Economics for a creative world: a response to comments

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Abstract.
We reiterate our main contributions: 1) our more careful demonstration of why "mechanistic" models have limited application, 2) our account of novelty as a system-level phenomenon, and 3) our identification of "novelty intermediation" as important to creative economic dynamics. We also address some criticisms. Pavel Pelikan's idea of stochastic causality does not somehow eliminate unprestateable change. We do challenge certain strong notions of universal causation, as Ulrich Witt notes, but such notions are probably best abandoned. Although we do not repudiate mathematical modeling as our paper suggested to John Foster, we may give less scope than Foster to such methods. Finally, we point out the extreme difficulty of implementing the sort of engineering vision Colander articulates.

Those, who are strongly wedded to what I shall call 'the classical theory', will fluctuate, I expect, between a belief that I am quite wrong and a belief that I am saying nothing new. It is for others to determine if either of these or the third alternative is right.
J.M. Keynes

We are honored to receive comments from an extraordinary group of economists, all of whom are working in one way or another beyond the bounds of orthodox economics. The generally positive tone of the comments and the high level of general agreement are striking to us. However, our commenters do have some objections. Colander thinks we are too hard on mainstream economics, while Foster thinks we are too soft. Foster objects that we seemingly reject the use of mathematics in economics entirely, and Witt seems to think our paper veers into a kind of theoretical nihilism. Pelikan thinks that economists "may not know what to do" with our rather high-level generalities. Finally, there is some question of how innovative our argument might be. Although our commenters were generally favorable to our analysis and always kind, we were sometimes reminded of Henry Hazlitt's criticism of Keynes' *General Theory*: "What is original in the book is not true; and what is true is not original" (1983, p.3).

We think that our analysis does make a contribution if it is correct. Before responding to individual comments, we would like to restate what we think our main contributions to have been.

First, our explanation of why "mechanistic" models are generally inappropriate in economics may be more careful and satisfactory than previous efforts. As we tried to indicate in our paper, we use the potentially elastic word "mechanistic" only for models in which the dynamics are represented as "the unfolding of a process fully described, up to a stochastic error term, by a master set of equations or an evolution function." We show that the phase space *is not* stable and *cannot be* stable, which has not quite been done previously as far as we can tell. We noted the similarity of our analysis to Shackle's discussion of the listing problem. But we get unlistability from a close internal critique of physics-based models rather than considerations of the creativity of human decision-making. (See also Felin, Kauffman, Koppl & Longo, 2014.) Strictly speaking, therefore, our discussion is applicable only to modeling strategies that require a stable phase space.

Second, and relatedly, we show how novelty emerges even without *ex nihilo* acts of "creation" by gods or persons. Novelty production occurs at the systems level in our analysis. As far we can tell this path to novelty and unlistability in economics has not been taken before.

Finally, we develop the concept "novelty intermediation." While we have stuck mostly to a metatheoretic framework, our discussion of novelty intermediation was meant to show that our framework matters for applied work. We noted that Potts (2012) and Earl & Potts (2004) gave us "the first clear and reasonably complete statements of the basic idea." Our treatment advances upon theirs, however, because we identify the structural element in the system that makes it possible for novelty intermediaries to emerge. With Potts (2012) and Earl & Potts (2004), the idea is that certain businesses know about recent innovations that have already taken place, whereas the retail consumer does not. These businesses inform the consumer by suggesting certain combinations or offering products that exhibit certain combinations. In our analysis, instead, the intermediary knows what combinations of *inputs* may generate new *discoveries*.

We turn now to some remarks of our individual commenters. Space constraints prevent us from going into some of the issues as deeply as we might otherwise desire.

Pelikan and Witt both question our rather strong claims about causality. Pelikan thinks we neglect "stochastic causality." Witt upbraids us for denying the "principle of causation." It is a bit difficult to address these serious concerns in part because space limitations prevent each author from fully specifying what these terms mean. Thus, we are only too keenly aware of the risk that we might unintentionally misconstrue their arguments. Such a danger is well illustrated by Hodgson's (2004, pp. 57-65) lucid discussion of causality and of "the ambiguous bogeys of mechanism and determinism." Like Bunge (1959), he notes the diversity of meanings that may attach to terms such as "causality" and "determinism." Bunge's (1959) classic also notes the tension between theories such as those of Locke, Berkley, Hume, and Kant, that view "causation as a mental construct, as a purely subjective phenomenon" and is own view that "causation has an ontological status," though one that "raises epistemological problems" (pp. 5-6). In spite of the dangers of miscommunication, we will address the concerns of both Pelikan and Witt.

