History and Philosophy of Technoscience

FROM MODELS TO SIMULATIONS

Franck Varenne

ROUTLEDGE

From Models to Simulations

This book analyses the impact computerization has had on contemporary science and explains the origins, technical nature and epistemological consequences of the current decisive interplay between technology and science: an intertwining of formalism, computation, data acquisition, data and visualization and how these factors have led to the spread of simulation models since the 1950s.

Using historical, comparative and interpretative case studies from a range of disciplines, with a particular emphasis on the case of plant studies, the author shows how and why computers, data treatment devices and programming languages have occasioned a gradual but irresistible and massive shift from mathematical models to computer simulations.

Franck Varenne is Associate Professor of philosophy of science at the University of Rouen (Normandy – France) and associate researcher at IHPST (CNRS – Paris). His research focuses on the history and epistemology of formal models and computer simulations in contemporary science, especially in biology and geography. He has published around fifty-five articles and chapters. He has also published eight books and co-edited three collective books.

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French abbreviations

ADEME	(*Agence de l'Environnement et de la Maîtrise de l'Énergie*) – French environment and energy management agency
AIP	(*Action Incitative Programmée*) – a type of INRA management policy aimed at setting up collaborations (both internal and with external laboratories) and stimulating funding for projects
AMAP	(*Atelier de Modélisation de l'Architecture des Plantes*) – Plant architecture modelling workshop
ATP	(*Action Thématique Programmée*) – Scheduled research initiative
bac+8	In the 1970s, French university students were required to submit two theses: a post-graduate thesis (known in French as the "*troisième cycle*" or "*bac[calaureat] plus 8 [years]*"), which is equivalent to the present-day PhD, and a State thesis (called a "*thèse d'État*" or "*thèse d'habilitation*"), which was often written over a period of many years
Café, Cacao, Thé	(*Literally "Coffee, Cocoa, Tea"*) – IFCC Journal and also the name of an ORSTOM department
CIRAD	(*Centre de coopération internationale en recherche agronomique pour le développement*) – Agricultural Research Centre for International Development
CNRS	(*Centre nationale de la recherche scientifique*) – French National Centre for Scientific Research
DEA	(*Diplôme d'études approfondies*) – Diploma of advanced studies, comparable with a British Master's degree
DGRST	(*Délégation générale à la recherche scientifique et technique*) – General delegation for scientific and technical research
EFPA	(*Écologie des forêts, prairies et milieux aquatiques*) – Forest, Prairie and Aquatic Environments
ENGREF	(*École nationale du génie rural, des eaux et des forêts*) – French National School of Forestry

ENSAT	(*École Nationale Supérieure d'Agronomie*) – French Higher National Engineering School of Agronomy – a competitive-entry engineering institution
ENST	(*École Nationale des Télécommunications*) – French National School of Telecommunications
EPHE	(*École Pratique des Hautes Études*) – Practical School of Higher Studies
EPIC	(*Établissement Public à Caractère Industriel et Commercial*) – Public-Sector Industrial and Commercial Enterprise – a type of public body established by statute in France
GERDAT	From 1980–1984: (*Groupement d'étude et de recherche pour le développement de l'agronomie tropicale*) – Study and Research Group for the Development of Tropical Agronomy
GERDAT	From 1985: (*Gestion de la Recherche Documentaire et Appui Technique*) – Management of Documentary Research and Technical Support (as of 1985, GERDAT became part of CIRAD, retaining the same acronym but with this new name)
IFCC	(*Institut Français du Café, du Cacao et autres plantes stimulantes*) – French Institute of Coffee, Cocoa and other Stimulant crops (later renamed "IRCC")
IN2P3	(*Institut national de physique nucléaire et de physique des particules*) – French National Institute of Nuclear Physics and Particle Physics
INAPG	(*Institut National Agronomique Paris-Grignon (INA P-G)*) – French National Agronomic Institute, Paris-Grignon
INRA	(*Institut national de recherche agronomique*) – French National Institute for Agricultural Research
INRIA	(*Institut national de recherche en informatique et automatique*) – French National Institute for Research in Computer Science and Automation, now known as the *Institut national de recherche dédié au numérique* – French National Institute for Computer Science and Applied Mathematics.
IRCC	(*Institut de Recherche sur le Café, le Cacao et autres plantes stimulantes*) – Institute for Research on Coffee, Cocoa and other Stimulant Crops (see IFCC above)
IRD	(*Institut de Recherche pour le Développement*) – Research Institute for Development, previously called ORSTOM
LHA	(*Laboratoire de l'Horloge Atomique*) – Atomic Clock Laboratory of the CNRS
LIAMA	(*Laboratoire franco-chinois d'informatique, d'automatique et de mathématiques appliquées*) – Franco-Chinese Laboratory of Informatics, Automation and Applied Mathematics
METALAU	(*METhode, Algorithmes et Logiciels pour l'AUtomatique*) – Method, algorithms and software for automation

