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Models and Simulations in the Historical Emergence of the Science of Complexity

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Summary. As brightly shown by Mainzer [24], the science of complexity has many distinct origins in many disciplines. Those various origins has led to "an interdisciplinary methodology to explain the emergence of certain macroscopic phenomena via the nonlinear interactions of microscopic elements" (ibid.). This paper suggests that the parallel and strong expansion of modeling and simulation - especially after the Second World War and the subsequent development of computers - is a rationale which also can be counted as an explanation of this emergence. With the benefit of hindsight, one can find three periods in the methodologies of modeling in the empirical sciences: 1st the simple modeling of the simple, 2nd the simple modeling of the complex, 3rd the complex modeling and simulation of the complex. Our main thesis is that the current spreading (since the 90's) of complex computer simulations of systems of models (where a simulation is no more a step by step calculus of a unique logico-mathematical model) is another promising dimension of the science of complexity. Following this claim, we propose to distinguish three different types of computer simulations in the context of complex systems' modeling. Finally, we show that these types of simulations lead to three different types of weak emergence, too.

Key words: complexity, model, computer simulation, numerical simulation, algorithmic simulation, software-based simulation, weak emergence

1 Introduction

As brightly shown by Mainzer [24], the science of complexity has many origins in many disciplines. Whereas the definitions of complexity are numerous and did not lead to any general agreement, it is most of the time accepted that systems are complex when they are at least composed of a certain amount

of entities (elements or agents) interacting together. But it is not sufficient. Another granted condition is that the complexity of the system specifically comes from the 'kind' of interaction at stake and/or from the 'kind' of its results. Here begins a typical problem of vagueness in specification for such a 'kind'. Does the complexity come from the incompressibility of the algorithm of interactions? Or from the emergent phenomena resulting from these interactions? But according to which definition of emergence? The interaction at stake seems to be of the 'kind' which precisely cannot be captured by a single and simple concept otherwise complex interactions could be generically expressed, even perhaps formalized and unambiguously computed.

My aim here is not to give the last word on this topic. But it is to show that - parallel to evolutions in mathematical approaches - recent evolutions in methodologies of modeling and simulation of complex systems have allowed the asking of such seminal questions from a different and somewhat more distinguishing point of view. My particular thesis is that the current spreading of complex computer simulations of systems of models (where a simulation is no more a step by step calculus of a single logico-mathematical model) is another promising dimension - of course not the only one - , of the science of complexity. Although it has during years been condemned as self-contradictory or sterile, the development of complex modeling and complex simulation of complex systems seems to become a real interesting challenge in many areas of science. Why? What has changed in the applied epistemologies of the modelers of complex systems? And for which reason?

To understand this shift in the recent applied epistemologies, it is necessary to characterize the kinds of relations models and simulations had with complex systems in the past. In a first part, after many authors, I briefly recall how and why simple modeling of simple systems can be seen as one of the major factors that made the success of modern science. In a second one, I suggest that the science of complexity became a foremost subject of concern essentially when it became reasonable to hope for some mathematical and (relatively) simple modelings of complex systems. In a third part, I suggest that the complex modeling and simulation of complex systems were no more seen as pitfalls in the 90's because they began to be used to formulate, replicate and disentangle simultaneously many different 'kinds' of interactions and causal intrications in complex systems. In this context, I propose to distinguish three types of computer simulations. In a fourth part, I use this distinction to differentiate *three types of weak emergence* in the case of the modeling and simulation of complex systems.

Finally, I propose that we recognize the real variety of computer simulations, and that we particularly distinguish the one I call *pluriformalized simulations* from *algorithmic simulations* and from *numerical simulations*. Those distinctions help to further explain some recent contributions of simulations to the

science of complexity. And, through that, they help to clarify a bit the debated notion of emergence.

2 Simple Models of Simple Systems

Of course, nothing is absolutely simple in itself. And all the meanings of the terms 'simple' or 'complex' in this paper have to be thought of with a connotation of relativity. As already pointed out by Mainzer [24], in the ancient Greece, celestial movements were believed to follow simple geometric laws (i.e. geometric figures constructed with compass and ruler), whereas sublunar phenomena were thought to be very complex. Although we cannot much further define the term 'complex' (which is anachronic in this context), it remains possible for us to say that *relatively* to sublunar phenomena, supralunar ones were seen to be much less attainable to simple representation or modeling. Note that this was a coherent claim in the doctrine of Aristotle, for instance, because sublunar phenomena were subject to generation and corruption. And, because of this specificity, contrary to the celestial substances, their essences could not be grasped in an everlasting, simple and perfect (i.e. achieved) geometrical law.

