Session 1

Introduction

Heikki Hyötyniemi
Helsinki University of Technology
Control Engineering Laboratory

`heikki.hyotyniemi@hut.fi`

In the field of complex systems *everything* is complex — it is even difficult to define what it means that something is "complex". In this context, the idea of *emergence* is taken as the unifying principle: It is assumed that in a complex system some unanticipated higher-level functionality or structure pops up when simple low-level constructs are appropriately connected and when they interact. The results on the higher level cannot be predicted when only the building blocks on the lower-level are seen. In this way, research on complex systems defies traditional scientific practices: It is *holistic* rather than *reductionistic.*

As systems scientists (!) our ambition is to construct models of systems, however complex they happen to be. How to attack a modeling problem that by definition defies engineering-like deterministic analysis attempts?

1.1 Attacking complexity in general ...

The first steps when constructing model for a system, complex or not, involve outlining the system, determining its boundaries and dependency structures; after that, data is collected. Model construction involves abstraction: The available data is contracted so that only the most relevant information remains. Despite approved modeling principles a great deal of ingenuity and intuition is necessary when pieces are put back together.

We see various examples of complex systems around us, intuitively understanding that there is something fundamentally similar beneath the surface. However, the complex systems we see are so multifaceted that it is difficult to see what this underlying similarity is. Typically the low-level actors, whatever is their physical appearance, are essentially identical, but after some kind of *dynamic processes* involving *positive feedback loops* take place, some kind of *self-organization* emerges where some kind of *fractal* and *self-similar* patterns can be detected. For example, take the following examples:

- In a *genetic (cybernetic) system,* the emergent phenomenon is specialization of tissues and organs, resulting from interaction between genes and enzymes.
- In a *cognitive system,* the emergent phenomenon is intelligence or consciousness, resulting of interaction between individual neurons.
- In a *social system,* the emergent phenomenon is the birth of organizations, nations, and business enterprises, and the rivalry among them, resulting from actions of individual humans.
- In a *memetic system,* the emergent phenomenon is the formation of ideologies and religions, and also scientific paradigms, resulting from an interplay between ideas.

What can be said about *all* complex systems in general? From the modeling point of view, one has to recognize that there exist much too little data to draw definite conclusions. All examples of complex systems are too diverse, there is too little similarity to uniquely determine the underlying similarities. Will the “General Theory of (All) Complex Systems” remain just a dream forever?

It has been claimed that complexity theory will revolutionize old ways of thinking. Is this just empty jargon, or is there some relevance beneath the bluster? Indeed, it can be claimed that ways of doing science *will not be the same any more.* The natural sciences doing analysis on natural phonomena will change drastically because the *nature being studied will change.* — What on earth does this claim *mean?*

As compared to traditional modeling problems, where, too, phenomena in the physical environment are being explained, a deeper transition is taking place. Let us study the power of new thinking in concrete terms:

> The proponents of traditional science say that the intuitive nature and shaky definition of complex systems is a disadvantage — a pessimist would say that one cannot study complexity because one cannot define it in the first place. But one recognizes a complex system when seeing one. As optimists we can think that luckily enough we *do not have to* define what a complex system exactly is! A complex system is anything that *looks like a complex system.* The essense is there, no matter what is the origin of the system, whether that creature is dwelling in biosphere or in infosphere. This makes it possible to artificially *generate fresh data* — examples of complex behavior — for analysis purposes.

Complexity as a research topic, or searching for the general rules underlying all complex systems, is perhaps a better manageable problem than studying only one complex system at a time.

For example, in *artificial life research* one is no more bound to the limitations of real, existing carbon-bound life forms: The science of the principles governing the universal properties of life may transcend from "life as we know it" to "life as it could be". It is easier to see the big ideas and basic principles when there exists a larger body of material to study — this kind of wealth can be reached when artificial life forms are constructed. The computer is the tool for creating strange worlds where creatures defined in terms of simple mathematical formulae interact. Whether or not complex behaviors emerge — the boundaries of life can thus be mapped. The philosophers will also have new material to ponder about: How to focus the age-old definitions of what life is?