ORSC	(*Office de la recherche scientifique coloniale*) – Office of Colonial Scientific Research
ORSTOM	(*Office de la recherche scientifique et technique outre-mer*) – Office of Overseas Scientific and Technical Research
PIAF	(*Physique et Physiologie Intégratives de l'Arbre en environnement Fluctuant*) – Integrative physics and physiology of trees in fluctuating environments
PNTS	(*Programme national de télédétection spatiale*) – French National Programme for Space-based Remote Sensing
SESA	(*Société de services et des systèmes informatiques et automatiques*) – Software and Engineering for Systems and Automata
SYRTE	(*Systèmes de Référence Temps Espace*) – Time and Space Reference Systems
ULP	(*Université Louis Pasteur*) – University of Strasbourg
UMR	(*Unité Mixte de Recherche*) – Joint Research Centre
USTL	(*Université des sciences et technologies du Languedoc*) – University of Science and Technology of Languedoc
UTC	(*Université de Technologie de Compiègne*) – University of Technology of Compiegne
X-ENGREF	Engineer from the *École Polytechnique* who has completed post-graduate practical training or internship (*école d'application*) at the French National School of Forestry (ENGREF: *École nationale du génie rural, des eaux et des forêts*)

Introduction

Many philosophical articles or books on computer simulation begin with general definitions or explanations, and then choose two or three specific sub-domains of science – along with a very small number of selected publications – that illustrate and confirm their definitions and interpretations. As a result, although they may be accurate regarding the epistemological meaning of a given technical solution, they sometimes lack a certain sensitivity to real field solutions, to their multiplicity and to the dramatic epistemological innovations that emerge mainly from the field. Other books on history or sociology of science may be more aware of both the diversity of technical solutions and the importance of field innovations. However, since a significant number of these books are multi-author volumes, they simply juxtapose, or at best loosely compare, the many descriptions of different technical and epistemological solutions, and their comparisons are made between simulations of different target systems with overly disparate formalisms, and methodological and computational solutions that are too heterogeneous. For this reason, although these publications may be particularly informative, most are not ultimately conclusive from an epistemological standpoint. Nor can they guard us against a sense of general dissonance. With such approaches, the meaning of the term "simulation", or even its understandable polysemy, remains vague and somewhat disheartening.

Exceptions to these two frequent limitations of the current literature on computer simulations can be found in some works regarding simulation techniques in a specific domain of objects whose evolution is studied across a sufficient lapse of historical time. A brilliant example exists in nuclear physics, namely the work of Peter Galison.[1] Since the early 2000s, however, it has become clear that there is often a greater diversity of simulation techniques and consequently of epistemological innovation in the biological and social sciences – which are constantly developing new computer simulations – than there is in physics, in contrast to the general rule in the technosciences in the immediate post-war period. This book can be seen as an attempt to help fill the gap in this regard.

Starting from the undeniable achievements, as well as from the limitations, of the previous studies of many other researchers, and based on a longitudinal case study in quantitative, mathematical and computational biology, this book first adopts a historical and comparative approach to the different research programmes operating in the same field: *modelling the growth and morphogenesis of single* *vegetative plants in botany, forestry and agronomy*. Having chosen this relatively vast domain, along with these three different types of approach, and without neglecting the personal, social and institutional factors, this book's methodical approach is mainly based on an intellectual and comparative analysis of the different solutions to modelling and simulation issues both in the theoretical approaches to plant growth and in the more applied and technoscientific approaches that have emerged since the 1950s.

It is important to note that the content of this book is based not only on analyses and comparisons of publications (in English, German and French), but also on more than twenty interviews or personal correspondence with some of the key actors. Using a diachronic and comparative perspective, the book describes the exact field involved, as well as the technical and formal reasons and the epistemological decisions that explain why each kind of computer simulation of the various aspects of plants gradually replaced the mathematical models, i.e., the pre-existing models that originated from theoretical biology, biometry or morphometrics. It is hoped that, as a consequence, this book will give the reader the epistemological and conceptual acuity that seems necessary today to avoid many interpretative confusions: namely the confusions between quantification and formal modelling, between laws and models, between models and simulations, between mathematical models, computational models and simulation models, between simulations of models and models of simulations, and, last but not least, between different types of computer simulations.

