Published in Simulation in Industry, Proc. of the 13th European Simulation Symposium, Marseille, France, October 18-20th, 2001, ed. by N. Giambiasi and C. Frydman, SCS Europe Bvba, Ghent, 2001, pp. 549-554.

What does a computer simulation prove ?

The case of plant modeling at CIRAD (France)

Franck Varenne - Ph.D. student
Centre Inter-universitaire d'Histoire des Sciences et des Techniques - University of Lyon 2
Personal address: 187 Avenue du Maine - 75014 PARIS - FRANCE
E-mail: fvarenne@wanadoo.fr - Phone: (33) 45 40 38 72

KEYWORDS
Environmental science, Forestry, Plant modeling, Model credibility, Discrete simulation, Philosophy.

ABSTRACT
The credibility of digital computer simulations has always been a problem. Today, through the debate on verification and validation, it has become a key issue. I will review the existing theses on that question. I will show that, due to the role of epistemological beliefs in science, no general agreement can be found on this matter. Hence, the complexity of the construction of sciences must be acknowledged. I illustrate these claims with a recent historical example. Finally I temperate this diversity by insisting on recent trends in environmental sciences and in industrial sciences.

INTRODUCTION

A Fully Scientific Question, Which May Well Interest Historians and Philosophers of Sciences

It could seem extremely presumptuous for an historian of contemporary sciences to ask the question: *Are computer simulations genuine experiments or simple arguments in a theoretical discourse?* If an answer exists, it belongs to the present and forthcoming scientists but neither to philosophers nor to historians. That is the reason why my aim here is neither to give any authoritative view, nor to evaluate any scientific results in this field: I would just like to depict the content of my latest feelings about simulation in today's sciences. So I begin with three remarks that could legitimate a philosophical approach upon this scientific question.

First, when I was a student of physics, I once faced this sometimes forgotten property of finite elements methods to simulate. Although my colleagues and I knew another but explicit analytical solution of our problem - in atomic physics -, the step-by-step computer calculated solution was better than the analytical one! Of course, there was no mystery there: We understood that the truncation caused by our method "simulated" the noise really existing in the experiment. The noise had been neglected in the analytical approach. Through this incident, it became crucial for me to understand the real value of simulation in sciences. I did not know what conclusion to draw: Was the computer simulation a real experiment that could sometimes falsify a theory? As a scientist, I did not know what to think about this. So I decided to work on history of contemporary sciences. This paper gives several of my results in this epistemological field.

Second, when you read contemporary scientists, you may find all possible theses on the real status of computer simulation in scientific research. Today, it seems that nobody refuses to use simulation, but no general agreement exists among scientists about the real power of demonstration of this both intellectual and practical technique. Some scientists say that a computer simulation is a genuine experiment whereas some others definitely refuse this assimilation and see it simply as another numerical technique, i.e. as another intellectual technique. These obvious contradictions about the value of a technical proof are also disconcerting when you try, as an historian, to understand the progress of knowledge in sciences. Inasmuch as this problem is still a present controversy in sciences, it seems almost impossible to clarify it only by reading and listening to scientists.

Third, the fact is that a historian of recent sciences cannot wait for the time to decide and has to solve this difficulty for himself. Over the past three decades, computer simulation has spread through all the different sciences: History went on, whatever our ability to understand it. And it became more and more difficult for historians to find out an intrigue in order to understand the recent advancement of science. We miss a clear conception of the driving forces for this history. Up to the 1950's, both theory and experiment were roughly seen as dialectically correlated activities. For instance, the French epistemologist Bachelard wrote in 1951 (Bachelard 1951): "The scientific culture is driven by a subtle dialectic, which constantly goes from theory to experiment, and then goes back from experiment to the fundamental organization of principles." But nowadays theory and experiment do not play exactly the same demonstrative roles, as another way of learning things - from nature itself or from our concepts alone, that is the problem…- seems to have emerged near them, namely computer simulation. What results from those three remarks is that not just scientists and philosophers but also historians are faced with this crucial question of computer simulation status.