Not only the notion of 'complexity' but also the notion of 'model' is questionable when referring to this historical context. Nevertheless, it seems rather acceptable too when we recall the brilliant study of Pierre Duhem entitled after a famous word of Simplicius (6th century AD)"To save the phenomena" [12]. In this book, Duhem shows that many philosophers of the late antiquity had newly insisted upon the distinction between the work of an astronomer and the one of a physicist. According to Posidonius, Simplicius or Proclus, for instance, the physicist is concerned with the very essence of the celestial entities whereas the astronomer has only to "save the phenomena", i.e. to speak about the apparent figures, sizes and distances of celestial entities. Being limited to these considerations, it is understandable that his objects of study really match the discourses of arithmetic and geometry.

Put in another way: those late antique philosophers reverse the traditional arguments of the ancient philosophers to explain the same thing, i.e. the relative good matching between celestial movements and geometrical discourses or figures. For the latter, this was due to the capture of the perfect essence of celestial beings through mathematics. For the former, on the contrary, this matching was due to the fact that the astronomer, unlike the physicist, limits his work to the salvation of the pure appearances. In 1908, Duhem is keen on exhuming those troubling arguments as he is convinced that such a phenomenological epistemology for the physical theory is the good one and that it must prevail: generally speaking, scientists shall not try, nor hope, to represent what surpasses the human being. This is the reason why, according to Duhem,

a good law ought to remain mathematical and ought not to be represented through a physical model as Maxwell or Boltzmann erroneously tried to do. In the rest of his book, Duhem shows that modern science was born not in the neoplatonist works of Kepler or Galileo but rather in those earlier times of the 14th century when nominalist philosophers decided that the sublunar world itself was as complex (hence only mathematically salvable) as the late antique philosophers finally say the celestial world was. According to Nicholas of Cusa (1404-1464), for instance, the sublunar world was no more seen as "unachieved" (i.e. "imperfect" for the ancient Greeks) but, more positively, as infinite because it was seen to inherit a kind of infinity and complexity from its divine creator. So physicists, as astronomers, had to renounce to seek something more than a salvation of phenomena. They were told to limit themselves to fictitious essences and hypothetical causes (ibid.: 68).

Through this historical and epistemological contribution published in 1908, Duhem fights against the then traditional essentialist interpretation of the laws of the mechanics. Doing this, he lays a foundation for a neo-positivist reinterpretation of all the physics which will be influent, particularly through the contemporary works of the philosopher of science Quine. The traditional essentialist interpretation says, on the contrary, that the laws of mechanics are simple mathematical laws because the phenomena are *simple in themselves*. In fact, pace Duhem, the predominance of such a thesis from 1687 (the year of publication of the masterwork of Newton) to the end of the 19th century was confirmed at length, for instance, by the more recent studies of the historian of science Alistair Crombie [8]. In fact, Crombie recalls that, during this period, most of the physicists admitted that, with his laws, Newton had provided both an explanation and a prediction of the gravitation. They admitted that these laws had captured in a simple way the simple essence of the mechanical phenomena. But Duhem had a strong religious conviction which obliged him to recognize the infinite complexity of the creation, be it sub- or supra-lunar. Simultaneously, he felt that he had to put the stress on a pure mathematical presentation of laws and that, consequently, he had to fight against the British physicists of his time who used physical models to assess the laws of electromagnetism.

It is noteworthy that most of the historical accounts of the science of complexity stress on the traditional essentialist interpretation. By quoting Duhem here, my aim is not to contest this point. What I want to suggest is that when I say that in the times of modern science "we modeled simply the simple", it can be put in this more correct form: we made mathematico-physical theories to explain simply what was generally thought to be simple in itself. Contrasting with this view, Duhem had in fact a modelist and positivist interpretation of physical theories, although he rejected the physical modeling for the mathematical laws. So, let's say that the dynamical 'model' of Newton was 'simple'

and had a 'simple' target system.

On the one hand, the simplicity of the model came from the fact that Newton (along with Leibniz) had constructed well-fitted and well-designed mathematics (techniques of notation and techniques of computation) at the same time. The simplicity of the model resides in the efficiency of its notation and in the ease of the symbolic manipulation, combination and resolution it permits. On the other hand, the simplicity of the target system came from three main ontological hypotheses: supposedly, there was only one framework of space and time in nature; all the phenomena in this framework were thought to be of the same mechanical kind (one type of underlying causes); any dynamical systems were (or could be) isolated from any other. The simplicity of the target system (TG) resides in a representation of the world which entails ubiquity, genericity and separability.