With the aim of presenting an updated and extended version, supplemented with more in-depth epistemological insights, this English translation includes several additions to the original introduction and conclusion, as well as to a number of the chapters. Chapter 8, entitled "Twenty-one functions of models and three types of simulation – classifications and applications", is entirely new, however. This chapter's aim is first of all to present a distinctive general classification of the epistemic functions of scientific models, as well as a classification of the different types of computer simulation. This approach is intended to remain very general in scope, in the hope that it will thus benefit research on models and simulations in completely different fields from those of plants or biology. Its content is the result of a work of comparison and induction carried out not only on the basis of the comparative history of plant models presented herein, but also on the basis of several collaborative research efforts that have been carried out since then, as well as on my own, even more recent, large-scale research in the field of comparative history of models and simulations in geography.[2] Next, with the two-fold aim of confirming the relevance of these conceptual analyses based on the available evidence on the one hand, and, on the other, of reviewing the comparative history recounted in Chapters 1 to 7 from a more discerning and discriminating epistemological perspective, Chapter 8 will end with a systematic application of these classifications to the different types of models and simulations encountered in the case of plants.

It may be remarked that a fairly substantial portion of this book focuses on French research and researchers, leading to the conclusion that this reflects an unjustified bias. With regard to plant studies, however, there are certain situations specific to

France, such as the enduring existence of French research institutions in previously colonized tropical countries such as Côte d'Ivoire (see Chapter 3), even long after these countries obtained political independence. Such situations played a large part in the dynamics and focus of the research reported here insofar as they enabled quantitative botanists to have very early and direct access to the huge diversity of tropical flora, while, at the same time, providing them with access to adequate instrumentation. As a counterbalance to what may potentially be perceived as a French-oriented bias, however, I also describe in detail, in Chapter 1, how and why Jack B. Fisher, together with Hisao Honda, were among the first botanists to attempt to tackle the problem of using computers to faithfully represent the growth and architecture of vegetative plants. I also explain why, as a perhaps too rigorous botanist, Fisher ultimately decided not to develop his simulations further. It is no accident that Fisher also worked in a quasi-tropical context, in the Fairchild Tropical Garden of Miami; like the researchers in Côte d'Ivoire, he was also exposed to the incentive of maximal diversity. The fact remains, however, that for a long time, apart from some tropicalists such as Fisher, most of the researchers in quantitative botany and forestry working in North America and Great Britain remained in the mainstream of classical mathematical modelling. Important exceptions can be found in Canada, in the Prusinkiewicz school in particular, and also – from the 1990s onwards – in Germany and Finland. I have also been careful to include these exceptions and their specific "pre-histories" in Chapters 2 and 7 in particular.

The period of history involved here covers the end of the 1960s up to the first few years of the 21st century. This period is, of course, not without antecedents. This work does not aim to dwell in detail on the periods that preceded it, but, in order to better understand its specific technical and epistemological aspects, and especially what I propose to identify as a transition "from mathematical model to software-based simulation", I consider it necessary to give a preliminary outline in this introduction of the way in which the formal models took root and were originally grasped and used in the study of plant growth.[3]

Thus, when we examine the period prior to the one we will study – the period from the 1920s to the beginning of the 1960s – we can see two different epochs emerge fairly clearly. The first corresponds to the years before the spread of the digital computer. It extends from the 1920s to the end of the 1940s. In those years, mathematical modelling permeated several sectors of biology. Briefly put, it had a two-fold effect of increasing the available types of formalisms and of diversifying the epistemic functions of the mathematical formalizations, in contrast to the usual functions attributed to the mathematical laws and theories traditionally used in biology. A second, much shorter epoch stretched from the beginning of the 1950s to the mid-1960s. This was the epoch of the first impacts of computerization on formal models, and it included, in particular, the appearance of the first computer simulation techniques. These techniques would interfere in both a competing and a constructive manner with the formal modelling practices that were then still flourishing. Over the next few paragraphs I will describe these two epochs in somewhat greater detail.

With regard to the first epoch, what the scientists often called the "formal-model method" became progressively established in the quantitative biology of morphogenesis, based on four different areas: biometry; population biology; mathematical biology; and biocybernetics. The formal-model method, in biometry in particular, had its roots in the epistemological decision of Ronald A. Fisher[4] to abandon the Bayesian interpretation of statistics and instead to propose – in line with the theory of errors that had emerged from works on astronomical observation, and in the wake of the famous article by Student[5] on probable error in the estimation of a mean – a "hypothetical law" for estimating statistical parameters in the case of small sample sizes. In my view, this hypothetical law, which was explicitly free of any attempt at representation and thus of rootedness in the actual causal connections, acted as the first detached formalization, or first formal model in the full sense of the term as it is used in biology. The "hypothetical law" itself took the form of a frame of reference for field data, and was widely termed "model" from the end of the 1940s. It was this fictive and detached formalization that would to a large extent serve as a prototype for the other types of formal model in biology, including in population biology, from the 1920s onward. This first epoch may be called the "epoch of detachment of formalisms", since it is characterized by an increasing and normalized use of this type of formal construct, known as formal models.