The Existing Literature

From an historical point of view, the question of computer simulation credibility is a very old one. Since the first texts of the Society for Computer Simulation, this question periodically reappears (McLeod 1980a) (McLeod 1980b) (McLeod 1986) (SCS Technical Committees 1979) (Schruben 1980). In these papers, it is regularly assumed that the problem of credibility comes first from a lack of communication between the simulationist and the expert in the field to simulate, and second, from the youth of statistical techniques. As a result, although the awareness of the difficulties is real, conclusions are nevertheless optimistic because the authors argue that these defaults will probably vanish with time. But, more recently, the same problem re-emerged in quite different terms through a general reflection on the modeling methodology. Although it was already sometimes expressed this way since the 1960's in economics and operational research (Naylor 1966), simulation models are more and more asked to be credible thanks to verification, validation and accreditation procedures, especially in environmental modeling (Hill

1995) (Hill 1996) (Shannon 1998). The same old problem reappears here in that no general procedures seem to exist. And, unlike a few decades ago, the old saying, which describes computer simulation more as an art than as a science, does not seem funny any more.

How To Proceed

In the fist section, my thesis is that many opinions may coexist on this matter. Different epistemological implicit beliefs may justify the use of computer simulation, only for different reasons. In this sense, I make an analytical review of the existing theses, classifying them, and I try to reveal the corresponding epistemological beliefs at stake. In the second section, I illustrate this alleged variety of standpoints on a recent historical example: The debate on plant modeling between the *Centre International de Recherche en Agronomie pour le Développement* (CIRAD) – which is a semi-public research center in agronomy, working toward developing countries - and the *Institut National de la Recherche Agronomique* (INRA) - the French public institute in Agronomy - which took place in France from 1990 to 1993. Through this example, I show my second thesis according to which our answer cannot be general and must not be reduced to the alternative certified/uncertified model, because different standpoints on the simulation status may alternately prevail in a given field, depending on the different epistemological beliefs at stake. Finally and subsequently, I propose to refer this complex situation to the history of formal mathematics and proof theories in computer science, which dates back to the 1930's.

AN ANALYTICAL REVIEW

Contradictory And Disordered Answers, Which Nevertheless Can Be Classified

What is surprising about our question is that the various answers people gave to it do not correspond with the traditional partitions between scientists, philosophers, sociologists and historians. On the contrary, for each particular standpoint, you may find either a scientist and a philosopher or a scientist and a sociologist and so on. This question is a very frequent one but the specific answers and arguments in each case could be common to different approaches of science. The only existence of such various and disordered opinions shows that, at present, the making of science slowly but unquestionably changes its nature and that we still are not prepared to properly think this evolution. So I tried to find out an order among this apparent confusion by isolating the hypotheses behind each common standpoint. My first thesis in this article is that every standpoint on the value of computer simulation in science could be classified and referred to one of only three major categories of arguments: Either you see computer simulation as a kind of experiment, or you see it as a simple intellectual tool or you consider it as a third real and new means of learning things just between theory and experiment. To justify this thesis, what I suggest is to deduce *a priori* these three possible standpoints from a few definitions and then to illustrate each of them by particular existing theses in the literature.

So let us begin with a few definitions. Of course, every definition can be discussed but my aim here is only to strengthen our present ability to read and compare the various arguments on this problem, and not to say the last words on theory, experiment and models.

A Few Definitions:

The terms "computer simulation" may denote quite different things. Pritsker made an inventory and found 21 different definitions (Pritsker 1979). But we can discern three major categories of meanings for this expression, from the largest to the narrowest. In its largest meaning, a computer simulation is the "use of computers to model things" (McLeod 1986). Let us call it "S1". A second meaning - "S2" -, which is more restricting, refers to computer simulation as any computer treatment of either a mathematical model without analytical solution or a rules based inference motor - like cellular automata, multi-agent systems or object oriented modeling. The emphasis here is on the discretization and the step-by-step resolution. So, neither a computer resolution of a logico-mathematical model with an analytical solution in a closed form nor a computer-aided formal calculus is a simulation, in this more precise meaning. These are nothing but calculations or demonstrations. Moreover, according to S2, it is not necessary to handle a model to simulate: A rules based inference motor cannot properly be seen as a model (Franc 1996). You could object that there exist simulation models (Hill 1995). But here the term "model" does not refer to a traditional mathematical model but either to the set of rules or to the simulation result itself. At last, in its narrowest sense - "S3" -, computer simulation may only denote the use of stochastic elements in a step-by-step computer work. S3 is then a reduction of S2 to the Monte-Carlo methods.