3 Simple Models of Complex Systems

The science of complexity has known multiple births during the 20th century. It has known many historical reconstructions too. But it is only in the 1980's that it began to be a well recognized academic domain. At that time, interdisciplinary research programs became more systematic. There are many reasons for this long maturation. According to me, one of the main factors is quite prosaic. But simultaneously, as we will see, this factor reveals a relative continuity in epistemological positions, contrasting with what is often said about the paradigm shift from simplicity to complexity. This factor is the brutal and large diffusion of PC's in all types of labs all over the world in those years.

Let's recall that, in 1892, Poincaré showed that easily writable non-linear hamiltonian equations could lead to chaotic behavior. Doing this, he did not show that the world is complex neither that the solar system really is chaotic[1]. But he introduced a decisive split between the *attributes* of the different properties of the Hamiltonian formalism. Such a formalism possesses at least three distinct properties: (1) it is a notation; (2) it enables symbolic manipulation and combination; (3) it leads to formalized solutions. Poincaré shows that a particular mathematical formalism that possesses the two properties to be a notation and a working symbolic system (which can each be attributed a kind of simplicity), does not necessarily possess the (simple) property to give simple result of its combinations.

So as to facilitate the comparative exposition, let's define now a 3 dimensional vector called 'F' (for 'Formalism' or 'Properties of the formalism'). Its three

[1] This will be demonstrated only in 1989 by J. Laskar, with the aid of a computer and the "pursuit lemma"

dimensions represent the *degrees of simplicity attributed* to each of the three main properties of the formalism F, namely: notation, combination, solution. Let's assume that these degrees can only be of the two extreme kinds: S (for simple) and C (for complex). Beware that this notation has no pretention to be neither exact nor comprehensive. It is only an *illustrative notation* which will be useful for the clarity of some of my arguments.

Hence, the claim about the formalism used by Poincaré can be represented by the complexity vector:

$$F(S, S, C)$$

Roughly speaking, the possibility to lead always with ease to a simple solution is denied. I.e. the Hamiltonian formalism reveals a kind of complexity in *itself*. But it affects only one of its properties. As it presents two other properties to which simplicity still can be attributed, such a formalism can still be put in the set of the 'simple' ones. Following this major advance of Poincaré, researches in the mathematics of dynamic systems may focus on the internal relations between those apparently contradictory *attributes of properties* of some family of formalisms. In this context, one of the key questions is this one: how is it possible for a same formalism to be at the same time simple and complex?

Another distinct (and more recent) question is this one: what are the relations between such a complexity of the formalisms and the complexity that can be detected or measured in experimental works in physics or chemistry?[2] To what extent are those complexities of properties of different entities (a formalism and a real system) of the 'same' nature? Contrary to the first one, these last two questions are not only on complexity but on the *external validity* of the model of complexity for real systems (such as chaotic models of turbulent fluids).

When studying the spreading of models of deterministic chaos in theoretical ecology, the historian of science Deléage [9] has shown that the most impressive dimensions of the work of Robert May in the 70's was the fact that he gave a simple and "unified formalism" to explain both the stochastic variations and the cyclic oscillations of populations. Note that, in this particular case, because of the remaining simplicity of their notation (and not of their resolution), the non-linear equations of the theoretical model of chaos could serve as a theoretical argument against any further complexification in the notation of ecological models. Why complexifying the notation when a simple notation seems to give a quite realistic (complex) appearance to its numerically computed results? The complexity of real systems seemed to be sufficiently captured and/or represented by the internal relation between the attributes of the different properties of the model. So, the epistemological position of

[2] For this question, see [3]

Robert May was rather conservative from this standpoint. More generally, we can say that, in a first period, the shift to chaos models in many disciplines remains in fact in the continuity of the traditional hope to capture and reproduce in a simple way (at least at the level of the notation) what is complex in reality. So, these approaches mostly consist of confronting "simple models (at least at the level of notation) to complex reality".

Indeed, the schools working on dissipative systems or on systems far from equilibrium (Prigogine, Nicholis, Kauffman [27]) and on synergetics (Haken [16]) are fully aware of the non validity of some hypotheses of modern sciences concerning the real systems. They consider that most of the systems are open, not closed nor separable. But, similarly, they are still in search of the simplest and most generic mathematical or symbolic notation for a wide range of complex systems.

See, for instance, Kauffman:

> "If all properties of living systems depend on every detail of their structure and logic, if organisms are arbitrary widgets inside arbitrary contraptions all the way down, then the epistemological problems that confronts us in attempting to understand the wonder of the biosphere will be vast. If, instead, core phenomena of the deepest importance do not depend on all the details, then we can hope to find beautiful and deep theories." [22].