In this context, by "formal model" I am referring to any type of formal construct of a logical or mathematical format with an axiomatic homogeneity that is capable of answering certain questions and fulfilling certain functions (cognitive, empirical, communication-related) with respect to an object, a system or an observable phenomenon. The formal model differs from theory in its validity, which is often only local, in its prior adaptation to certain questions that are posed at the outset, and in its inability to directly produce general results in the form of theorems. It should be pointed out already here that it also differs from simulation, although the term "simulation" is ambiguous, since it designates both a symbol-processing operation and the symbolic result of that processing. I will revisit all these points in greater detail in Chapter 8. We could say that, as a first approach, a computer simulation – insofar as it is a process – may be seen as a computer-assisted symbolization and formalization technique consisting of two distinct steps. During the first step, termed *operative*, symbols that more or less realistically represent elements of an actual or fictive target system interact step by step in accordance with rules, and these rules themselves may represent certain real or fictive mechanisms of the target system. Adopting a term used in connectionist artificial intelligence, I consider that this step is based on a *sub-symbolic*[6] use of certain formalisms and certain systems of symbols. The second step of a simulation consists of an equally symbolic processing of the results of the first step. This step may be described as *observational*, and it consists of a set of *reckonings*, *measurements*, *observations* or *visualizations* regarding the outcome of the first step. The main epistemic function of the computer simulations that were initially the most widespread, i.e., "numerical simulations", was to replace impossible formal calculations with *measurements carried out on these interaction results*. Thus, these interactions did indeed take place between symbols that had also been given the status of sub-symbol: they were sub-symbols from the point of view of the resulting patterns. Not all the computer simulations still have the primary function of replacing an analytically intractable calculation, but all retain this two-step structuration. One consequence of this structuration is that, as we will see in this historical and comparative case study, even though a computer simulation uses formal models, unlike those models it is not always a homogeneous formal construct. For that reason, a simulation does not necessarily have to be based on a single selective viewpoint on the target system, and nor is it obliged to have a formal homogeneity because of its format. This point will form one of the main established facts of this investigation, and I will return to it in more detail, giving specific examples.

First, let us return to the characterization of the formal-model method in the empirical sciences, and to its innovative nature in biology in the first epoch, starting in the 1920s. It should be noted that – although mechanical models, in the sense still used by William Thomson (Lord Kelvin), Maxwell or Boltzmann in the 19th century, responded to a demand for *visualization of calculations, or picturability*, and although a formal model in that part of mathematics known as the mathematical theory of models was itself still considered to be a more concrete, albeit mathematical, representation of a purely formal theory – it was no longer the model's concrete and representable nature or its ability to interpret a theory that were sought from the 1920s onwards in the "formal-model method". Instead, what was sought was an ability to directly and formally represent certain relationships between observable properties or physical quantities, if necessary in a way that remained purely phenomenological, i.e., precisely without representing a credible underlying mechanism, but also without interpreting a formal theory that had been explained in advance. As a result, the *formal model* tended to be a direct formalization that was no longer based exclusively either on a prior physical model or on a more abstract theory. This type of epistemic function was new for models in the empirical sciences. By virtue of the model's henceforth formal nature, and owing to this new function it was given, the model may seem to conflict with the nature and the epistemic functions of the traditional formal laws. Nevertheless, it was still recognized as a model and not as a law: these two characteristics – its local validity and the fact that the justification for its construction is based on a particular question and a precise perspective – are still used to differentiate between the scope and function specific to a formal model and those specific to a mathematical law.

Thus, because of the specific different epistemic functions now given to models, and because of the correlative divergences in terms of fieldwork epistemology, this first epoch, which saw the emergence of the model method, is characterized by a general renewal of the legitimization of formalisms in the life sciences, in particular for studying morphogenesis: the formalisms became more varied once they were no longer necessarily determined by representations resulting solely from physics or theories that could be completely and formally mathematized. The legitimization of these formalisms could itself become more varied. The term "model"[7] becomes more frequent in the literature, and then systematic. In this, mathematics first played a purely pragmatic role as a tool for data investigation or data representation, combined with an epistemology favouring detached formalizations and pluralism, an epistemology that was in fact often fictionalistic and instrumentalistic. Parallel to this effect favouring a pluralistic epistemology of formal models in applied biology, mathematics still played a major role for the most speculative of the bio-mathematicians in their theoretical-mathematical models. This was no longer a role of symbolic replication of entities and elementary mechanisms (entity realism), however, but rather a role as a means of revealing the directly mathematical-type stable structures (structural realism). It is the recognition of the latter epistemic function that emerges, for example, in the transition from biophysics to biotopology that the biomathematician Nicholas Rashevsky first invoked in 1953.[8] Thus, not only with regard to the rise of descriptive mathematics in applied biology, but also with respect to the most theoretical works, mathematical ingenuity was directed at what I have called a *detachment of formalisms*. To that extent, this mathematical ingenuity would partially replace the models and metaphors that traditionally derived from physics and its related disciplines.