A computable theory is a system of knowledge - axioms, assumptions, correspondence rules, etc. - with computable formal links between its elements, i.e. from which we know there always exists a finite procedure to calculate a resulting value from another one at an arbitrary given accuracy, such as the formal link "√" or "cos" in the equations $x = \sqrt{a}$ or $x = \cos(a)$. In fact, here we have the right to use abbreviating notations in order to denote computation procedures because there are known to be tractable. At last, an experiment is the knowledge we get from the reaction of a thing to a precise and partially controlled or known stimulation. Hence a thing is a being, which teaches us something through an experiment.

Three Possible Standpoints On the Status Of Simulation

Now, here are the various standpoints we can find:

First, if you consider that the very essence of a "thing" is to be something, which, for any reason and whatever its mode of existence - a material object, a social fact, etc. -, resists to our mental attempt to think it completely (Durkheim 1895), then you probably will find that a computer simulation is an experiment. And you will agree with the Artificial Life research program hypothesis: Computer simulation - in the sense of S2 - is a genuine experiment. To show this, you most likely will lay stress on our mind's impossibility to deal with complex pluridimensional representations, then on the allegedly really existing formal basis of the life phenomenon whatever its substrate, and finally on the intractable emergent behaviors of coexistent simple beings (Langton 1987). If you don't accept this extreme position, but you think that the object you study is of discrete nature, such as those manipulated by digital computers, and obey stochastic laws such as computer generated pseudo-random numbers, you nonetheless will see computer simulation - in the sense of S3 - as a kind of experiment. As you depict nature as discrete and stochastic, you consequently argue that your simulation model is objective as it is a real replica of nature, but you perhaps will add that it cannot exactly be seen as a genuine experiment, since the scale of the represented phenomenon in your model is not the same as in nature. So did Von Neumann and Ulam, when they served as nuclear physicists and developed the Monte-Carlo method in touch with "discrete problems" of nuclear disintegration (Galison 1997). But so did the geneticist Kimura too, in the 1960's, when he was also working on discrete objects, namely genes (Dietrich 1996).

Second, if you add to the definition of "thing" the property to be always such as natural, and then, because of this, infinitely profound and always partially opaque to our mind, with the consequence that, on the contrary, all artificial beings are of finite nature and can or could be controlled by our mind, you will probably think that computer simulations - in the sense of S1 - are only tools. From this point on, because you insist on the artificial and then virtually transparent essence of computer simulations, contradicting the obscurity of natural objects, either you see simulations - in the sense of S1 - as practical tools, such as a hammer, whose role is to help theorizing by treating experimental data or preparing real experiments (Legay 1997), or - in the specific sense of S2 - as theoretical tools, by giving them the role to assist human mind in time-consuming logico-numerical computations and subsequently to produce theoretical arguments (Hartmann 1995) (Dennett 1995) (Stöckler 2000) or opaque thought experiments (Di Paolo 2000). You also can argue, like many statisticians, that a Monte-Carlo simulation - S3 - is only a model sampling and, therefore, that digital computers simulations did not fundamentally change the nature of this old numerical practice (Marshall 1954).

Third, if you prefer to dwell on the fact that simulation, in spite of its artificiality, nevertheless can surprise the mind of its programmer, arguing that the limitation theorems of mathematics on computability enable this phenomenon, you probably are willing to consider simulation - in the sense of S2 - as a new and intermediate source of knowledge, just between theory and experiment (Humphreys 1990) (Rohrlich 1990) (Bedau 98) (Thompson 1999) (Parrochia 2000). To show this, you will argue that there exists some experimental mathematics, which, through Monte-Carlo methods, for instance, can help producing conjectures, or, else, that there exists "numerical experiments" based on the ergodicity theorems or on the pursuit lemma (Laskar 1989) (Ekeland 1995). You can argue too that simulation sometimes can falsify a theory but also can be falsified by a real experiment (Wagensberg 1985). Or, by insisting on sociological points, you can picture it as a way to make science in a new "trading zone", between pure theory and experimentation (Galison 1996).

So these various standpoints can be summarized:

I- *Thesis 1* : A computer simulation is an experiment.
- I-1- A genuine experiment : Artificial life (Langton 89).
- I-2- A kind of experiment : A simulation imitates the granularity of nature: See Von Neumann, Ulam and Kimura (Galison 96) (Dietrich 96).