In another context, see Jensen on self-organized criticality (SOC):

> "The paper by Bak, Tang, and Wiesenfeld (1987) [Phys. Rev. Lett., 59: 381] contained the hypothesis that, indeed, systems consisting of many interacting constituents may exhibit some general characteristic behavior. The seductive claim was that, under very general conditions, dynamical systems organize themselves into a state with a complex but rather general structure. The systems are complex in the sense that no single characteristic event size exists: there is not just one time and one length scale that controls the temporal evolution of these systems. Although the dynamical response of the systems is complex, the simplifying aspect is that the statistical properties are described by simple power laws." [21].

Jensen again, in his chapter 5 entitled 'The Search for a Formalism':

> "One wants a model with sufficient structure to contain nonobvious behavior, but the model should not be so complicated that analytic approach cannot be carried out." [21]

The search for "general laws" or for "simplifying aspects" is then often *a search for an homogenization of the degrees of simplicity within the formalization* once it has be shown to have some realistic acceptable results. That is:

physicists or theoretical biologists most of the time want to build a formalism $F2(S,S,S)$ from a formalism $F1(S,S,C)$ or $F1(S,C,C)$.

We could object that the works on Cellular Automata ([34], [33], [37]) remains very empirical. It is true. Their theoretical "empiricity" comes from the format of the complexity vector of their formalism which is not $F(S,S,C)$ but $F(S,C,C)$. I.e. not only the resolution is not simple but the combination is not too, and both have to be assisted by computers. In the case of the SOC, Jensen proposed to build an approximate $F2(S,S,S)$ (with the hypotheses of a Mean Field Theory) using the work on cellular automata made by Bak and his colleagues and which was first published in a $F1(S,C,C)$ form.

Specifically, researches on CAs were innovating at their beginning in that they contrasted with the classical numerical simulations of manually intractable analytical models of the form $F(S,S,C)$. But, the *trend to model simply the complex* goes back in this field too, when some research programs finally - and very interestingly - seek for some generic CAs, i.e. CAs simulating back analytical results. Wolfram [37], for instance, is seeking a move $F1(S,C,C) \rightarrow F2(S,S,S)$. Similarly, people working on Artificial Life (Santa Fe) finally often look for some similar equivalence with cases of physical phase transitions so as to make possible the applications of subsuming models which would be simpler from a combinatorial point of view. This would be a move of this kind: $F1(S,C,C) \rightarrow F2(S,S,C)$ or $F1(C,C,C) \rightarrow F2(S,S,C)$. This second situation happens when we are in front of rich formal agents with 'complex' - i.e. non-trivial - notations instead of simple CAs.

For all those reasons, the emergence of the science of complexity has given a central role to formal modeling. And, in conformity either with a positivist and ontologically parsimonious epistemology (devoted to Ockham's razor) or with an essentialist one seeking great transdisciplinary, theoretical and simple principles, the quest for a remaining dimension of simplicity was not abandoned. Hence, most of the first approaches in the science of complexity have been guided by the quest of simple modeling, i.e. of *modeling with at least simple notations*. From this epistemological viewpoint (be it positivist or essentialist), the complexity of the model is accepted. But it has to reside in its treatment, not in its notation.

This is the very reason why most of the important roots of the science of complexity can be found in the pre-computer era. But this is the reason why too the entrance of this science in the computer era - and particularly in the PCs' era - has led to rapid and new convergences and - globally - to a noteworthy academic reinforcement. The famous tale about Edward Lorenz (1963) gives us a key to understand this point. With the aid of the computer, he rediscovers quasi-empirically the sensitivity to initial conditions of non-linear equations (with an additional important factor of dissipativity). But by doing this, he

was entering a kingdom of a new and fertile ambiguity. He first thought he was ordering the computer to treat numerically some supposed analytical model of the kind $F(S, S, C)$. But, by visualizing the results, he couldn't be very sure of the epistemic status of what he was doing: wasn't he doing a *virtual experiment* on a stylization of the target system $TG(NL, D)$, i.e. a Target System with Non-Linear causal relations and with Dissipativity? What was in view: the complexity of the model or the complexity of the stylized fact?

