Journal of Institutional Economics (2015), 11: 1, 39–45
© *Millennium Economics Ltd 2014* doi:10.1017/S174413741400040X
First published online 27 August 2014

Next steps toward an economics for a creative world: a comment

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Abstract. In this comment I discuss the excellent contribution of Koppl *et al.* (2014) to our understanding of the role of institutions in the economic system. Much that is offered by them reinforces and extends previous analysis by institutional and evolutionary economists. However, I argue that there are areas of evolutionary economics and complex economic system analysis that need to be integrated into their evolutionary economic perspective to make it more complete. Also, I argue that they are too kind to mainstream economists who purport to deal with institutions and complexity and that they should consider where the boundaries of science should lie in economics.

This is a very welcome article by Koppl *et al.* (2014). To have two biologists, along with two management scientists, contributing to a coherent and well-argued defence of positions already held by many evolutionary economists, particularly those who have built their analysis explicitly upon complex systems theory, is very encouraging. Starting out in modern evolutionary biology, as might be expected, they see the economic system as complex, adaptive and subject to the forces of competitive selection. But they also see important differences to biological systems. Although not discussed in any detail, the key differences are thermodynamic. Biological and economic systems are both composed of dissipative structures that maintain themselves away from the thermodynamic equilibrium that physical systems always tend towards. Both constantly import free energy and export entropy and both are subject to evolution.

In the biological case, knowledge encoded in DNA, *imposed* by experience and competitive selection, drives evolution. In the economic case, knowledge is not only imposed but actively *acquired* through creativity and cooperation in production and trade (Foster (2005)). The filter of competitive selection also operates on the resultant 'self-organised' entities but, as Koppl *et al.* (2014) stress, this is heavily dependent upon the 'enabling' institutional structure that is collectively upheld in the 'mental models' of individuals. And, of course, this structure also evolves. So, they focus upon the knowledge dimension of economic evolution but do not discuss, in any explicit way, its energetic character. This is

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https://doi.org/10.1017/S174413741400040X Published online by Cambridge University Press

not really necessary for the arguments that they make about the importance of institutions in evolving economic systems but I think it is important to deal with energetic questions when making general comparisons between the evolution of biological and economic systems (Schneider and Sagan (2005), Foster (2011)).

Koppl *et al.* (2014) waste no time in pointing out that, when there is evolution going on of any kind, both the parameters and the range of observables in any mathematically-defined model must change. So the fixed phase space in conventional economic models must, necessarily, change and thus, we cannot actually write down what are referred to as 'entailing equations'. Economic evolution is seen, quite correctly, as ultimately about 'emergence' and this, in turn, is facilitated by 'novelty intermediation' which, to a large extent, involves entrepreneurship of some kind.

Now there is nothing particularly new in all this: similar discussions can be found in the modern evolutionary economics literature and, indeed, the cited Harper and Lewis (2012) article points to many of the key contributions. For example, the article by Foster and Metcalfe (2012) deals with the evolutionary economic perspective on emergence and overlaps strongly with what is said about novelty and entrepreneurship. It is encouraging, however, to see non-economists concluding that it is in the *non-mainstream* economics literature that a better understanding of how and why economic growth comes about can be gained. It is not easy in a short article to do justice to the richness of all of the relevant literature in evolutionary economics. However, the limited selection of contributions discussed is good and I am in considerable agreement with much of what is written but there are some points where I must disagree.

First, in discussing the constrained optimizing algorithmic approach of neoclassical economics and its offshoots Koppl *et al.* (2014) refer to an abstract model of the neoclassical kind, such as the Dynamic Stochastic General Equilibrium (DSGE) model, as a 'low dimensional approximation to a possibly high dimensional reality'. However, it has to be stressed that any theory that makes a set of highly abstract assumptions that are not stylised approximations of historical reality *cannot* be a theory that is connected to that reality. It cannot be denied that such constructions can spark of a better understanding of some real phenomenon but the body of logic itself is not, and cannot be, a simplification of reality. This was even pointed out a long time ago by two mainstream economists who both played key roles in the development neoclassical economic analysis, namely, Hahn and Solow (1997). However, despite being major figures in mainstream economics for decades, these eminent economists have been virtually ignored, particularly in macroeconomics.

This is very important to understand because it disqualifies much of mainstream economics as science. This is reinforced by the fact that, although much of neoclassical economic logic has been firmly refuted when exposed to real world evidence, many economists have carried on regardless. We do not observe this in a serious science. So in Foster (2005) such models are

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classified as 'simplistic' rather than 'simple.' They don't connect with reality in any ontological or wider methodological sense. So it is not just the absence of 'creativity' that is the problem – acknowledging the role of creativity just compounds a fundamental problem that already exists.

