I wish here to suggest corrections and complements to the broad picture painted in Hollingworth and Müller (2008). I will discuss two major claims in some detail. First, in Section 3.1, I take issue with the claim that complex systems are in general 'not susceptible to mathematical analysis, but must be understood by letting them evolve—over time or with simulation analysis'. In Section 3.2, I present evidence of the limits of the claim that scientists working within Science II do not make predictions about the future because it is too complex. I conclude with Section 4, in which I point out a possible missing link between Science I and Science II, namely 'Quantum Science', and the associated conceptual and philosophical revolution. I also tone down the optimism echoed by Hollingworth and Müller (2008) that the approaches in terms of complex networks will allow for a stronger transfer of theoretical models across widely disparate fields; in particular, between the natural and social sciences.

2. A personal highlight illustrating the power of interdisciplinarity and unity of the mathematical description of natural and social processes

Let me illustrate with a personal anecdote how the power of interdisciplinarity can go beyond analogies to create genuinely new paths to discovery. In the example I wish to relate, the same fundamental concepts have been found to apply efficiently to model, on the one hand, the triggering processes between earthquakes leading to their complex space–time statistical organization (Helmstetter *et al.*, 2003; Ouillon and Sornette, 2005; Sornette and Ouillon, 2005) and, on the other hand, the social response to shocks in such examples as Internet downloads in response to information shocks (Johansen and Sornette, 2000), the dynamic of sales of book blockbusters (Sornette *et al.*, 2004; Deschatres and Sornette, 2005) and of viewers' activity on YouTube.com (Crane and Sornette, 2007), the time response to social shocks (Roehner *et al.*, 2004), financial volatility shocks (Sornette *et al.*, 2003) and financial bubbles and their crashes (Johansen *et al.*, 1999, 2000; Sornette, 2003; Andersen and Sornette, 2005; Sornette and Zhou, 2006). The research process developed as follows.

First, the possibility that precursory seismic activity, known as foreshocks, could be intimately related to aftershocks has been entertained by several authors in the past decades, but has not been clearly demonstrated by a combined derivation of the so-called direct Omori law for aftershocks and of the inverse Omori for foreshocks within a consistent model. In a nutshell, the Omori law for aftershocks describes the decay rate of seismicity after a large earthquake (called a mainshock), roughly going as the inverse of time since the mainshock. The inverse Omori law for foreshocks describes the statistically increasing rate of earthquakes going roughly as the inverse of the time till the next mainshock. The inverse Omori law has been demonstrated empirically only by stacking many earthquake sequences (see Helmstetter and Sornette, 2003 and references therein). We had been working for several years on the theoretical understanding of a statistical seismicity model, known at the ETAS model, a self-excited Hawkes conditional point process in mathematical parlance. This model or its siblings are now used as the standard benchmarks in statistical seismology and for evaluating other earthquake forecast models (Jordan, 2006; Schorlemmer *et al.*, 2007*a*, *b*). We had the intuition that the inverse Omori law for foreshocks could be derived from the direct Omori law by viewing mainshocks as the 'aftershocks of foreshocks, conditional on the magnitude of mainshocks being larger than that of their progenitors'. But we could not find the mathematical trick to complete the theoretical derivation. In parallel, we were working on the statistical properties of financial returns and were starting a collaboration with J.-F. Muzy, one of the discoverers of a new stochastic random walk with exact multifractal properties, named the multifractal random walk (MRW; Bacry *et al.*, 2001; Muzy and Bacry, 2002), which seems to be a powerful model of financial time series. We then realized that similar questions could be asked on the precursory as well as posterior behaviour of financial volatility around shocks. The analysis of the data showed clear Omori-like and inverse Omori-like behaviour around both exogenous (11 September 2001, or the coup against Gorbachev in 1991) and endogenous shocks. It turned out that we were able to formulate the solution mathematically within the formalism of the MRW and we showed the deep link between the precursory increase and posterior behaviour around financial shocks (Sornette *et al.*, 2003). In particular, we showed a clear quantitative relationship between the relaxation after an exogenously caused shock and the relaxation following a shock arising spontaneously (termed 'endogenous'). Then, inspired by the conceptual path used to solve the problem in the financial context, we were able to derive the solution in the context of the ETAS model, demonstrating mathematically the deep link between the inverse Omori law for foreshocks and the direct Omori law for aftershocks in the context of the ETAS model (Helmstetter *et al.*, 2003; Helmstetter and Sornette, 2003). The path was simpler and clearer for financial time series, and their study clarified the methodology to be used for the more complicated specific point processed modelling earthquakes.

This remarkable back-and-forth thought process between the two *a priori* very different fields will remain a personal highlight of my scientific life.