Let us turn now to the second epoch, which precedes our own and extends from the end of the 1940s to the early 1960s. This epoch has a series of features that I will sum up briefly. First of all, owing to the availability of digital computers, *digital simulation* developed very early on alongside the formal models, but in an equally polymorphic manner. A different hurdle was cleared from the path of formal modelling for plant morphogenesis with each of the contributions from the new authors – all of whom were mathematicians and not biologists. Alan Turing (1952) emphasized the contribution of the discretization of formalisms. Murray Eden (1960) highlighted the need to formalize real random events with simulated random events through "stochastic" models based on the laws of probability. Lastly, Stanislaw Ulam (1962) demonstrated the importance of the spatialization of formalisms in order to formalize spatial phenomena.[9] This would mark the beginning of cellular automata. In this context, computer simulation proved from the start to be a formalization strategy that operated on a lower level of abstraction than classic formal modelling, with a complicated manual processing that was offset by a massively iterative processing delegated to the machine. It is in fact in this sense that computer simulation relies on a sub-symbolic use of the usual sets of axioms. The formal models in a computer simulation are not calculated formally by the computer thanks to deductive rules; instead it is their axiomatic functioning that is simulated by the sub-symbolic representations, which in turn possess their own set of rules and axioms. These other sets of rules and axioms are at times – but not always – of a more immediately interpretable nature, as is the case, for example, of discrete representations that use one-to-one relations between single-memory addresses in the computer and neutrons in the first computerized nuclear physics.

Until the beginning of the 1960s, however, each of these digital simulations of growing living beings, by selectively sub-symbolizing a formal representation, extended the power of expression of the formal model in accordance with a maximum of one or two dimensions that had until then been inaccessible to mathematics. Each of these simulations thus gave rise to just one selective digital representation. Moreover, none of these simulations could be fed precise field data, which would have enabled an effective calibration to be carried out. All of the results of these digital simulation processes thus remained merely qualitative models. From this point of view, these simulation processes were ultimately comparable to the contemporaneous theoretical-mathematical models that, in their turn, still sought to explain by invoking a single fundamental or predominant mechanism, such as those of biophysics, biotopology, relational biology, differential topology or plant-structure thermodynamics.[10] In this context, a computer that simulates remains an unrefined and purely qualitative simulator. It produces graphs or curves that admittedly bring to mind the shapes found in nature. But this similarity remains purely qualitative. Thus, whether it is a case of formal models or of those first computer simulations, formal multiplicity and diverging formal solutions remain the rule. It is the divergence and dispersion of mere intention, of speculation and of selective mathematical actions without a grip on the world of real plants.

As for field modelling, such as the modelling used during this second epoch in agronomy and forestry – the very modelling that was most expected to have a grasp on reality – formal divergence and diversity were in fact its method, its *credo*. For multifactor experimental designs applied to increase in biomass, for improvements in crop management, for problems of blight control, the biometric models of plant growth worked very well. They were designed to do so. Nonetheless, despite all this newly available formal diversity, these models failed when it came to focusing on monitoring of morphogenesis on the scale of the individual plant. As a result of this failure, they ultimately rather glaringly revealed the unavoidably perspectivist and selective nature of the formal model. The problem was, so they said, that the properties of a living organism could not all be formalized *at the same time*. But what may seem here to be a defect of the model, field biometrics often decides to interpret as a quality of nature, as proof that we are indeed dealing with nature, in its infinite complexity. These formal field models are selective in their perspective. What is more, they are mutually exclusive: nothing could be more normal, as we often read in the scientific literature itself, than this fruitful tension between representation and action. Beyond certain cultural differences, an epistemology of a pragmatic type that is adopted principally in the English-speaking countries due to the overwhelming influence of nominalism and of pragmatist philosophies may, strangely enough but very significantly, harmonize on certain points with a dialectic-type epistemology that is more specifically adopted on the European continent, and in particular in France, due to the persistent influence in this context of Hegelian rationalism and dialectic materialism. During this epoch, these two epistemologies, which were otherwise so distinct, could thus be seen to confirm each other's intuitions, since both claimed that it was necessary to renounce the aim of simultaneously representing the infinite multiplicity of dimensions of the object under study. Both claimed that it was necessary to try to offset this impossibility by a multiplicity of formal and selective modelling approaches to that object.[11] It is true that these formal approaches remain mutually incompatible, because they are axiomatically not co-calculable. As a result, they can only be juxtaposed but not aggregated. We may pass from one to the other, but they are never aggregated with each other.