Second, it is asserted that 'mainstream economics has been greatly influenced by complexity theory'. However, any reading of the main body of modern literature in, for example, mainstream macroeconomics, suggests that this is hardly the case. The opposite is true: the key tenants of complexity theory have been obstinately and/or strategically ignored to an increasing extent. Take, for example, the introduction to general equilibrium by Balasko and Geanakoplos (2012) that is cited by Koppl *et al.* (2014). This does not incorporate understandings gleaned from complexity theory in any fundamental way. On the contrary, it is a hard-line 'Lakatosian' defence of highly unrealistic neoclassical economic theory in the face of the challenge posed by complex system thinking.

Economists who have come to view the economic system as complex and adaptive have all strongly rejected neoclassical economic theory as a basis for modelling a real economic process. One of the earliest and best examples was Nobel Laureate Friedrich Hayek who understood well the complex character of economic systems and, thus, the importance of institutional rules to facilitate order and to enable entrepreneurship in economic systems. There is a great deal more of an understanding of the economy as a complex system in Hayek's writings (see Hayek/Caldwell (2010)), than in anything provided by modern general equilibrium theorists. For this reason, I would have liked Koppl *et al.* (2014) to have connected their discussion much more to the work of Hayek and other neo-Austrians, as well as pioneering systems theorists writing back in the 1950s and 1960s, particularly Herbert Simon, who doesn't get a mention.

Now it is often said these days that the rise of experimental economics and behavioural economics is indicative that complex system thinking is pervading mainstream economics. Although it is true to say that these developments do acknowledge the inherent complexity of the economic system, invariably the starting point is some individualistic proposition or hypothesis derived from neoclassical economic theory, either in its strategic or non-strategic guise. And even when the evidence turns out to be negative it is remarkable how little of the so-called 'heterodox' literature is cited, even when highly relevant. In contrast, Koppl *et al.* (2014) demonstrate a keen awareness of this literature but this is most likely because they do not suffer the disadvantage of being 'well-trained' economists! However, on the down side, they also cite Durlauf (2012) who ignores almost all the relevant non-mainstream literature. Unsurprisingly, he comes to a negative judgement on the usefulness of complexity theory as a general starting point in economic analysis. All he is willing to concede is that mainstream economics can be improved, here and there, by insights derived from complexity theory. To reach this judgement what he does is to narrow down the

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role of complexity theory to its capacity to provide new mathematical tools that can generate mainstream models with better 'predictions.' Of course, this is precisely the methodological mistake that Koppl *et al.* (2014) point to.

Agent-based modellers (ABMs) receive the strongest reprimand from Durlauf (2012). Why? Because they calibrate their models on real historical data and, as such they are viewed as 'observationally equivalent' to standard models, such as DSGE, which can do the same. But the key difference that he ignores is that ABMs engage in historical investigations to obtain realistic assumptions for their models, unlike neoclassical economists who try to relate their abstract and timeless models to real experience by the judicious choice of unrealistic assumptions: of parameters, of functional forms and through the imposition of 'ad hoc' hypotheses concerning slow 'disequilibrium' adjustment, lags in acquiring information or computational limitations, so that they seem to 'fit' real historical data. But this means there is no observational equivalence since, as has been pointed out, neo-classically based models do not have scientific content.

There are, of course, well known problems with the ABM methodology in relation to its use of calibration but at least the models bear some stylised connection to reality. This does not mean that ABM is science in the conventional sense but, at least, it offers a model-based account of history, guided by complex system theory, which can then be compared with the intuitive and subjective interpretations of reality offered by economic historians. Malerba *et al.* (2001) have shown how enlightening the resultant 'history-friendly' modelling methodology can be in understanding how economic evolution has occurred over historical time and, being model based, they show how counterfactual analysis can be conducted for the benefit of policymakers or business strategists.

So, in my view, Koppl *et al.* (2014) are much too generous to modern mainstream economists and they do not spend enough time looking back at the advances that occurred prior to the takeover of economics by neoclassical economists adhering either to strategic (mainly microeconomic) or non-strategic (mainly macroeconomic) constrained-optimization-in-equilibrium theory. I think it is much better to go back to the key insights of an earlier era than to begin with inappropriate modifications to an unrealistic and unscientific body of theory which pays only lip service to selected aspects of complexity theory.

Third, Koppl *et al.* (2014) are too quick to dismiss mathematics as a useful tool in understanding economic evolution. They highlight the importance of 'novelty intermediation' echoing strongly many writings in both the neo-Austrian and neo-Schumpeterian economic traditions, where the entrepreneur is placed centre stage in the story of economic emergence in states of uncertainty. It is true that deductive mathematical models are of little use when economic dynamics involve creativity and entrepreneurship. As Koppl *et al.* (2014) point out, once 'entailing laws' are dismissed as useful in understanding economic evolution we

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are in the domain of 'uncaused causes'. But they also admit that 'broad patterns' of emergence can be discerned, in line with Hayek's thinking. We can't write down any valid equations of motion for an evolutionary process or, indeed, for any kind of complex process. But, in economics, we don't actually observe such processes. Instead, what we observe are aggregate outcomes of such processes usually measured in monetary values. This is true for firms, industries and whole economies.