3. On self-organizing processes and multi-level analysis

The emphasis of Hollingworth and Müller (2008) on self-organizing processes and multi-level analysis to comprehend the nature of complex social systems is welcomed as it indeed reflects an important strategy used by researchers. But more problematic is the endorsement of the claims, which are variations of a common theme, that

(1) 'increasingly analysts maintain that such systems are not susceptible to mathematical analysis, but must be understood by letting them evolve—over time or with simulation analysis' (p. 399),
(2) 'the emerging perspective, rapidly diffusing across academic disciplines, suggests that the world does not change in predictable way' (p. 398),
(3) 'hardly any scientist in these fields is able to make successful predictions about the future, as self-organizing processes are understood best by retrospective analysis' (p. 403).

Hollingworth and Müller (2008) give thus resonance to a view upheld by various groups in different communities, which I find misguided and dangerous, while unfortunately widespread.

3.1 On models of complex systems

Let me first address claims (1) and (2), perhaps best personified by Stephan Wolfram and elaborated in his massive book entitled 'A New Kind of Science' (Wolfram, 2002). According to Wolfram, the most interesting problems presented by nature (biological, physical and societal) are likely to be formally undecidable or computationally irreducible, rendering proofs and predictions impossible. Take the example of the Earth's crust and the problem of earthquake prediction or the economies and financial markets of countries and the question of predicting their recessions and their financial crashes. Because these events depend on the delicate interactions of millions of parts, and seemingly insignificant accidents can sometimes have massive repercussions, it is argued that their inherent complexity makes such events utterly unknowable and unpredictable. To understand precisely what this means, let us refer to the mathematics of algorithmic complexity (Chaitin, 1987), which provides one of the formal approaches to the study of complex systems. Following a logical construction related to that underpinning Gödel's (1931) incompleteness theorem, most complex systems have been proved to be computationally irreducible, i.e. the only way to decide about their evolution is to actually let them evolve in time. The only way to find out what will happen is to actually let it happen. Accordingly, the future time evolution of most complex systems appears inherently unpredictable. Such statement plays a very important role in every discussion on how to define and measure complexity.

However, it turns out that this and other related theorems (see Chaitin, 1987 and Matthew Cook in Wolfram, 2002) are useless for most practical purposes and are in fact misleading for the development of scientific understanding. And the following explains why. In a now famous essay entitled 'More Is Different', Phil Anderson (1972), 1977 Nobel Prize winner and a founder of the Santa Fe Institute of complexity, described how features of organization arise as an 'emergent' property of systems, with completely new laws describing different levels of magnification. As a consequence, Physics, for instance, works and is not hampered by computational irreducibility. This is because physicists only ask for answers at some coarse-grained level (see Buchanan, 2005 for a pedagogical presentation of these ideas). In basically all sciences, one aims at predicting *coarse-grained* properties. Only by ignoring most molecular detail, for example, did researchers ever develop the laws of thermodynamics, fluid dynamics and chemistry, providing remarkable tools for explaining and predicting new phenomena. From this perspective, one could say that the fundamental theorems of algorithm complexity are like pious acts of homage to our intellectual ancestors: they are solemnly taken out, exhibited and solemnly put away as useless for most practical applications. The reason for the lack of practical value is the focus on too many details, forgetting that systems become coherent at some level of description. In the same vein, the butterfly effect, famously introduced by Lorenz (1963, 1972) to communicate the concept of sensitivity upon initial conditions in chaos, is actually not relevant in explaining and predicting the coherent meteorological structures at large scales. As a result of the spontaneous organization of coherent structures (Holmes *et al.*, 1998), there is actually predictability in meteorology and climate as well as in many other systems, at time scales of months to years, in apparent contradiction with the superficial insight provided by the butterfly effect.

These points were made clear by Israeli and Goldenfeld (2004, 2006) in their study of cellular automata, the very mathematical models that led Wolfram to make his grand claims that science should stop trying to make predictions and scientists should only run cellular automata on their computers to reproduce, but not explain, the complexity of the world. Cellular automata are systems defined in discrete Manhattan-like meshed spaces and evolve in discrete time steps, with discrete-valued variables interacting according to simple rules. These remarkably simple systems have been shown to be able to reproduce many of the behaviours of complex systems. In particular, it is known that most of them are ‘universal Turing computational machines’, i.e. they are capable of emulating any physical machine. Because they can emulate any other computing device, they are therefore undecidable and unpredictable. But which of the systems are capable of universal computation is not generally known. In this respect, one results stands out for our purpose. Matthew Cook, whose theorem is reported in Wolfram (2002), showed that one simple cellular automaton, known as ‘rule 110’ in Wolfram’s nomenclature of one-dimensional cellular automata with nearest-neighbour interaction rules, is such a universal Turing machine.

