

This was a paper hard to read. It does not mean that the paper was badly written. The difficulty of the task that the author sought enforced him to write this difficult paper. After struggling a week in reading the paper, I am rather sympathetic with Delorme. In a sense, he was unfortunate, because he came to be interested in complexity problems by encountering two problems: (1) road safety problem and (2) the Regime of Interactions between the State and the Economy (RISE). I say "unfortunate," because these are not good problems with which to start the general discussion on complexity in economics, as I will explain later. Of course, one cannot choose the first problems one encounters and we cannot blame the author on this point, but in my opinion the good starting problems are crucial to further development of the argument of complexity in economics.

Let us take the example of the beginning of modern physics. Do not think of Newton. It is a final accomplishment of the first phase of modern physics. There will be no many people who object that modern physics started by two (almost simultaneous) discoveries: (1) Kepler's laws of orbital movements and (2) Galileo's law of falling bodies and others. The case of Galilei can be explained by a gradual rise of the spirit of experiments. Kepler's case is more interesting. One of crucial data for him was Tycho Brahe's observations. He improved the accuracy of observation about 1 digit. Before Brahe for more than one thousand years, accuracy of astronomical observations was about 1 tenth of a degree (i.e. 6 minutes in angular unit system). Brahe improved this up to an accuracy of 1/2 minute to 1 minute. With this data, Kepler was confident that 8 minutes of error he detected in Copernican system was clear evidence that refutes Copernican and Ptolemaic systems. Kepler declared that these 8 minutes revolutionize whole astronomy. After many years of trials and errors, he came to discover that Mars follows an elliptic orbit. Newton's great achievement was only possible because he knew these two results (of Galilei and Kepler). For example, Newton's law of gravitation was not a simple result of induction or abduction. The law of square-inverse was a result of half-logical deduction from Kepler's third law.

I cite this example, because this explains in which conditions a science can emerge. In the same vein, the economics of complexity (or more correctly economics) can be a good science when we find this good starting point. (Science should not be interpreted in a conventional meaning. I mean by science as a generic term for a good framework and system of knowledge). For example, imagine that solar system was composed of two binary stars and earth is orbiting with a substantial relative weight. It is easy to see that this system has to be solved as three-body problem and it would be very difficult for a Kepler to find any law of orbital movement. Then the history of modern physics would have been very different. This simple example shows us that any science is conditioned by complexity problems, or by tractable and intractable problem of the subject matter or objects we want to study.

The lesson we should draw from the history of modern physics is a science is most likely to start from more tractable problems and evolve to a state that can incorporate more complex and intractable phenomena. I am afraid that Delorme is forgetting this precious lesson. Isn't he imagining that an economic science (and social science in general) can be well constructed if we gain a good philosophy and methodology of complex phenomena?

I do not object that many (or most) of economic phenomena are deeply complex ones. What I propose as a different approach is to climb the complexity hill by taking a more easy route or track than to attack directly the summit of complexity. Finding this track should be the main part of research program but I could not find any such arguments in Delorme's paper. ([Yoshinori Shiozawa](#), A Cognitive Behavioral Modelling for Coping with Intractable Complex Phenomena in Economics and Social Science. In Economic Philosophy: Complexity in Economics (WEA Conference), 10/10/2017.)

1) My paper can be viewed as an exercise in problem solving in a context of empirical intractability in social science. It was triggered by the empirical discovery of complex phenomena raising questions that are not amenable to available tools of analysis, i.e., are intractable. Then the problem is to devise a model and tools of analysis enabling to cope with these questions. Then, unless someone comes with a complex system analysis or whatever tool that solves the problem at stake, a thing I would welcome, I can't think of any other way to proceed than focusing on the very cognitive process of knowledge creation and portraying it as a reflective, open-ended, problem-first cognitive behavioral endeavour. It is an approach giving primacy both to looking and discovering rather than to assuming and deducing, and to complexity addressed in its own right rather than to complex systems in which complexity is often viewed tautologically as the behavior of complex systems. The outcome is a new tool of analysis named Deep Complexity in short. I believe that the availability of this tool provides a

means to take more seriously the limitations of knowledge in a discipline like economics in which inconclusive and non demonstrative developments are not scarce when sizeable issues are involved.

2) Yoshinori Shiozawa raises the question of where to start from, from tractable problems or from the intractable? He advocates the former and suggests to “evolve to a state that can incorporate more complex and intractable phenomena”. But then, with what tools of analysis for intractable phenomena? And I would have never addressed intractability if I had not bumped into unresolved empirical obstacles. Non commutative complementarity is at work here: starting with the tractable in a discipline dominated by non conclusive and non demonstrative debates doesn't create any incentive to explore thoroughly the intractable. It is even quite intimidating for those who engage in it. This sociology of the profession excludes de facto intractability from legitimate investigation. Then starting from the possibility of intractability incorporates establishing a dividing line and entails a procedural theorizing in which classical analysis can be developed for tractable problems when they are identified, otherwise the deep complexity tool is appropriate, before a substantive theorizing can be initiated. It is a counterintuitive process: complexification comes first, before a further necessary simplification or reduction. ([Robert Delorme](#), (WEA Conference), 11/30/2017.)