When Pelikan invokes "stochastic causality" he may have in mind something like the "probabilistic causality" described by Suppes (1970) and Cartwright (2006). In this theory, C "causes" E if the conditional probability of E given C is greater than the conditional probability of E given not-C. This and similar ideas may seem to conflate epistemology and ontology. However that may be, our purposes suggest a very different sort of response. The similar definitions given by Suppes and Cartwright both require us to have "a state description over a 'complete' set of confounding factors" (Cartwright, p. 58). In a footnote, Cartwright says, "The scare quotes are around 'complete' because it is a difficult notion to define." This wise remark suggests why our "reasoning about the non-algorithmic nature of economic change" may not be "significantly weakened" by notions of "stochastic causality." Such notions would seem to depend on a stateable set of possibilities such that a probability measure may be defined. But if our vision of creative dynamics is about right, a probability measure cannot generally be defined unless possible events are described only in very general terms. For similar reasons, we doubt that the notion of "stochastic algorithm" mentioned by Pelikan would much alter our analysis.

We might evade Witt's criticism of our views on "law" and "cause" by pointing out that we have criticized only a very narrowly circumscribed set of ideas on law and cause. Our repudiation of "entailing laws" leads us to reject the notion that the future is somehow wholly contained in the past. We are weaving our way into the future, not unrolling a tapestry that was completed long ago. We are conscious of C. S. Peirce's remark, "The great principle of causation which we are told, it is absolutely impossible not to believe, has been one proposition at one period of history and an entirely disparate one [at] another and is still a third one for the modern physicist. The only thing that has stood . . . is the *name* of it" (1898, p. 197). Witt seems to recognize the issue when he says, "The problem seems to be that the notion of a 'law' can be given different meanings." We explicitly note, as Witt acknowledges, that "cause and effect still operate in the econosphere."

We could dodge Witt's criticism in this way, but we will not. We have indeed taken a swipe at notions to the effect that every thing has a cause, or at least a cause that we can identify in advance. We think it is noteworthy that our position emerges from a careful look at one relatively narrow class of models rather than general considerations of "cause" or "law." In any event, the conclusion that economic change is not algorithmic does compromise at least some notions of the "principle of causation." In this sense it is, perhaps, "radical." We are not alone in chipping away at such notions. Chaitin has interpreted algorithmic information theory to imply that "certain mathematical facts are true for no reason" (2006, pp. 77-78). Citing Bell's inequalities, Filk and von Müller have noted that "Leibniz' principle of sufficient reason does not hold in quantum theory" (2009, p. 64). If we are going to hold fast to strong notions of universal causation, it seems, we will have to ignore or deny some important results in mathematics and physics.

In physics we know what we mean by cause. In classical physics, for example, force is mass times acceleration. In chemistry too we know, at least for classical chemical reaction systems. In quantum mechanics "cause" is less clear. And in evolution it may be even less clear. Certainly, we would all say that the tiger biting the gazelle "causes" its death. But Longo Montévil and Kauffman (2012) deny entailing laws for evolution. If "causal mechanism" and "cause" require *entailing laws*, then there is no causal mechanism for evolution as a whole. In the social sciences, where responsible free will and consciousness may play a role, what should we mean by "cause"? Are preferences (utility functions) "causes"? Beyond that, our claim is that enablement is real. We might say that finding a loophole in a law (as it were) *enables* but does not *cause* the new strategy that may emerge and endure. The mainframe computer's success did not *cause* the personal computer but it did (with the invention of the chip) *enable* the PC whose wide sale did not *cause* but did *enable* the emergence of word processing, and so on to the web and to selling on the web. Each of these innovations was *enabled-but-not-caused* by what had come earlier.