To have a glimpse of what kind of manifestations complex systems can have, it is necessary to look at different kinds of examples. Thus, showing examples of complex systems from different points of view is the goal in this report.

Whatever is the final destiny of complexity research, one contribution that will remain is this shift from *analytic* way of doing science towards *synthetic* science. It is *simulation* that plays a central role in future science. However, simulation must not be an end in itself.

1.2 ... and complexity research in particular

One reason for the current interest in complex systems research is due to Stephen Wolfram's new book "A New Kind of Science". However, complex systems research has a much longer, colorful history, its mathematics dating back to Poincaré Orbits, continuing to Lorenz Attractors, and Mandelbrot Sets, etc. No definite conclusions have ever been drawn, and the same challenges are invented once again after the previous generation of researchers has left the field. Research seems to be cyclic — at least the branches of research that are driven by fashionable buzzwords. Indeed, the society of complexity researchers itself constitutes a complex system: The field is a mosaic where different ideas exist — every now and then particularly good ideas emerge from chaos, staying alive and flourishing, being seeds of further great ideas.

The goal here is to try and understand the field of complex systems research. What is the dynamics there? Can we estimate the future state, and perhaps even personally contribute in reaching this new understanding? How to make the research cycles into *spirals,* so that when the same old ideas pop up later, they can be seen on some higher level of understanding?

When trying to understand complex systems research, history cannot be forgotten. Typically in sciences, it is the results that are of importance, and one can forget about the background with its detours and dead ends. When seen in retrospect, the Kuhnian "paradigm shifts" only remain visible. Now, on the other hand, one is facing a science during its making, and everything is in turmoil — the new stasis has not yet been reached. It is not yet clear what is the hard core of the new paradigm. Modeling of dynamic transients is much more challenging than static modeling of the steady states. Without enough understanding of the Hegelian "Spirit of Complexity", its natural dynamics and inertias, there seems not to be much sense in the bumping of this research field. This Spirit determines what is "interesting" at some time; the Spirit is an emergent phenomenon, not bound to individual researchers but to the dynamics of the whole research community. If the Spirit is not mature enough, an idea, however good, is not understood and widely discussed, and it suffers a "memetic death", not becoming a landmark along the evolution of the research branch. To understand the whims of this free Spirit, one first has to understand what is *interesting.* One has to know what kind of understanding there already exists, how the current state has been reached, and what are the painstaking problems at the moment.

It turns out that the results from complexity theory can be applied for analysis of this interestingness issue. As shown later, the interesting areas typically reside *between order and chaos.* How to remain there in research work, not to sway over the borderline to hopeless disorder and chaos — this is a key question. Indeed, it is easy to do "ironic science" in the chaotic domain, introducing new and fancy ideas with no solid grounding; to remain on the boundary between order and chaos in complexity research, one explicitly has to connect new ideas to relevant mathematics. Mathematics is an emblem of order; it is just a way to express things in a logical, consistent way, and non-mathematics is not much more than hand-waving.

Stephen Wolfram speaks of New Science where simulation is the exclusive way to do science, and mathematics has to be abandoned altogether. However, after seeing the flood of endless images and patterns, one has to face the fact that intuition has to be fetched from more concrete analyses. The hypothesis here is that the traditional, mathematically oriented analytic science still remains, and this way the ongoing further complexity research can continue along the edge of chaos.

Sticking to the central role of mathematics may sound like a harsh limitation — but, truly, this restriction only opens up new horizons. Actually, one could be *more ambitious* here than what Stephen Wolfram is: Even if there will not exist New Science, it is the *whole world* of observations that will be changed. Within the computer, there exist an infinite number of New Worlds to be conquered!

To summarize, simulation is a new way to have new data, but it is no substitute for theoretical analysis. However, traditional mathematical tools are not well suited for the new challenges, and new ones need to be developed for analysis of complex systems. Seeing the multitude of complex systems and approaches to attacking them may help to see the problems one is facing when trying to develop such mathematics.