In my first comment in this paper, I have promised to argue the track I propose. I could not satisfy my promise. Please read my second post for the **general comments** in discussion forum. I have given a short description on the working of an economy that can be as big as world economy. It explains how an economy works. The working of economy (not economics) is simple but general equilibrium theory disfigured it. The track I propose for economics is to start from these simple observations.

As I have wrote in my first post, modern science started from Galileo Galilei's physics and Johannes Kepler's astronomy. We should not imagine that we can solve a really difficult problem (Delorme's deep complexity) in a simple way. It is not a wise way to try to attack deep complexity unless we have succeeded to develop a sufficient apparatus by which to treat it. ([Yoshinori Shiozawa](#), A Cognitive Behavioral Modelling for Coping with Intractable Complex Phenomena in Economics and Social Science. In Economic Philosophy: Complexity in Economics (WEA Conference), 11/30/2017.)

Dear Dr Shiozawa, it seems that we are not addressing the same objects of inquiry. Yours seems to stand at an abstract level of modern science in general. Mine is much less ambitious: it is grounded in research on how to deal with particular, empirically experienced problems in real economic and social life, that appear intractable, and subject to scientific practice. Deep Complexity is the tool that is manufactured to address this particular problem. It may have wider implications in social science. but that is another story. ([Robert Delorme](#), A Cognitive Behavioral Modelling for Coping with Intractable Complex Phenomena in Economics and Social Science. In Economic Philosophy: Complexity in Economics (WEA Conference), 11/30/2017.)

You are attacking *concrete social problems*. I am rather a *general theorist*. That may be the reason of our differences of stance toward your problem.

Our situation reminds me the history of medicine. This is one of the oldest science and yet as the organism is highly complex system, many therapies remained **symptomatic**. Even though, they were to some extent useful and practical. I do not deny this fact. However, modern medicine is now changing its features, because biophysical theories and discoveries are changing medical research. Researchers are investigating the molecular level mechanism why a disease emerges. Using this knowledge, they can now design drugs at the molecular level. Without having a real science, this is not possible,

Economics is still in the age of pre-Copernican stage. It would be hard to find a truth mechanism why one of your examples occurs. I understand your intention, if you want say by the word of “deep complexity” a set of problems that are still beyond our ability of cognition or analysis. We may take a method very different from the regular science and probably similar to **symptomatology** and **diagnostics**. If you have argue in this way, it would have made a great contribution to our forum on complexities in economics. This is what I wanted to argue as the third aspect of complexity, i.e. complexity that conditions the development of economics as science.

To accumulate symptomatic and diagnostic knowledge in economics is quite important but most neglected part of the present day economics. ([Yoshinori Shiozawa](#), A Cognitive Behavioral Modelling

for Coping with Intractable Complex Phenomena in Economics and Social Science. In Economic Philosophy: Complexity in Economics (WEA Conference), 12/1/2017, *italics added.*)

It is interesting to learn that, as an economist and social scientist, I must be in a "pre-Copernican" stage. Although what this means is not totally clear to me, I take it as revealing that our presuppositions about scientific practice differ. You claim to know what is the most appropriate way of investigating the subject I address, and that this way is the methods and tools of natural science. I claim to have devised a way which works, without knowing if it is the most appropriate, a thing whose decidability would seem to be quite problematic. And the way I have devised meets the conditions of a reflective epistemology of scientific practice, in natural science as well as in social science. Your presupposition is that the application of the methods of natural science is the yardstick for social science. This is scientism.

My presupposition is that there may be a difference between them, and that one cannot think of an appropriate method in social science without having first investigated and formulated the problem that is presented by the subject. As a "general theorist", your position is enjoyable. May I recall what Keynes told Harrod: "Do not be reluctant to soil your hands". I am ready to welcome any effective alternative provided it works on the object of inquiry that is at stake. It is sad that you don't bring such an alternative. As Herb Simon wrote, "You can't beat something with nothing". I borrow from your own sentence that "if you had argued this way, it would have made a great contribution to our forum..." ([Robert Delorme](#), A Cognitive Behavioral Modelling for Coping with Intractable Complex Phenomena in Economics and Social Science. In Economic Philosophy: Complexity in Economics (WEA Conference), 12/1/2017, *italics added.*)