This aggregate data involves 'broad patterns' because the order that must exist in a complex economic network induces a degree of historical irreversibility. Thus, many economic aggregates are observed to grow quite stably over time, i.e., their values are highly correlated with their values in a previous period. This is very true for macroeconomic data which involves a vast aggregation of the output of all processes. As we move to lower levels of aggregation covering smaller numbers of processes we observe data that is less stable over time. Equally, the longer the time period over which we observe macroeconomic data the less stability we observe. When there is so much historical continuity in data it can be captured in a mathematical specification, at least over some historical periods. Such specifications are not a product of formal deductive logic but, rather, they are inductive constructions that can be interpreted using complex systems theory. So what we have are not 'entailing equations' but statistical representations of non-equilibrium (historical) processes. They tend to be 'quasi-stable' when growth is pronounced and structurally unstable in stationary states. Econophysicists have been greatly interested in non-equilibrium dynamics of this kind but their studies are not discussed much by Koppl *et al*. (2014).

If, as Koppl *et al*. (2014) attest, novelty intermediation gives rise to a new niche that can be filled with new flows of economic value then, based upon a large number of empirical studies of innovation diffusion, we can hypothesise that growth will follow a sigmoid-shaped curve toward a limit. Such a limit is not an 'equilibrium' in any identifiable phase space but, rather a stationary state reached in history which is structurally unstable. Underlying this growth in value which, in neo-Schumpeterian economics, follows a radical innovation made possible by entrepreneurship, are incremental innovations and learning by doing (i.e., incremental innovations in skills and teamwork). The 'framing', extensively discussed by Koppl *et al*. (2014), is very much about the diffusion of innovations whereby there is a dual emergence of new order and increased complexity in a vast range of activities. Part of economic structure erodes and a new part emerges. Although this process is often discussed in evolutionary economics in terms of the operation of competitive selection it is, at base, a self-organisational process that yields economic structures that are then selected.

Koppl *et al*. (2014) state that their 'main result is negative' but I would argue that it is overly negative. There are ways of using quantitative modelling in economic systems, because of the widespread valuation of many stocks and flows in monetary terms, which are not possible in biological systems. They note,

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correctly, that there is no shortage of novelty, only the capacity to take advantage of it through forging new connections in existing economic networks and making new connections outside an existing network. 'Novelty intermediation' is closely connected to the notion of 'entrepreneurship' and, of course, there is a large literature covering this in neo-Austrian and neo-Schumpeterian economics and also in management science. In contrast, mainstream economists have tended to avoid the whole issue. A good example of this is the influential 600 page book of Aghion and Howitt (1998), the 'bible' of modern endogenous growth theorists. They claim that their theory is 'Schumpeterian' but there is not a single entry in the index on 'entrepreneur' or 'entrepreneurship'!

Koppl *et al.* (2014) also discuss the role of 'mental models' in coordinating economic decisions in a society. Denzau and North (1994) are cited but there is considerably more literature on this, both before and after. Shared mental models result in the emergence of institutions, or 'meso-rules' (Dopfer *et al.* (2004)). Over the past three decades, even mainstream economists have taken on board the idea that institutions are important. But the so-called 'new institutional economics' is, yet again, an attempt to graft on institutions to standard constrained optimization models of economic behaviour.

Finally, Koppl *et al.* (2014) point out that 'experts' can pose a problem as monopolists in the ownership of useful knowledge. They can slow progress. A state of distributed knowledge, although messier, is viewed as more progressive and this can be rationalised using standard variation and selection thinking. This is an insight but one that is best expressed within a network representation of economic systems. However, networks are mentioned only at the very end of the article yet any complex system representation should begin with the identification of the incomplete network that exists and how and why it has been changing over time. For example, the 'cambiodiversity' that is discussed is really about the complexity of elements and connections in an economic network, therefore, is should be specified as such at the outset, as occurs, for example, in the cited Hildago *et al.* (2007). Also, although important work by Jason Potts is cited by Koppl *et al.* (2014), Potts (2000) would seem to be a better starting point in developing an appropriate network perspective. Another example of an important concept discussed Koppl *et al.* (2014) that only has real meaning from a network perspective is 'adjacent possible'. There is already a literature on 'preferential attachment' that is related to this concept and would be useful to explore (for example, Newman (2003)).

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