1.3 About the contents of the Report

The role of this report is to try to give an overall view over the field of complex systems research. The view is necessarily incomplete and biased, mainly reflecting the editor's personal view. But if the view were not incomplete, then (following the Gödelian intuition!) it would be certain that it would be *inconsistent.* Different perspectives to the same thing may open up new ways to understanding, and in what follows, the ideas of the selected four projections are briefly explained. All these four projections span a continuum where nothing is black-and-white — neither is it grey, but extremely colorful!

1.3.1 Philosophical view

The field of complex systems cannot be understood if the deedply human nature of this endeavour is not taken into account. The huge promises and equally deep disappointments have characterized the turbulent developments in the field. The fascinating nature of the research persuades people to deep discussions ... this kind of layman's philosophy has divided people's opinions, so that there is a continuum from euphoria to absolute denial.

1. **New Kind of Science ...**
   It has been claimed that the theory of complex systems would someday solve all problems concerning all kinds of complex systems. These systems include biological, economic, and social ... and the problems that would be solved include diseases, poverty, and unhappiness! The goal in Chapter 2 is to give insight in the huge promises.

2. **... or End of Science?**
   It seems that different kinds of unsubstantiated promises have become commonplace in different branches of science, and the concept of *ironic science* has been coined. There are warning voices saying, for example, that physics, starting from natural philosophy in the Middle Ages, is regressing back to metaphysical philosophizing, being based on mere speculations. The claims can be neither validated nor invalidated. This kind of hype is specially characteristic to such fields as complex systems research, where no actual results have been reached to justify the huge promises, and where making unsubstantiated prophesies seems to be the basic style of doing research (Chapter 3).

1.3.2 Top-down view

There are different kinds of ideas of what kinds of structural constructs the complex systems are qualitatively composed of. Even though the differences between paradigms are clear, it is interesting to see the "mental inertia": One often wants to see the world in terms of his/her own mental constructs, being reluctant against fresh approaches; this holds true what comes to the whole research community — at certain times different ways of seeing the world dominate. Looking complex systems research in a perspective, one can recognize the shift from centrally organized to completely distributed control.

1. **From strict hierarchies ...**
   Since the Aristotelian taxonomies, the traditional way of structuring

systems is through *hierarchies;* in 1960's, these views were formalized by Herbert Simon, and the ideas of "empty world" and "almost decomposability" were coined. It has been shown that hierarchies are more robust structures against disturbances and environmental changes that structureless constructions are; perhaps this robustness is the reason for why such hierarchic structures seem to be so common results of natural evolution in different domains, not only in natural systems (biological, neuronal, etc.) but also within societies, organizations, etc. (Chapter 4).

2. **... towards decentralization ...**
   The pursue towards enhanced system robustness has led to extension of the hierarchy idea: The substructures can be given more and more freedom and independence. This kind of ideas are developed within the *agent paradigm.* For example, in concurrent artificial intelligence research speaking of agents seems to be fashionable (Chapter 5).

3. **... and interaction ...**
   In agent systems, there still exist some central coordination; when this external control is explicitly ripped off, so that the structure is more or less random, one has a *network* where no node can be seen as being more important than the others. The interesting thing is that some organization automatically emerges ... As an example of changes in the thinking of what is *efficient,* is that the army-like hierarchic organizations are changing to project organizations where people are networked (Chapter 6).

4. **... only on the local scale?**
   When the distribution idea is taken to the extreme, there is no external control whatsoever, the nodes being able to communicate only with their immediate neighbors, and one has a *cellular automaton.* This approach is the latest hit; however, it seems that more quantitative approaches are needed to reach real analysis tools (Chapter 7).

1.3.3 Bottom-up view

If starting from top, explicitly determining the structures, the essence of emergence is lost. In this sense, the new contributions to the philosophical age-old discussions result from quantitative simulation analyses, where data and algorithms can reveal their inner self without predetermined prejudices. Different kinds of universality properties have been observed in the behaviors of different kinds of complex systems, some of these underlying principles having more long-lasting value than others.