With this example, we understand that before the use of the computer as a plain simulator, the exploration of the internal relations between simple properties and complex properties of formalisms of the type $F(S, S, C)$ could easily be dissociated from the question of the empirical matching between the complexity of a $TG(NL, D, SO, SOC...)$ (where NL = Non-linearity, D = Dissipativity, SO = Self-Organization, SOC = Self-Organized Criticality...) and the kind of complexity which appeared by making use of such a $F(S, S, C)$. But, the more recent development of computer-aided simulations - in parallel with more classic computer-aided resolutions of mathematical models - has led to a perplexity. On the one hand, it has led to a significant facilitation for direct applications of the concepts of the science of complexity (such as in CAs or in Multi-Agent Systems). But it has caused a reconfiguration of the problem of the relations between the simple and the complex. These relations were not only thought *within* the properties of formalisms but within the different uses of the formalisms (be they computer-aided or not). Consequently, the till then constant idea that simplicity must remain a *minima* an inner attribute of the model seen as a system of notation directly speaking to our mind (so that we "understand" the laws of nature) begins to be questioned ([6], [7]).

4 Models and Complex Simulations of Complex Systems

But what is a model?

According to a characterization due to Minsky,

> "To an observer B, an object $A*$ is a model of an object A to the extent that B can use $A*$ to answer questions that interest him about A." [25]

This minimal characterization is interesting because it adopts a pragmatic approach. In particular, it does not make use of the notion of representation. So, it lets open the question whether a useful model is a true or approximate representation of the target system.

The main characteristics of a model are (1) its objectivity, i.e. its ontological independency compared to analogy; (2) the relativity (to an observer) of its property to model the TG (target system); (3) the property of the modeling

relation to be interpreted in terms of a simplicity in the asking and answering some questions about the TG (compared to a direct questioning of the TG).

Let's focus on this simplicity in the questioning and answering. As the diversity of the practices of modeling shows, it can have many interpretations. Among them are the following:

1. Simplicity in the reproduction of an observable behavior: models of prediction, operational models...
2. Simplicity of an experimenting on the model instead of an experimenting on the TG (due to time, space, technical, material or deontological limits)
3. Simplicity in the answering a question about some main causal factors: explanative models.
4. Simplicity in the providing of an easy global understanding (for the human mind): theoretical functional models. According to many theoretical biologists, such a simplicity necessarily entails beauty, deepness and ... truth. See Kauffman, *supra*.
5. Simplicity in the testing of the coherence or realizability of a formal theory: models in logic, mathematics or theoretical physics.
6. Etc.

In particular, a formal model should be characterized as a formal construct possessing a kind of simplicity either in its notational power (simplicity of symbols, unity, formal homogeneity) or in its combinatorial power or in its ability to lead to a solution. This simplicity is chosen so as to satisfy a specific request: prediction, explanation, decision ...

Note that the characterization of Minsky entails that such simplicity, whatever its nature, remains relative to the observer. But the question is: where are the limits of the observer and the limits of the observed object? Where is their mutual frontier? Hence, to whom or to what this simplicity has to be relative? Is this simplicity relative to the system 'human programmer and observer + computer' or to the system 'human mathematician and observer'? We have to be aware that, at this level, different answers lead to different epistemological choices. A formalism appearing as a $F(S, S, S)$ or $F(C, S, S)$ [in the case of a continuous formalism to be implemented in a discrete fashion] to the former can appear as a $F(S, S, C)$ to the latter.

A similar problem is at stake in a rather popular question among the epistemologists of computer simulations: is a computer simulation an "extended observation" (Humphreys [20]) or only a "conceptual argument" (Stöckler [32])? The main argument of Humphreys lies on the fact that computers are kinds of instruments which are very similar to material ones. Whereas, according to Stöckler, any use of a computer is founded on systematic and deterministic uses of pure symbols (at least at the level of the machine language). It implies

that a fictitious human mind, with only more time, attention and patience than ours, could always deduce by itself the result of any computer simulation. Whereas it is at the material level that a pure difference of degree prevails for Humphreys, it is at the symbolic level that such a difference prevails for Stöckler. So, let's have a more precise look at computer simulations.

What is a computer simulation?

It is often said that "a simulation is a model in time". Hence, there seems to be no computer simulation without a unique underlying model. In 1996, Stephan Hartmann follows this widely accepted characterization:

> "Simulations are closely related to dynamic models [i.e. models with assumptions about the time-evolution of the system] ... More concretely, a simulation results when the equations of the underlying dynamic model are solved. This model is designed to imitate the time evolution of a real system. To put it another way, a simulation imitates a process by another process." [17].

In this view, the imitating process is always supposed to be a delegation to the computer of the complicated combination of symbols of some model of the type $F(S, S, C)$ or $F(S, C, C)$ which would lead to its resolution. But it has been shown ([35], [36]) that this dynamic imitation is not fundamental in every Computer Simulation (CS). Some CSs are imitative (compared to an eventual formal calculus of the model or to the dynamic of the TG itself) in their results but not in their dynamic. If, for instance, the CS uses a numerical trick - such as fictitious finite elements - to solve an analytical model, each phase of the step-by-step computation hasn't to be realistic or imitating in itself. Similarly, Varenne [35] has shown that there are simulations of the growth of botanical plants which are imitating the process of the real plants at the end of each computation, in its result, but not during the computation itself.