Now, Israeli and Goldenfeld applied a technique called ‘renormalization group’ (Wilson, 1999) to search for what could be the new laws, if any, that describe the coarse-grained average evolution of such cellular automata. Technically, the new laws are determined by a self-consistency condition that (i) coarse-graining the initial conditions and applying the new laws should provide the same final description and (ii) letting the system evolve according to the true microscopic laws and then coarse-graining the resulting pattern. By coarse-graining, one focuses only on the most relevant details of the pattern-forming processes. Israeli and Goldenfeld established that computationally irreducible cellular automata become predictable and even computationally reducible at a coarse-grained level of description. The resulting coarse-grained cellular automata that they constructed by coarse-graining different cellular automata were found to emulate the large-scale behaviour of the original systems without accounting for small-scale details. In particular, rule 110 was found to become a much simpler predictable system upon coarse-graining. By developing exact coarse-grained procedures on computationally irreducible cellular automata, Israeli and Goldenfeld have demonstrated that a scientific predictive theory may simply depend on finding the right level for describing the system. For physicists, this is not a surprise: by asking only for approximate answers, Physics is not hampered by computational irreducibility, and I believe that this statement holds for all natural and social sciences with empirical foundations.

3.2 *On predictability of the future in complex systems*

Let me now turn to the third claim cited in the above introduction of Section 3 that 'hardly any scientist in these fields is able to make successful predictions about the future', and more generally that predicting the future from the past is inherently impossible in most complex systems. This view has recently been defended persuasively in concrete prediction applications, such as in the socially important issue of earthquake prediction (see e.g. the contributions in Nature debate [1999]). In addition to the persistent failure in reaching a reliable earthquake predictive scheme up to the present day, this view is rooted theoretically in the analogy between earthquakes and self-organized criticality (Bak, 1996). Within this 'fractal' framework, there is no characteristic scale and the power-law distribution of earthquake sizes suggests that the large earthquakes are nothing but small earthquakes that did not stop. Large earthquakes are thus unpredictable because their nucleation appears not to be different from that of the multitude of small earthquakes.

Does this really hold for all features of complex systems? Take our personal lives. We are not really interested in knowing in advance at what time we will go to a given store or drive in a highway. We are much more interested in forecasting the major bifurcations ahead of us, involving the few important things, like health, love and work, that count for our happiness. Similarly, predicting the detailed evolution of complex systems has no real value, and the fact that we are taught that it is out of reach from a fundamental point of view does not exclude the more interesting possibility of predicting the phases of evolutions of complex systems that really count (Sornette, 1999).

It turns out that most complex systems around us do exhibit rare and sudden transitions which occur over time intervals that are short compared with the characteristic time scales of their prior or posterior evolution. Such extreme events express more than anything else the underlying 'forces' usually hidden by almost perfect balance and thus provide the potential for a better scientific understanding of complex systems. By focusing on these characteristic events, and in the spirit of the coarse-graining metaphor of the cellular automata discussed in Section 3.1, a small but growing number of scientists are re-considering the claims of unpredictability. After the wave of complete pessimism on earthquake prediction in the West of the 1980s and 1990s, international earthquake prediction experiments such as the recently formed Collaboratory for the Study of Earthquake Predictability (Jordan, 2006) and the Working Group on Regional Earthquake Likelihood Models (Schorlemmer *et al.*, 2007*a*, *b*) aim to investigate scientific hypotheses about seismicity in a systematic, rigorous and truly prospective manner by evaluating the forecasts of models against observed earthquake parameters (time, location, magnitude, focal mechanism, etc.) that are taken from earthquake catalogues.

Recent developments suggest that non-traditional approaches, based on the concepts and methods of statistical and non-linear physics, could provide a middle way to direct the numerical resolution of more realistic models and the identification of relevant signatures of impending catastrophes, and in particular of social crises. Enriching the concept of self-organizing criticality, the predictability of crises would then rely on the fact that they are fundamentally outliers (Johansen and Sornette, 2001), e.g. financial crashes are not scaled-up versions of small losses, but the result of specific collective amplifying mechanisms (see Chapter 3 in Sornette, 2003, where this concept is documented empirically and discussed in the context of coherent structures in hydrodynamic turbulence and of financial market crashes). To address this challenge, the available theoretical tools comprise in particular bifurcation and catastrophe theories, dynamical critical phenomena and the renormalization group, non-linear dynamical systems and the theory of partially (spontaneously or not) broken symmetries. Some encouraging results have been gathered on concrete problems [see the reviews by Sornette (2005, 2008) and references therein], such as the prediction of the failure of complex engineering structures (a challenge generally thought unreachable by most material scientists), the detection of precursors to stock market crashes with real advance published predictions (another unattainable challenge generally according to most financial economists) and the prediction of human parturition and epileptic seizures, to cite some subjects I have been involved with, with exciting potential for a variety of other fields.