1. **From chaos ...**
   Simple nonlinearities in systems can result in mindbogglingly complex behaviors. Starting in the 1970's, many conceptually valuable phenomena were observed, including *deterministic chaos, bifurcations, fractals, self-similarity, strange attractors,* and *Feigenbaumian universality.* However, it soon became clear that chaos where all order disappears is too trivial; the interesting phenomena take place *between* order and chaos.

2. **... towards new order ...**
   Whereas chaos theory recognizes that simple systems result in hopeless complexity, complexity theory, on the contrary, says that some simplicity can emerge from complex behaviors. Concepts like *phase transitions, edge of chaos, self-organized criticality,* and *highly optimized tolerance* reveal that there exists underlying similarity in very different environments (Chapter 9).

3. **... with new laws of behavior ...**
   It seems that the unifyining principles are concentrated around *power law distributions* that are characteristic to all systems where some kind of self-organization takes place, and where self-similarity can be detected. In such environments the Gaussian distribution is no more the normal distribution! The resulting "Zipf law" structure among variables in complex systems, artificial or natural, seems to offer new tools for high-level analysis of systems (Chapter 10).

4. **... or with no laws?**
   It can be seen that most of the proposed complex systems architectures are very powerful; indeed, the *Turing's machine* can be implemented in those frameworks. Even though this power sounds like a benefit, it is not: It can be shown that in such frameworks the *Gödel's incompleteness theorem* makes it clear that no analysis tools can be developed for such systems. It seems that there is a cycle here from order or higher-level understanding back to chaos — or has something been gained? How to circumvent the scientific dead end (Chapter 11)?

1.3.4 Systems view

It should not be a surprise that complex systems are complex — one just has to somehow tackle with such systems in practice. In the final chapters, examples of practical approaches, old and new, are presented. The objective of systems engineering is to manage and understand systems, however complicated they might be.

1. **From hard mathematics ...**
   One of the last theoretical success stories of modern system theory was the framework that was intended for the hierarchical control of large-scale systems. In the 1960's and 1970's they still thought that sophisticated system models could be constructed, or understood. The practitioners voted against such illusions, and selected "postmodern" approaches (fuzzy and neural) instead (Chapter 12).

2. **... towards applying intuitions ...**
   When studying truly complex systems, no explicit models exist. The proponents of "system dynamics" assume that qualitative approaches suffice, forgetting about exact details and numbers, and still utilizing system theoretical tools. Simulation of more or less heuristically determined qualitative causality models is by no means mathematically well justified, but without such assumptions not very much useful is left that could be applied in practice (Chapter 13).

3. **... to reach a systemic view?**
   In *general system theory* the abstractions are brought to extremum — having no more concrete numerical models available one just has to manipulate symbols, the methods being too abstract to be of any real use, and one easily ends up doing mere philosophy. However, the system theoretic approaches may still offer holistic ideas that are not bound to today's practices. And, perhaps, from the system theoretic considerations one can complete the spiral back to hard mathematics.

   Can there exist a systemic view of complexity? How to see complex processes in a perspective? How the emergent patterns could be controlled, and how to make something interesting emerge out from the simulations? What kind of "holistic mathematics" might be available when analyzing complex systems? Specifically, how to restrict the power of tools so that they remain on the boundary between analyzability and non-analyzability? Indeed, such questions *can* be studied. For example, it seems that the idea of one single complexity theory has to be abandoned — perhaps the *theory itself* has to be fractal: To make something interesting emerge from the mindless simulations, the domain-specific semantics has to be coupled in the system structure. The structures also differ in different application domains. This kind of observations only make the questions more interesting and challenging: The area of complexity research cannot be exhausted by the pioneers alone. For example, most probably there will exist separate research areas like "complex cognitive system theory" and "complex automation system theory", where the ways how emergent phenomena pop up differ from each other. Correspondingly, the appropriate mathematical tools are different, and the new intuitions and tools are different. The last chapter hopefully gives some insight on what this all really means.