As for mathematical models with a theoretical function, those who are dedicated to these models in theoretical biomathematics may lament their diversity, while at the same time nonetheless also contributing to increasing this diversity. Thus they seek to make them not exclusive, but rather mutually absorbing, since in that way they can demonstrate that they are capitalizing on earlier works and that they are doing better than them. The metaphor I am suggesting here is that of absorption: this is the direct opposite of the metaphor of aggregation that applies for integrative simulations. I would say that a theoretical-mathematical model is *absorbent* because it is conceived to replace and emulate one or several other models, while at the same time bringing its own epistemic contribution. It emulates other models in the sense that it seeks to be more general by dispensing with the explicit formulation of the preceding theoretical-mathematical model, but fulfilling almost the same epistemic functions of comprehension – and sometimes of partial prediction – as the previous one while adding several other functions of its own. From this point of view, the formalisms of theoretical biology are in competition with each other for theoretical dominance. They neither accept nor seek a peaceful juxtaposition. They seek to reduce each other in the secret hope that there will remain only one at the end: this is the process of absorption. But any contemporary historian of science can nonetheless see that biomathematics fails to propose a final, convincing absorption, namely a comprehensive general theory of morphogenesis and growth that would be based, for example, on information, entropy, the mathematical theory of catastrophes, on fractals, or indeed on a general theory of signals or networks. The result of this relative failure is that, rather ironically, and even tragically from their point of view, these theoretical models actually become very different also in the scientific literature. In these multiple works of resistance to multiplicity, to perspectivist and pragmatist modelling, as well as to the dispersion of detached field models, the search for a unique and monoformalized theoretical model – i.e., one that is formalized in only one sole mathematical set of axioms – plays the role of substitute for the lost and seemingly direct rooting of the old models in the physical world.

This second epoch therefore is characterized, on the one side, by a calm acceptance of the mutual incompatibility of models as long as they promote human action, and on the other by an uneasy rejection of that dispersion because it heralds a loss of meaning, in particular for those who disagree with pragmatism or dialectic rationalism. This, then, is the portrait of an epoch that, for other equally fundamental reasons (such as the changing social demands with regard to science in the post-war period, the recognized limitations of the capabilities of instruments and formalisms, the changing objects of study), with relative coherence developed its own consensual epistemology of the plurality and dispersion of representations, ending with its later explicit affirmation during the 1980s in some research work and symposia on epistemology and science studies. In some ways, the movement towards a pluralization and dispersion of models that was specific to this second epoch is the same as what we are still witnessing today, in a large part of contemporary science. The epistemologies of the dispersion and disunity of science were able to come into being and find ways of justifying themselves during that epoch, in particular by exploiting the method of formal models and of correlative iconoclasm, or rejection of integral representation.

And yet, in the case of an object that is complex, because it is particularly composite, such as the plant for example, it turns out that monoformalized models, even when multiplied, or even when they have a statistical nature and only a pragmatic aim, are no more capable than monoformalized theories of providing predictive and effectively operational formalizations. And in the face of social demand, science has thus had to try to advance further still and circumvent this hurdle. This is essentially the reason why, as I will show more particularly in this work, from the mid-1970s onwards, botanists, agronomists, foresters and other plant specialists all turned towards *integrative software-based computer simulation*[12] based on an individual-based approach, since, thanks to the visualization devices and object-based computer languages, such simulation permitted the convergence of perspectives, scales and mechanisms, and thus of multiple formalisms. I will demonstrate that *software-based simulation* thus brought about two fundamental innovations. First, simulation broke with the supremacy of formal models and their associated epistemologies, albeit without downgrading them entirely. Next, it broke with the numerical simulation that had emerged in the immediate post-war period and that was still dependent on mathematical models and the assistance they provided. Indeed, software-based simulation made it possible to achieve precise calibration and, in many cases, quantitative prediction, or even – which remains a heresy for many – an outright "experiment on simulation", also known as a "virtual experiment". Its essential principle, as we will see, is what I propose to call *pluri-formalization* or, in other words, a computer integration of formalisms of different natures (logical, mathematical) and from different points of view. This latest-generation simulation, far from being simply a discretization of models, takes a position that is at times in competition with models and mathematics, insofar as it makes something that is not compatible mathematically compatible on the level of the programming language and of the computer program. This, to my mind, seems to be its most decisive contribution since the beginning of the 1990s. Its truly empirical nature obviously remains in question, and we will see this in detail during the investigation, and also in the conclusion, in the form of a comparative table. But the questions that arise in the matter of its empirical nature are in fact not all the same as those that have already arisen regarding the empirical nature of numerical simulation. I can already say that the formalization that such an integrative simulation carries out takes on a compactness and a depth due to the fact that several different perspectives, and therefore the approaches of several different disciplines (physiology, mechanics, architecture, etc.), are possible at the same time. Simulation thus breaks, at the very least, with the perspectivist and purely pragmatist epistemology that often accompanied the first formal models: modelling from a precise perspective, and with a precise objective. Software-based and object-based simulations go beyond an *integrative pluralism*[13] as well as a *selective realism*,[14] and truly implement an *integrated plurality*. It is thus a very different epistemic practice than traditional formal modelling and its technical extensions. It is therefore necessary to try to look at it in a different manner.