II- *Thesis II* : A computer simulation is only a tool.
- II-1- A tool to treat real experiments (Legay 97).
- II-2- A theoretical tool.
  - II-2-1- A numerical method among others, according to statisticians (Marshall 54).
  - II-2-2- A conceptual argument (Hartmann 95) (Dennett 95) (Stöckler 00).
  - II-2-3- An opaque thought experiment (Di Paolo 00)

III- *Thesis III* : A computer simulation is an intermediate between theory and experiment.
- III-1- A new means of capturing and understanding complexity without comprehending it (Wagensberg 85)
- III-2- A step-by-step computation is an *a priori* experiment (Laskar 89) (Humphreys 90) (Rohrlich 90) (Ekeland 95) (Bedau 98) (Thompson 99) (Parrochia 00).
- III-3- Simulation is a "trading zone" between theorists and experimenters (Galison 96) (Galison 1997).

Of course, this classification surely is not complete. It is not a closed system. The deductive approach is artificial and must not be taken as the only one. But this kind of conceptual reconstruction (Lakatos 1978) is quite revealing and suggestive. Many other combinations of thoughts and beliefs are certainly possible. But most of the already existing ones seem to enter this list.

The Impossibility To Answer Unilaterally

It comes out from this first study that, for each precise historical situation, there is a possibility to decide which categories of theses could be at stake in the scientific arguments. But it shows too that the philosopher or the historian has to be careful and humble as well, when facing this question "what does a computer simulation prove?" The second thesis of this article is that none of the three categories of arguments could be applied to contemporary sciences in general, whatever their objects, their methods and the moment of their history we consider. None of these three categories could be considered as the only true one. We cannot have a general point of view on the value of computer simulations, because of the different implications and meanings of mathematics in the different fields of science, and because of the various philosophies of nature at stake. This fact remains true for a given field throughout its own history, because the role of mathematics and the definition of the studied object evolve: You cannot find a unique and stable value that would be given to its simulation uses once for all. Again and hopefully, this thesis illustrates the fact that it does not belong to the historian to decide on the value of computer simulation in a given field but to the scientists themselves. These preliminary reflections prove the importance to investigate the intellectual history of contemporary sciences and not only their sociological construction nor their philosophical general insights. To substantiate these considerations, I will report the debate on modeling between CIRAD and INRA.

A RECENT EXAMPLE: THE DEBATE ON PLANT MODELING BETWEEN CIRAD AND INRA (90-93)

Here comes the helpfulness of a precise historical case, which is significant because it shows controversies and changes among scientists about the status of simulation. This case is significant too because simulation emerged quite late in the field of plant modeling. To simulate was not as obvious to people in this field as it could have been in nuclear physics, hydrodynamics, or even economics, sociology and psychology. But today agronomists nonetheless announce that "virtual agronomic experiments" very faithful to botany are available. How is it possible?

Some Hints On The History Of Individual Plant Simulation (1962-1990)

I will not write here the whole history of the De Reffye's work at CIRAD. It will be sufficient to note that the prehistory of individual plant simulation can be referred to the Ulam's digital computer simulations on branching patterns with the cellular automata, at the beginning of the 1960's. Then Lindenmayer's work on substitution formal systems - the so-called L-systems -, which were first published in 1968, helped some biologists to accept such a formal computer modeling. But unlike many theoretical biologists, most of physiologists and botanists saw this research as pure speculation as they perfectly knew the incredible complexity of the morphogenesis of a real plant: It could not be reduced to a formal grammar. For this reason, they rapidly - and for a long time - mistrusted computer simulation. De Reffye's work partially laid on this disenchantment toward the theoretical approaches to botany. And his chance was not to directly confront this mistrust as he was working as an agronomist. His approach was an agronomic one, hence pragmatic and empirical. Neither had it anything to do with the old traditional but sterile mathematical phyllotaxy. In 1979, he produced and published through his Ph.D. thesis the first universal 3D simulation of botanical plants. He could simulate them, whatever their "architectural model" in the sense of the botanist Hallé. It was a real simulation, in its narrower - S3 - meaning, unlike the first works of Lindenmayer, as it used stochastic modeling to represent the growth of meristems - buds -. This growth was treated meristem after meristem and obeyed some elementary laws of stochastic processes. The law parameters were directly measured on real trees. So De Reffye followed the then quite recent approach of operational research and produced one of the first individual based simulations in botany. Moreover, his "individuals" - the simulated buds - were faithful to botany and were treated individually, without appealing to any physiological and controversial details. From a technical point of view, that is the precise reason why his model was easy to calibrate and validate - such as an object oriented approach permits (Hill 1995). From a sociological point of view, through beautiful and faithful synthesized images, he increased the credibility of the simulation approach and convinced more and more botanists and agronomists. From a conceptual point of view, the new architectural vision, due to Hallé's work in the 1970's, enabled De Reffye to consider plants as discrete events generated discrete trees and not as chemical factories. This made possible the representation of their growth through a simulation, in the sense of S2 or, more precisely, S3.
During the 1980's, the work went on with Jaeger and Blaise, who were Ph.D. students of Françon, a Computer science professor, at the University of Strasbourg. Rapidly, it became visible that many new agronomic applications were possible. It seemed possible to predict the fruit or wood production of a tree. So the INRA too decided to play a role in this forefront of agronomic research in 1990.