Our biggest disagreement with Foster concerns his characterization of us as "non-economists"! This is perhaps technically true of Kauffman, Felin (more or less), and Longo—at least in terms of their primary training and disciplinary background. But Koppl's graduate training was in economics. Indeed, Koppl is usually considered something of an "Austrian," which might make him a heterodox economist (Koppl 2006). And his previous co-authors have included economists from other heterodox schools. Kauffman and Felin have also respectively contributed to economics: Kauffman to evolutionary economics and Felin to the domain of organizational economics. Space limitations drove us to make only some references to these literatures, but not because we were unaware of them. Furthermore, Foster thinks we are "too quick to dismiss mathematics as a useful tool in understanding economic evolution." Perhaps. But we did express a favorable disposition to several mathematical tools, including the one Foster seems to like best, namely, mathematical network theory. But part of the problem is that even heterodox economics, including mathematized versions, are often built on a particular brand of theorizing (e.g., Nelson & Winter, 1982) which cannot meaningfully account for the emergence of novelty (Felin and Foss, 2011).

Colander says we are too hard on the mainstream and Foster says we are too soft. Those two very different opinions give us hope that we may have hit the Goldilocks point: neither too hard nor too soft, but just right. Colander says the "reasonable mainstream" already has a "skeptical attitude toward economic experts." If so, some well-respected names may fall outside the "reasonable mainstream." For example, Reis (2013) has said, "The central bank may be more effective in technical tasks where ability to incorporate quickly changing knowledge is more important than effort at meeting the goals in a strict mandate" set down by the elected representatives of the people (p. 19). This defense of central bank independence expresses little skepticism of economic experts. Policy prescriptions emerging from some of the network-theoretic literature on financial-market contagion (Acharya 2009; Beale et al. 2011; Caccioli et al. 2011; Gai, Haldane,and Kapadia 2011; Haldane and May 2011; and Yellen 2009, 2011) also seems to reflect a sanguine view of economic expertise. Acharya, for example, says it is "paramount" that banks report their "portfolio compositions" to "the regulator" so that it can compute systemic risk and "determine the collective risk capital charge for each bank" (2009, p. 248). But he does not ask whether this procedure gives too much power to economic experts.

Colander says, "the reasonable mainstream's commitment to formal scientific methodology requires any theoretical considerations to have all i's dotted and t's crossed before these policies become part of the academic discourse." But at least two Nobel laureates have expressed a very different attitude. Akerlof and Shiller (2009) compare the policymaker to the Cat in the Hat who, they remind us, "tried Plan A, and then Plan B, and then Plan C, and then even Plan D." We should emulate the Cat, they say, and "go on down the alphabet, until we find something" that works (location 263). This zeal to "go on down the alphabet" is hardly the slow and cautious attitude of someone who wants all i's dotted and t's crossed. Colander would surely object that he explicitly referred to *science* and not to *policy*, which is always an art (Colander 1992), especially in times of crisis. Yes. But the policy experiments advocated by Akerlof and Shiller seem largely unconstrained by theory, which calls into question the hyper-conservatism of theory Colander describes. The theoretical conservatism of the mainstream (in macroeconomics at least) would not seem to be a point in its favor. The tools of mainstream macroeconomic theory have *not* been evolving in the right direction. The mainstream has been committed to methods that implicitly deny the creativity and dynamism of the real system. It is as if the mainstream is saying that we need to do more work on the theory of ox carts if we are to hope for progress in understanding rocket ships! This hyper-conservatism of macroeconomic theory might be changing in the wake of the crisis, but if so the center of gravity has not yet moved very far.

Finally, we would like to address Colander's important remarks about engineering. Colander has long emphasized the lost art of economics (Colander 1992). Economists have lost sight of the art of economics as understood by John Neville Keynes. Beside the "positive *science of political economy* which is concerned purely with what is" and the "*ethics of political economy*," which "seeks to determine *economic ideals*," there is the "*art of political economy*, which seeks to formulate *economic precepts*" (Keynes 1917, p. 36). These "precepts" are "rules" or "maxims" through which "given ends may best be obtained" (Keynes, 1917, p. 32). This "art" is what Colander has in mind when he tells us to think of applied economics as engineering.

It seems hard to question the desirability of adopting "the strategy for causing the best change in a poorly understood or uncertain situation." But what is that strategy? On this vital question, we do not agree amongst ourselves. Should we support the approach of "complex engineered systems" in which "performance characteristics emerge from the implemented system rather than existing in a fully specified form ex ante" (Koppl et al. 2010)? Or is a more urgent collective response is required, as Longo believes, particularly considering the damage humans may be doing to the ecosystem? Perhaps activities such as the production of new chemicals that might present dangers such as endrocrine disruption (a major challenge now) require the dynamic proposal of rules forbidding this, directing that, and canalizing the other. Perhaps the engineering challenge is how to make economic projects in a continually changing frame.