Although simulation was conceived from the practices of modelling, it has admittedly not made modelling disappear. But it has shifted, amplified and somewhat disaggregated modelling by giving a new status to the formalisms: a quasi-empirical status. It is here that lies the central role of the computer, the half-material, half-formal instrument that has contributed to building bridges of various types between the practices of minimally abstracted replication and the more classic practices of abstraction and calculation. Computer simulation was developed first of all in the form of so-called *numerical* simulation. In this form, it first served to resolve the mathematical models that were otherwise intractable, and in so doing made it possible to considerably extend the methods of calculation by finite elements that date back to mid-19th-century techniques for the calculation of structures. Since the 1990s, however, computer simulation has decisively broken with the monopoly of that single function of approximate calculation of models. At times, it even precedes the model. To such an extent that, for the past ten or fifteen years, far from limiting itself to the numerical resolution of mathematical models that have been conceived beforehand with one single set of axioms and from one single perspective, more and more scientists seek formal models on virtual integrative mock-ups or on pluriformalized integrative models of simulation. In such simulations, it is not just various homogeneous algorithmic rules that replace the mathematical laws (this is the case of the *algorithmic simulations* developed since the beginning of the 1960s), but these rules may go so far as to be fundamentally pluralistic, evolutive, heterogeneous and spread out over the different times and spaces of the computation. The order of priority between model and simulation is thus inversed: we simulate before we model. Software-based computer simulation thus is distinguished not just from numerical simulation, but also from algorithmic simulation.[15] Having now become the complex double of a reality that is perceived and conceived as complex, computer simulation has ended up melding with the experimentation per se and the monoformalized modelling. Thus, since becoming software-based in the 1970s, simulations have had a tendency to become considerably more complex. They now allow an *integrative and figurative realism*, and these detailed, multiscale and multi-process representations have taken on an altogether remarkable weight. In return, when they are validly calibrated and stabilized, these simulation strategies make it possible for modellers to leave behind the completely simulated approaches and to enter into a phase of formalization that, starting in Chapter 7, I propose to call *remathematization*. Thus, it becomes more and more clear that in certain domains that study objects, such as plants, that are considered to be complex, searching for a formal model directly from the data, without prior integrative simulation, now seems to be truly too arbitrary and something that should be avoided. Today, a mathematical modelling that aims to skip the step of integrative simulation, even if its declared aim is merely theoretical, heuristic or pragmatic, becomes more and more open to question. Thus this inversion of priority between the practice of simulation and the practice of mathematical formalization is not the least of the recent contributions of computerization in the sciences that use models.

What particular technical and epistemological choices determined this type of decisive innovation? What are the precise types of the various integrations and convergences that, after a period of detachment and then of pluralization and dispersion of formal models, characterize this new epoch into which we have entered – an epoch in which, as we will see, plant-growth models and simulations have been precursors to a considerable extent?

Might it not be said – with regard to the formalisms that are applied to the objects studied by the empirical sciences – that this epoch of integration and convergence of formalisms in fact testifies to a simple practice of "rerooting"? In other words: to what extent can it be said that the convergences made possible by computerizing the methods of formalization exhibit neither a return back towards a mathematicist essentialism according to which the world is seemingly written in a single mathematical language, nor an escape forwards to a naïve and illusory figurative realism, the result of our apparent fascination with images and virtual worlds, rather than a desire for comprehension and true science? For that matter, in what sense can it be said of a computer simulation that it possesses an empirical dimension? Is this true of all simulations? Otherwise, of which ones is this true, and why? What are the limitations of the knowledge conferred by software-based and object-based simulations if we are already able to perceive them? What precise epistemological lessons can we already draw from this very recent evolution? And finally, what new conceptual and terminological propositions can the modern epistemology of models and simulations adopt to try to go a step further than the old epistemologies of models that, in the 20th century, were successively or concurrently of syntactic (logicism), dialectic, semantic and then pragmatic influence?[16]