The "Programmed Incentive Action" (AIP: 1991-1993)

In 1990, INRA managers proposed to organize with the CIRAD a three years long incentive action toward the INRA researchers, so that they can benefit from the new technique. In those times, INRA laboratories did not develop such models. This could sound very paradoxical because there were indeed many plant modelers at INRA who intensively used digital computers to handle their models. So when you take apart the natural jealousy between people, it is very enlightening to try to discern the reasons of the conceptual resistances toward this new computer use in agronomy, at the beginning of the 1990's.

The Epistemological Beliefs At Stake

The sources on this publicly muffled debate are (Bouchon 1995) (Franc 1996) (Bouchon 1997) (Legay 1997).
At INRA, modeling was a quite old tradition. But, in order to control the productivity of plantations, plants and trees were, most of the times, captured in aggregates, i.e. only at the level of a forest or a field. As ecophysiologists and agronomists were accustomed to see a plant as a chemical process, they found it obvious to use compartment modeling in a systemic presentation and treatment. The use of computers was then justified by the complexity of the calculations in retroaction loops. In this energetic and dialectic prevailing vision of life and matter, dating back to the 1930's Marxist thought in France, natural things were seen as infinitely profound and complex, unlike man-made objects. And consequently, computer was treated as a huge calculator and nothing more, i.e. as a numerical tool -in agreement with Thesis II -, whose role was only to calculate the solution of a model, this model being nothing more that an artifact, sometimes dangerously fascinating, but nonetheless totally controllable by man. Thus, most of the INRA researchers avoided using the word "simulation", although its larger sense - S1 – existed in the USA. The second obstacle to simulation in agronomy came from a great mistrust toward detailed models, at that time. In fact, in the post-war neo-Marxist epistemology, modeling was seen as a dangerous idealistic approach of sciences, quite near a religious one. Therefore, agronomists were told that an optimal model was always a minimal one and could not represent reality. On the contrary, De Reffye's simulation appeared too much ambitious, then very suspect. It was a kind of detailed descriptive meta-model, which could integrate any process based models. The third and last obstacle was a semi-technical one. As computers did not have enough power and memory until recent times, it was not only conceptually difficult to treat the trees as individuals in a forest, but technically impossible. First in the American forestry, and later in the French tropical agronomy, it became more and more obvious that there were emergent effects due to the variety and complexity of social relations between trees and between species in a forest, which were unpredictable within systemic global models: It became crucial to produce complex models, including spatial effects. But computer limits prevented this idea from emerging. So, during this transition period, there seemed to be no patent solutions, which could replace an ecophysiological approach. So why change one's habits?
But today this simulation approach is largely adopted, due to its ability to make quantitative predictions. It is now commanding many changes in the conceptual routines and

in the practical techniques, like data gathering. A mixed research unit in "bio-informatique" - biology through computer science - keeps developing the simulation model in Montpellier - France -, thanks to an association between the University of Montpellier, CIRAD, INRA and CNRS.

CONCLUSION

The Answer Depends On The Type Of Existence The Scientists Ascribe To Their Formal Tools