That is the reason why I propose to characterize CS in another way [35]. In particular, it can be useful to avoid the term "model" because it is no more obvious that every CS is based on a unique model. Today, we can observe that many CSs are rather based on *systems of models*, and sometimes on *complex systems of models*. If we do not confound the algorithm and the models at stake in a computer program dedicated to a CS, it is necessary to avow the spreading of CSs using, at the same time, a *multiplicity of models*. As it is visible through some contributions to this book, some CSs of complex systems tend to be more *software-based* (such as agent-based CSs) than directly *model-based* as in the classical fashion (mathematical model + computer-aided numerical simulation). Note that we can hardly say that our times see the shift from simple models to complex ones. The current doubtless move toward a complexification of formalisms impinges more directly on CSs than on mod-

els. It is sometimes said that "computational models" become more and more complex. But it is not a very different claim from mine. Because what is called "computational models" are in fact *software based systems of models* which are built to enable complex CSs.

Of course, software-based CSs are no panaceas. But they are chosen in some scientific fields (especially in biology and social sciences) because they enable to insert a certain degree of *heterogeneity* in the various models scientists aim to implement. I call *pluriformalization* those CSs that enable the coexistence and co-calculation of a multiplicity of formalisms [35]. Pluriformalized complex CSs enable to build multiaspectual and/or multiscale systems of formalisms. The notation's properties of their formalisms are much less constraining than the ones of traditional mathematical or physico-mathematical models. Beware that the system of notation can become complex in itself. But it is its ability to denote directly (without intermediate fictions) some trivial or well-known (in a scientific domain) or commonly-recognized (in the opinion, in the collective representations ...) things that is facilitated. This compromise is obvious in the definitions and the uses of multi-agent systems in social sciences. Consequently, as far as a characterization of CSs is concerned, I recommend focusing more directly 1st on the relation between symbols inside a CS and 2nd between the symbols of the CS and the symbols of the associated model(s).

So, let's say that a CS is minimally characterized by a *general strategy of symbolization* taking the form of at least one step by step treatment. This step by step treatment possesses at least two *major phases*:

- 1st phase: a certain amount of *operations running on symbolic entities (taken as such) which are supposed to denote* either real or fictional entities, reified rules, global phenomena, etc.;
- 2nd phase: any observation or any measure or mathematical or computational *re-use* (e.g.: the simulated "data" are taken as real data for a model or for another simulation, etc.) of the result of this amount of operations *taken as given* through a visualizing display or a statistical treatment or any kind of external or internal evaluations.

CSs are not always supposed to be iconic modeling, i.e. similar representations of systems. For instance, in the case of a *numerical* CS of a mathematical model, the symbolic entities denote fictitious entities. They really are taken as denoting entities, i.e. as real symbols. But they are particular denoting entities as they are denoting nothing really existing in the TG. As shown by eminent philosophers of language and science (Russell among others), these cases are not invalidating in ordinary language. For instance, it is meaningful to say that "Santa Claus does not exist". By doing this, we use a symbolic notation ('Santa Claus') which denotes nothing really existing. But it does not prevent the whole sentence from having a meaning and, even, from being true. Similarly, in a numerical CS, what is important is the phase of aggregation

and of measure (2nd phase) of the effects of this aggregation: the symbols are reified and treated as *relative subsymbols*.

It is in the context of the second AI that Smolensky [31] coined the term *subsymbol*. By this term, he denoted the entities processing in a connectionist network at a lower level and which aggregation can be called a symbol at an upper level. Subsymbols are *constituents of symbols: "they participate in numerical - not symbolic - computation"* [31]. Berkeley [5] has shown that Smolensky's notion can be interpreted from a relativistic point of view. This is this *relativity of the symbolic power of symbols in CSs* I want to express through my relativistic connotation of the term. In the second phase of any CS, aggregations of symbols are taken as subsymbols.

A *numerical CS* depends heavily on a model and can be seen as a direct subsymbolization of this model. For instance, a continuous model is treated by a strategy of discretization. Hence discretization is a subsymbolization of the previous explicit model. On the contrary, the kind of CS I propose to call *algorithmic* [35] is directly based on iterative and uniform rules. It can be for instance simple CAs, formal grammars, rewriting formal systems, L-systems ... Their denotational power is often emphasized because such CSs use symbols that tend to denote directly those rules that are supposed to really exist in the TG. For instance, CSs of biological morphogenesis which are based on L-systems are seen as algorithmic ones (see [28]). Note that, contrary to some computer scientists, many botanists think that such rules are fictitious entities too. They claim that they are more similar to computational tricks than to symbols really denoting something in the TG. We see that the denotational power of the symbols at stake is relative. It depends on the ontological commitment of the scientific field.