This historical and interpretative investigation, which I have the honour to submit here in updated form for English-speaking readers, attempts to answer some of these questions. It does so by choosing to focus on certain scientific works that have, to my mind, played a large part in determining this recent transition from model to simulation. As we will see, I have paid particular attention not only to the technical choices of these works, but also to the methodological and epistemological decisions that accompanied them, as well as, when necessary, to the administrative and institutional contexts that witnessed their emergence. This work, inspired by the reflections that cropped up during my own use of mathematical modelling and numerical simulation in the field of applied atomic physics,[17] is based primarily on field-survey work, on a systematic collection and analysis of publications and archives, and on oral and written interviews carried out with some twenty-odd of the main protagonists of this story. It is also based on the interpretation and epistemological contextualization of the various recent schools and practices of modelling and simulation. Based on the idea that a philosophy of science cannot do without a history of science that is both very contemporary and highly comparative, this work aims to draw an epistemological lesson that is, if possible, enriched and differentiated regarding the different practices of formalization used in the empirical sciences – practices that have continued without cease to characterize modern science since its first great successes of the 17th century.

Notes

1 P. Galison, *Image and Logic*, Chicago: University of Chicago Press, 1997.

2 F. Varenne, *Théories et modèles en sciences humaines. Le cas de la géographie* [Theories and models in human sciences. The case of geography], Paris: Éditions Matériologiques, 2017.

3 The comparative history of this earlier period was the focus of another book, which has not yet been translated: F. Varenne, *Formaliser le vivant: lois, théories, modèles?* [Formalizing living beings: laws, theories, models?], Paris: Hermann, 2010.

4 R.A. Fisher, "Studies in crop variation, I. An examination of the yield of dressed grain from Broadbalk", *Journal of Agricultural Sciences*, 1921, 11, pp. 107–135.
5 W.S. Gosset (alias "Student"), "The probable error of a mean", *Biometrika*, 1908, 6, pp. 1–25.
6 See glossary.
7 Regarding the polysemy of this term, see glossary.
8 F. Varenne, "Nicholas Rashevsky (1899–1972): de la biophysique à la biotopologie" [Nicolas Rashevsky, (1899–1972): from biophysics to biotopology], *Cahiers d'Histoire et de Philosophie des Sciences*, Special Edition, 2006, pp. 162–163.
9 For a comparative analysis of these three contributions, see F. Varenne, *Formaliser le vivant* . . . , 2010, op. cit., partie III "La naissance des simulations" [Part III "The birth of simulations"], pp. 163–217.
10 On these various theoretical approaches, see F. Varenne, *Formaliser le vivant*. . ., 2010, op. cit., partie IV "Le tournant mathématiste des théories" [Part IV "The mathematicist turning point of theories"], pp. 219–275.
11 This mutual exclusion of multiple models is not necessary, however, suggests A.F. Schmid in *L'âge de l'épistémologie* [The age of epistemology], Paris: Kimè, 1998.
12 "Integrative" in no way signifies "integral".
13 S.D. Mitchell, *Biological Complexity and Integrative Pluralism*, Cambridge: Cambridge University Press, 2003.
14 P. Humphreys, *Extending Ourselves: Computational Science, Empiricism and Scientific Method*, Oxford: Oxford University Press, 2004.
15 See the terminological distinctions set out in Chapter 8 and the glossary.
16 For confirmation of this reading, see M.S. Morgan, M. Morrison (Eds), *Models As Mediators*, Cambridge: Cambridge University Press, 1999. For a debate on interpretation, see F. Varenne, *Les notions de métaphore et analogie dans les épistémologies des modèles et des simulations* [The concepts of metaphor and analogy in the epistemologies of models and simulations], Paris: Pétra, 2006.
17 Between 1993 and 1996 I was first a trainee Engineer and then Research Engineer at the Laboratoire de l'Horloge Atomique (LHA – Atomic Clock Laboratory) of the CNRS (Centre national pour la recherche scientifique – National Centre for Scientific Research), at Orsay, near Paris, during two periods covering a total of 15 months. This laboratory has since merged with the SYRTE laboratory. The SYRTE department – Systèmes de Référence Temps Espace (Time and Space Reference Systems) – belongs to the Paris Observatory – Paris Sorbonne Lettres Research University and is also associated with the CNRS – National Research Centre and University Pierre & Marie Curie (Paris 6) – Sorbonne University. Website: https://syrte.obspm.fr. I would like to take this opportunity to thank the colleagues I had the pleasure of working with then, and with whom I continued my training in physics and modelling: Pierre Cérez, Noël Dimarcq and Bertrand Boussert.

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