Following the changes of standpoints about simulation in a particular field helped us to understand that under this question - what does a computer simulation prove? -, an unquestioned philosophical problem is at stake: What kind of existence does the scientist ascribe to the mathematical or logical equivalent he is using to model his phenomenon? The answer not only relies on the nature of the model but also on the nature of a computation which may either be understood as a mental activity or as a practical activity. Here is the implicitly disputed point between Thesis I and Thesis II. Some people say the nature of a mental computation differs from the one of a computer computation. But is there a difference of nature or only a difference of degree between them? Is it only a new form of thinking – an "extra-mind" thinking - which emerges from the quantity, the speed and the parallelism of calculations inside the machine or is it a new thing in itself with its obscurity, its irreducibility, which emerges outside our mind? Can we compare this obscurity to the one of the materially existing things surrounding us? In fact, this question deals too with what could be called a "thing" and what could be called a theoretical representation: Is a representation of a step-by-step parallel or stochastic logical - perhaps non computable - computation in my mind a real theoretical concept which exists entirely in my mind and that I potentially control from my mind - such as the finite procedure "square root" or "cosine"? In this case, it is not a thing and I would say that a computer simulation is only an argument. Or is this representation only a nominal term that only denotes an external practical procedure, i.e. the potentiality to make the experiment on a computer? In this case, I would say that a computer simulation is a genuine experiment on the matter of things, although this matter is independent of any material substrate.

We now realize that the advancements in the mathematical thought during the 1930's tend to govern this underpinning debate. Through the development of computer sciences, the computability theorems have slowly but very crudely displaced the double question of the existence and application of formal concepts, on the one hand, and of the definition of a thing, on the other hand. And this evolution goes on differently through all the different sciences.

The Changes In The Status Of The Mathematical Object Showed The Limits Of Theorization

Hence, the third and last thesis in this article is that the practice of simulation in current environmental sciences nevertheless shows a quite general trend, which mainly depends on the recent history of mathematical thought: With the non-computability problem, the status of the mathematical object has changed. And the vision on the surrounding things has consequently changed too. Today, more and more scientists agree with the third category of arguments: According to them, including the specialists in ecology and life sciences, computer simulation must be considered as a third real and new means of getting insights, just between theory and experiment. This currently scattering opinion deals with a new operationalistic view of theorization: Not all theories are directly computable. And a computation itself is seen as an experiment inasmuch as we do not *a priori* know if and what result could emerge from its step-by-step operations. Practically, that seems to give the computation the nature of a thing, the nature of something existing separately, outside our mind. This opinion is reinforced by the frequent impossibility to verify complex programs. Today, the scientific work is all the more difficult, as the scientists first of all have to choose the proper type of mathematics to express their theory or model. And it appears to be a new but today unavoidable task for them to rationally justify this first choice each time. Because there seems to be no more obvious and unquestionable mathematical formulation or style to express a given phenomenon, unlike a few decades ago – the differential equations reign -, this justification task leads to a crucial micro-epistemological work in sciences.

A Glimpse Back To Von Neumann Once More

Finally, I would like to point out an amazing opinion among engineers about the use of computer simulation in industry - especially in aeronautics -: They are more and more convinced that in many cases, real experiments are superfluous. They think that a good simulation is far better than an experiment on a prototype - apart from the financial considerations. Indeed, when you read (Von Neumann 1951), you see that analog models are inferior to digital models because of the accuracy control limitations in the first ones. Following this argument, if you consider a prototype, or a real experiment in natural sciences, is it anything else than an analog model of itself? The test on the prototype is a real experiment. But is it something different and better than the handling of an analog model? So the possibilities to make sophisticated and accurate measures on this model - i.e. to make sophisticated real experiment - rapidly are decreasing, while your knowledge is increasing. These considerations are troublesome because it sounds as if nature was not a good model of itself and had to be replaced and simulated to be properly questioned and tested! It looks as if it was not possible any more to end a paper on simulation by reassuringly using the traditional word: "Simulation will never replace real experiments". However, I suggest one of the questions that could now be addressed: Is this new astonishing opinion still true for a natural object, or does it definitely possess the nature of a thing, which always has to behave like a mysterious and profound oracle through a real experiment?

Special thanks to G. Ramunni, P. Matarasso and the members of the MOTIVE seminar of the PEVS/CNRS.

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AUTHOR BIOGRAPHY

Franck Varenne was born in Brive, France, and went to the Upper High-School *Ecole Supérieure d'Electricité* - Supélec, Paris - where he graduated as an engineer in physics and electrical engineering. Then he received his Master of Arts - maîtrise - in Philosophy of the University of Paris 1 / Sorbonne and passed the public entry examination - CAPES – in Philosophy. He also passed a Master of Art - DEA - in Contemporary History at the University of Lyon 2. He is now teaching philosophy at the *Lycée Militaire de Saint-Cyr* and preparing a Ph.D. thesis in history of contemporary sciences on the following theme: "The history of modeling in sciences since the emergence of computers." He is member of the MOTIVE seminar of the PEVS, i.e. the Environment Program of CNRS.