Finally, *Software-based CSs* are different from *numerical* ones and from *algorithmic* ones. Numerical CSs are most of the time designed to treat a previous intractable model of the type $F(S, S, C)$. The discretization of the notation of the model enables to smooth the difficulty of the resolution through its transformation into an iterative combination. The numerical CS has this kind of effect: $F1(S, S, C) \rightarrow F2(S, C, S)$. With the play on the non-denotational symbols, the difficulty of resolution is transformed in a difficulty (in terms of amount) of combinations and iterations. *Algorithmic or rule based CSs* tend to treat directly formalisms of the type $F(S, C, C)$. Most of the time (but there are exceptions: especially those CAs that are built to simulate other formalisms), their performance can be more easily interpreted as an iconic modeling and as a quasi-real experimenting on the TG because the denotational power of the symbols plays a major role.

Software-based CSs operate on systems of models. According to my notation, a system of models can be presented as a vector of formalisms (or a matrix of

properties) having different attributes of simplicity for each of their properties. For instance, as in [11], a multileveled CS in population ecology can intricate (1) some solvable differential equations working at each step at the level of the population $F1(S, S, S)$, (2) some models of real space or social space in a graph or network formalism $F2(S, S, C)$ and (3) some models of agents possessing a complexity of combination (in terms of diversity and variability) because they are cognitive agents or relatively complex reactive ones $F3(S, C, C)$. It can be represented as follows:

$$[F1(S, S, S), F2(S, S, C), F3(S, C, C), ...]$$

The relations between the ease of notation and the difficulty to iterate are more difficult to understand in this case. The main result of these analyzes is that such a CS is no trivial numerical treatment of a model. This vector of formalism (or matrix of properties) cannot be easily reduced to or transformed into a huge formalism having its own homogeneity, simplicity or complexity properties. So, most of times, such CSs have first to be experimented: through analyses of sensibility, of robustness, etc. Hence, beside more traditional and direct mathematical researches on complex formalism, more and more modelers (working often in applied sciences) are seeking models of such complex software-based simulations ([11], [13], [18], [19]). Significantly, the effort to standardize and re-homogenize multimodels in industry had stemmed from the same previous and necessary movement of complexification, i.e. of integration and co-calculation of heterogeneous models [38].

5 Types of Simulations and Types of Emergence

As we will briefly see now, the ability to take into account some kind of emergence is a key issue for any specific standpoint on the science of complexity, particularly when we focus on CSs. But there are many definitions of emergence (see [10]).

Following Bedau [1] and [2], we can define three kinds of emergence:

1. An emergence is *nominal* when "macro-level emergent phenomena are dependent on micro-level phenomena in the straightforward sense that wholes are dependent on their constituents, and emergent phenomena are autonomous from underlying phenomena in the straightforward sense that emergent properties do not apply to the underlying entities." [2]
2. An emergence is *strong* when, contrary to what happens in nominal emergence, emergent properties have some irreducible causal power on the underlying entities. In this context, "macro causal powers have effects on both macro and micro-levels, and macro to micro effects are termed downward causation." [2]

3. Finally, according to Bedau, *weak emergence* is a kind of *nominal emergence*. To explain it, he quotes Herbert Simon: "given the properties of the parts and the law of their interactions, it is not a trivial matter to infer the properties of the whole." [30]. Weakly emergent phenomena are those which are not easy to explain and which essentially need simulation to arise. So, all kinds of weak emergence are based on the property of "underivability without simulation". Assuming a system S, a microdynamic D on this system (*"which governs the time evolution of S's microstates"*), the locality of D (i.e. *"microstate of a given part of the system at a given time is a result of the microstates of 'nearby' parts of the system at preceding times"*), Bedau states that: *"Macrostate P of S with microdynamic D is weakly emergent iff P can be derived from D and S's external conditions but only by simulation."* [1]

According to Bedau, science is only concerned with weak emergence (WE). This question is of course still discussed. I won't enter this debate here. My aim in this section is not to state a general thesis on emergence but, more restrictively, to specify the definition of *weak emergence* by Bedau in relation with my definitions of CSs. Bedau claims that "simulation" is central for this scientific notion of emergence. But he does not give many details on what is to be understood by a simulation. So his definition can be completed.