Whether "the answer" is something about markets, something about democracy, or something else, our vision of creative dynamics shows how hard Colander's engineering task is. It shows how hard it is to know what is "the strategy for causing the best change." To have a *best* strategy, you need a partial order with a maximum and, therefore, some sort of pregiven space, which is exactly what we deny. Our analysis challenges strong notions of optimality in economics and (following Longo, Montévil, and Kauffman 2012) biology. The front legs of a kangaroo, elephant, or opossum are not optimal. They are the outcome of the one possible evolutionary trajectory for tetrapod front legs that happened to have been followed so far. Individuals and organizations may have preferences, which may imply a kind of optimality of individual choices. One may try, as it were, to become a kangaroo, though it may not work. But notions of optimality do not easily apply to the creative dynamics of the biosphere and the econosphere.

Giuseppe Longo's answers, only in part integrated in the published common text.

I will elaborate on some of Colander's comments, *in italics* :

p. 5
Koen defines the engineering method as "the strategy for causing the best change in a poorly understood or uncertain situation within the available resources

Heuristics includes all theories and models, and any other aid, such as intuition, experience

Using an engineering methodology, nothing is off the table.

A scientific methodology is focused on understanding for the sake of understanding

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The distinction science/engineering is surely not new, but it is nicely spelled out here and in a very pertinent way. In particular, it is pertinent as we borrow ideas from biology, evolution mainly. Now, Darwin's theory proposes historical knowledge and in no way it was meant nor it is meant for prediction and action: it is understanding for the sake of understanding.

However, knwoledge of evolution shaped all of biology and reached medicine. Moreover, we have a problem today: our relation to the ecosystem. Some of the effects of human action have an evolutionary relevance. I will mention a major one (endocrine disruptors and cancer) also because I will further elaborate on it on the grounds of our perspective on "creative economics" (the further problem, in my opinion is "which creation do we need to enable, besides new industrial and financial products, besides cambiodiversity?").

In the XXth century we produced 80,000 new (artificial) molecules. The process was accelerated after WWII, as it gradually became possible to apply, at the industrial level, a fantastic achievement of Quantum Mechanics: a deep or almost complete understanding of chemical interactions. So, it has become possible to create new molecules almost at leisure and very often these provided new (sometimes fantastic) material. Theoretical Chemistry, with its formal rules of molecular interaction, followed with delay this fast process and there was an even slower follow up by quantitative and qualitative analyses of the chemical structure vs. activity relationship (see C. D. Selassie, *History of Quantitative Structure-Activity Relationships*, **Burger's Medicinal Chemistry and Drug Discovery,** Sixth Edition, Volume 1: Drug Discovery Edited by Donald J. Abraham John Wiley & Sons, 2003).

As a consequence, we created, with little control and global scientific knowledge, major endocrine disruptors. A recent strong statement on the connection between the endocrine disruptions we witness and (finite combination of) the new molecules we threw in the ecosystem is in E. Diamanti-Kandarakis et al**.** *Endocrine-disrupting chemicals: an Endocrine Society scientific statement.* **Endocr Rev** 30:293-342, 2009. An example, resulting from this increasing cambiodiversity have been the halving of spermatozoa density in western men in 60 years (!!) as well as the increasing incidence of cancer in any life age, in spite of major fights against other specific causes – artificial colors in food, smoke (see A. Soto, C. Sonnenschein. *Environmental causes of cancer: endocrine disruptors as carcinogens.* **Nat Rev Endocrinol.** 6:363-370, 2010). Endocrine disruptors may have an evolutionary relevance: they are deeply affecting our species and many other forms of life.

So, even the purely historical analysis of species' evolution needs today to be transformed also into an "engineering" of our relation to the ecosystem. We need a project, that is a choice on how do we want to live in this or that context, we need to borrow tools from any discipline and combine inventions of techniques with the invention of *rules* on how to handle them.