Other pioneers in different disciplines are slowly coming to grip with the potential for a degree of predictability of extreme events in many complex systems (see, for instance, the chapters in Albeverio *et al.*, 2005). Let us also mention Jim Crutchfield who proposes that connections between the past and future could be predicted for virtually any system with a 'computational mechanics' approach based on sorting various histories of a system into classes, so that the same outcome applies for all histories in each class (Ay and Crutchfield, 2005; Crutchfield and Görnerup, 2006). Again, many details of the underlying system may be inconsequential, so that an approximate description much like Isreali and Goldenfeld's coarse-grained cellular automata models can be organized and used to make predictions.

Agent-based models developed to mimic financial markets have been found to exhibit a special kind of predictability. While being unpredictable most of the time, these systems show transient dynamical pockets of predictability in which agents collectively take predetermined courses of action, decoupled from past history. Using the so-called minority and majority games as well as real financial time series, a surprisingly large frequency of these pockets of predictability have been found, implying a collective organization of agents and of their strategies which 'condense' into transitional herding regimes (Lamper *et al.*, 2002; Andersen and Sornette, 2005). Again, grand claims of intrinsic lack of predictability seem to me like throwing out the baby with the bath water, forgetting that the heterogeneous nature in space and time of the self-organization of complex systems does not exclude partial predictability at some coarse-grained level.

Let me end this discussion by extrapolating and forecasting that a larger multidisciplinary integration of the physical and social sciences together with artificial intelligence and soft-computational techniques, fed by analogies and fertilization across the natural and social sciences, will provide a better understanding of the limits of predictability of crises.

4. Concluding remarks on quantum decision theory and the theory of networks

I would like to conclude with two remarks.

Hollingworth and Müller (2008) contrast the 'old' Descartes–Newton Science I with the new Science II framework which emphasizes concepts such as complex adaptive systems, self-organization and multi-scale patterns, scale invariance, networks and other buzzwords. I was surprised not to see discussed another Science the 'Quantum Science' emerging from the scientific and philosophical revolution triggered by the understanding that Nature works through the agency of fundamentally quantum mechanical laws, which have very little to do with the macroscopic laws apparent directly to our five perception senses. In my view, for a variety of disciplines, but perhaps not yet for the social sciences, quantum mechanics has had more impact than Science II. At the ontological level, Quantum Science has had a tremendous influence in all fields, by providing a fundamental probabilistic framework, rooted in the Heisenberg uncertainty principle, the intrinsic non-separability theorem and the existence of intrinsic sources of noise and energy in the fluctuations of the 'void' (showing that the void does not exist ontologically). This has attacked, much more deeply than, for example, the theory of chaos ever has, the misconception that future scenarios are deterministic and fully predictable.

In this concluding section, I would like to suggest that Quantum Science may enjoy a growing impact in the social sciences, via the channel of decision making operating in humans by emphasizing the importance of taking into account the superposition of composite prospects, whose aggregated behaviour form the structures, such as society and economies, that scholars strive to understand. In a preliminary essay, Slava Yukalov and I have introduced a 'quantum decision theory' of decision making based on the mathematical theory of separable Hilbert spaces on the continuous field of complex numbers (Yukalov and Sornette, submitted), the same mathematical structure on which quantum mechanics is based. This mathematical formulation captures the effect of superposition of composite prospects, including many incorporated intentions, which allows us to describe a variety of interesting fallacies and anomalies that have been reported to characterize the decision-making processes of real human beings.

My second remark concerns the claim that 'complex networks allow for a transfer of theoretical models across widely disparate fields'. I am afraid that the optimism that the theory of complex networks will play such a special role is nothing but more hype, somewhat in the lineage of those in the last decades that involved buzzwords such as fractals, chaos, and self-organized criticality. They all had their period of fame and excesses, followed by maturation towards a more reasonable balanced position within the grand edifice of science. With Max Werner, we have recently commented on the limits of applying network theory in the field of earthquake modelling and predictability (Sornette and Werner, 2008) and I believe much the same criticisms would apply to the use of network theory in the social sciences. With Malevergne and Saichev, we have developed this in theoretical synthesis (Malevergne *et al.*, submitted) showing in particular that the mechanism of 'preferential attachment', at the basis of the understanding of scale-free networks found in social networks, the world-wide web or networks of proteins reacting with each other in the cell, is nothing but a rediscovery and rephrasing in a slightly different language of the famous model of incoming and growing firms developed by Simon in 1955, based on the Gibrat principle of proportional growth (Gibrat, 1931). The 'new' science of networks thus has deep roots in economics! Viewing the rather unsophisticated level of many discussions on power laws and other statistical regularities reported in the 'new' science of networks, while not disputing the existence of significant progress in network theory, I wonder whether this 'new' science would not profit from a better reading of the best works in economics of the twentieth century. Ending on a more positive note, this illustrates my fervent faith in the power of interdisciplinarity, practiced with the rigor and diligence necessary to ensure depth and fecundity.

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