The definition of *weak emergence* is particularly revealing when we see the importance of "simulation" and when we keep in mind what I have said of the relation between models, systems of models and their CSs. As we have seen, this relation most of the time is a delegation, removal or displacement of some degrees of complexity from a property of the initial systems of symbols to another property of another systems of symbols. As far as a formalization is concerned (and not directly the TG), we can see some kind of emergence precisely in the experience of an *obstacle* or a *difficulty* or a *non-triviality*. Simon said *"given the properties of the parts and the law of their interactions, it is not a trivial matter to infer the properties of the whole"*. We could adapt this claim to the properties of formalisms: *"given the properties of the parts [the property of the notation of symbols] and the law of their interactions [the ease of their combination], it is not a trivial matter to infer the properties of the whole [the difficulty to infer solutions]."* [30].

So, according to Bedau (following the general idea of Simon), weak emergence arises when we have $F1(S, S, C)$ and when we transform it, through simulation, in a $F2(S, S, S)$. My claim is that the variety of types and uses of CSs enable a further analysis of what is at stake in weak emergence (WE).

Bedau says that WE is founded on a CS, whatever its nature. But he seems to say too that we have WE only when we make the computer transform a $F1(S, S, C)$ into a $F2(S, S, S)$. To avoid such a risk of erroneous restriction of

his definition of WE, we can say that we have WEs of at least three types. A WE of the first type (WE1) occurs when we make the computer transform a $F1(S, S, C)$ into a $F2(S, S, S)$. A WE of a second type (WE2) occurs when we make the computer transform a $F1(S, C, C)$ - e.g. multi-agents systems with complex cognitive agents - into a $F2(S, S, S)$ or when we only experiment on it. A WE of a third type (WE3) occurs when we make the computer experiment on a dynamic matrix of formalisms $[F1(S, S, S), F2(S, S, C), F3(S, C, C)...]$ (such as in pluriformalized CSs). As we have seen, in this case, it is still possible - but not necessary - to intend to simplify the computation into a calculable model of the type $Fn(S, S, S)$.

So, distinguishing between three major types of simulation has given us the opportunity to distinguish between three *types of weak emergence*. The main differences here stem from the differences between the kinds of obstacle - or difficulty or non-triviality - when facing the problem of inference or computation. In the case of Software-Based CSs, the difficulty of computation is not of the same nature than the one at stake in the case of numerical simulation. So, *the type of emergence cannot be exactly the same*. For the former, it is a problem of co-calculation of heterogeneous models. For the latter, it is a more classical problem of the effective computation of a previous analytical model.

6 Conclusion

I cannot enter here in more details about the differences between a CS and a model, in particular concerning their *routes of reference* [14] and the *entanglement* of these routes in the case of pluriformalized CSs. Let's keep in mind that the recent appearance of a variety of CSs has led to a multiplication of splits, of new frontiers between what is considered simple and what is considered complex. This frontier can be seen either only within the formalism, between the attributes of its properties, or within the different uses of a computer to treat a model or a system of models. So, the properties of models and the possibility to attribute to them the 'simple' or 'complex' predicate have been diversified and extended due to the intensive use of computers in the modeling techniques and, in particular, due to the spreading of object-oriented programming techniques.

More generally, what makes a system of models work? Most of the time, it is due to a massive discretization of parameters, variables, time and space, amount of populations, etc. In my opinion, the reason why this strategy of discretizing is a good one does not lie upon the metaphysical fact that nature really is discrete or is a computer. More pragmatically (from the standpoint of the management of formalisms), discretization works because it causes multiple and heterogeneous occasions of choices in computations (such as in Discrete Event CS or Individual-Based modeling techniques ; see [15]). It causes

decisive new degrees of freedom in a CS, especially when you see it more as an intensive computer-aided supervision of formalisms. It causes a multiplicity of instant of choices which enable a possibly constant change (1) of the status of symbols at stake in the denotational hierarchy (either subsymbolizing, denotating or exemplifying), (2) of the ways those formalisms denote each other during the CS (for the sake of a tractable co-calculation), (3) of the ways those formalisms punctually denote levels or parts of the TG (for the sake of the external validity of the resulting CS).

Above all, the multiplication of the types of CSs has given birth to new ways of creating weak emergence with computers. Of course, the multiplication of the variety of emergence which can be simulated on computers does not solve the problem of the external validity of these new formalizations of emergence. Is the emergence in a complex CS an adequate model of a real emergence in the target system? Such a question, especially when asked at this level of generality, remains open. But, this diversification makes the models and simulations of emergence more accommodating and less constraining as diverse ways of reproducing different kinds of emergence possibly occurring in real systems become available.

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