Like engineers, in economy, as Colander rightly says, we need a project, in particular an economic project. Yet this project cannot be given in a predefined phase space, this is our central theme. First we clearly reject any idea of economy as an equilibrium system and in no way we consider the various equilibrium approaches as scientific, in economy (and biology). It would be like analyzing a Benard cell or a hurricane by extremizing a lagrangian function in an hamiltonian equilibrium context: it is simply wrong. No ecosystem, biological, human, is at (thermodynamic) equilibrium; most of the time, they are not even stationary (constant flow of energy or matter). Nor the "new" approaches to economy, based on statistical physics, escape this judgment. At best they suppose a thermodynamics tending to equilibrium, based on (often implicit) maximum entropy principles; this implies a characterization of the ensemble of micro-states within a given theoretical frame, that is pre-defined phase space and transformations on it.

Finally, we consider inadequate also the very rare far from equilibrium analyses. In physics, also these systems are "state determined", that is history does not matter, only the instantaneous state "determines" the future, even in Quantum Mechanics, where the structure of determination includes randomness (Schroedinger's equation determines the dynamics a probability law, in a pre-given phase space, a Hilbert space – but the treatment, in this frame, is "at equilibrium" as the equation is derivable from an hamiltonian). Biological and human contexts instead are heavily depending on history. In humans, the historical path followed to reach a state of affairs is part of common memory, it contributes explicitly to action and forecast (see G. Longo, *How Future Depends on Past Histories in Systems of Life,* donwloadable, in print, 2015)

Moreover, in our paper, we recalled the peculiar role of randomness in biology, which independently appears in many levels of organization and determination (molecular, cellular, tissue, organismal…) and in their interactions (bio-resonance). Following, Longo and Montévil, 2014, we see randomness as a key contribution to adaptation and diversity, thus to stability, via the notion of "extended critical transition", that is the permanent reconstruction of biological coherence structures in ever changing phase spaces. Similarly, we believe that randomness contributes, in economy, to the creation of new phase spaces, that is the very space of observables and parameters.

This is in radical contrast with existing physical theories and their applications and variants in economy, as randomness is always mathematically analyzed within a pre-defined space of possibilities, or phase space. Out analysis moves randomness up, at the level of the very constitution of the phase space.

Colander's engineering challenge is then: “how to make economic projects in such a continually and changing frame?”. We cannot give this duty up, both for ecosystemic and social reasons. The free creation of new derivative products caused a highly inproductive concentration of richness, similarly as the free creation of new molecules is affecting our biological life. And making human projects means proposing new ways of being together, both as scientific and economic actors; projects refer to the social link and the role of democracy, also in economic (and ecosystemic) decisions. “How can I built this new kind of bridge, which tools, which physical/engineering rules should I follow? Do I need to discover new ones?” asks the engineer. In economy, how can we propose rules that coordinate our human action without killing entrepreneurial, private and public, creativity? We must produce new molecules, but this must be done under a close, scientific follow-up of their ecosystemic consequences; this requires a dynamic proposal of rules forbidding this, directing that, canalizing that other, a major scientific challenge.

In summary, the engineering challenge we are facing in economy is how to contribute to a social environment continually capable of invention as well as to propose rules maintaining an ever changing, but working “coherence” among humans and between humans and their environment. Democracy as the explicit debate on principles is at the core of this process: which is the social agenda in the use of derivative products and tax policies that allowed in the Bush era the transfer of 80% of the growth to 1% of the population? Is this socially and economically viable? How to regulate the fantastic mathematical creativity that accompanied financial market and is now disrupting actual production?

Increasing endocrine disruption and cancer, say, are not viable paths; thus, lists of forbidden carcinogens or biological disruptors, joined to severe control of their industrial production, are not limitations of creativity - as a matter of fact they require major biological understanding and invention. We have to be creative in ruling our human and ecosystemic interactions, by making an explicit, not conservative, but dynamic project, capable of regulating while promoting new viable paths. The fundamental “engineering” challenge is then the creation of viable, motivated, explicitly discussed (an essential component of democracy), dynamically changing institutional regulations, even in absence of a pre-given space of possibilities. A fundamental regulation of our societies also derives from the free social debate, including the right to strike. The disappearance of strikes in the private sectors in euro-countries is a dramatic change; it the loss of a major form of democratic control as well as of a pressure towards industrial novelty creation that had a major role since the end of the XIXth centuries, in spite of the abuses.

In summary, a regulated, yet changing, viable, coherence, as a democratic project, between humans and within their environment is the invention we have to face: inventing working regulations is the hardest but essential creativity which is now more than ever required.

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