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Introduction

Modern science is, to a large extent, a model-building activity. In the natural and engineering sciences as well as in the social sciences, models are constructed, tested and revised, they are compared with other models, applied, interpreted and sometimes rejected or replaced by a better model. Some models help scientists to systematize huge amounts of data, coming from experiments or generated through computer simulation, and to extract information out of them. Other models are developed with the aim to explain a puzzling scientific phenomenon – a task that typically requires a number of clever idealizing assumptions and, more and more, the use of computer simulations. By now it is uncontroversial that scientific models are indispensable for solving scientific problems. While some philosophers (such as Ronald Giere (1999) and Bas van Fraassen (1990)) think that science can do without laws, it seems utterly impossible for science to do without models.

The extraordinary importance of models in science has not gone unnoticed by philosophers of science. Starting in the 1960s, scholars such as Peter Achinstein (1968) and Mary Hesse (1963) focused on simple models, such as the billiard ball model of a gas, to illustrate various philosophical claims about, for example, the role of metaphors and analogies in science. Others, most notably Patrick Suppes (1969), explored the connections between scientific models and mathematical (model-theoretical) models and stressed the role of models in the analysis of data ("models of data"). Later, beginning in the 1980s and initiated by seminal contributions by Nancy Cartwright (1983), Ronald Giere (1988), Ian Hacking (1983) and Bas van Fraassen (1980), increasingly complicated scientific models, from physics as well as from the special sciences, gained center stage, and new questions, for example about the relation between theories and models, came to the fore. This debate led to a rethinking of many traditional topics in the philosophy of science, including the nature of confirmation, explanation, and the structure of scientific theories, as well as the role of approximations, idealizations and intertheoretic relations. For a detailed overview of these debates, we refer the reader to the survey article by Frigg and Hartmann (2012). Bailer Jones (2009) gives a book-length discussion of models in science, including an intriguing account of the history of the philosophy of scientific models.

In order to narrow down this tremendously broad and rich field of study, we decided to focus on the modeling of complex systems. All natural and social sciences are concerned with such systems, and it is here where one of the great advantages of model-building becomes especially vivid: Modeling helps scientists to make complex objects or systems comprehensible. With the help of a model, and by studying its features, scientists learn about the object or system that the model represents (van Fraassen (2008)). To model an object or system means to *reduce its complexity* and to provide a simplified description of it. This requires the identification of relevant features of the object or system under investigation that suffice, or so it is hoped, to serve a certain purpose (e.g. confirmation, explanation, prediction or understanding). This volume illustrates how this works by focusing on examples from real science, especially from bioinformatics, climate science, mathematics, neuroscience, physics, psychology, and the social sciences. We will see that the resulting equations are typically too complicated to be solved analytically, and so computer simulations are required to proceed, which stresses the pragmatic constraints on scientific models and simulations (Humphreys 2004). For further philosophical discussions of the role of computer simulations in science and the methodological problems that they raise, we refer the reader to Hartmann (1996) and Winsberg (2010).

While these questions may appear to be of exclusively philosophical importance, the practice of model-building also raises many specific methodological problems that worry scientists. Many of these problems are so specific that they are exclusively dealt with in the respective scientific community. Other questions are somewhat more general and call for a philosophical analysis; these are the ones we want to address in this volume. To do so, this volume assembles eight articles by leading scientists, each of which is commented on by a philosopher of science. At the conference, a general discussion followed. This speaker-commentator-discussion scheme led to a lively debate, and we hope that the essays assembled in this volume reflect this exchange of ideas. At this point we want to outline three major areas of discussion and interaction between scientists and philosophers of science.

First, we are interested in *descriptive questions* regarding how scientists in the various disciplines proceed when they model complex systems. Which modeling strategies do they apply? Are these strategies subject-specific, or are there more universal strategies that are useful in several disciplines? Can one scientific discipline learn from the techniques and strategies used in another?

Second, we are interested in *normative questions* of model assessment. There are several factors that play a role here and that scientists value. Scientists want, for example, that a model accounts for the available data. At the same time, they want it to be consistent with relevant theories, and they want the model to provide understanding. Note that these goals may be in conflict with each other: Models that provide understanding often do not get the data right, and, conversely, models that get the data right do not provide understanding. This raises the question of how the various goals should be weighted. And: Are these weights subject specific, or can one say something more general here? Can the different goals of scientific modeling be reduced to one goal, say truth? These are some of the normative questions that philosophers of science address. Other, more specific normative questions, include issues regarding the empirical testing of models and the question which normative conclusions, e.g. in the social sciences, should be drawn from (typically) highly idealized models.

Third, we are interested in *epistemological and metaphysical implications* of the practice of model-building. What picture of science and the world makes best sense of this practice? Should we conclude, inspired by the apparent patchwork of theories and models, that also the world is a patchwork, i.e. shall we follow Nancy Cartwright (1999), who famously argued that the world is dappled? Or is there hope that, one day, all the bits and pieces that scientists collected will fit into a neat coherent picture of the world? And: How are theories and models related anyway? Is there a hierarchy of theoretical approaches, or do all approaches operate at the same level of fundamentality? See also Hartmann et al. (2008) and Morgan & Morrison (1999) for collections of papers on these topics.

These are only some of the questions that are addressed in this volume. Let us now shortly outline the individual chapters.

The essay by *Matthias Bartelmann* (University of Heidelberg) explains that there are at least three reasons why any attempt to construct cosmological models seems to be a bold enterprise: Firstly, we cannot do experiments with the universe as a whole. Secondly, we are part of this universe. Thirdly, we always only see a small – although growing – section of it. In spite of these challenges, the standard model of cosmology is a remarkably successful example for how the complexity of the real world can be reduced. The starting point for the construction of this model is Einstein's General Theory of Relativity. By adding two simple symmetry conditions – namely the requirements that arbitrary spatial rotations and translations should leave the observable universe unchanged for any observer – the class of so-called Friedman models is obtained. These models are characterized by a small number of parameters. It can be shown that it is possible to find a single set of parameters such that from these models a consistent picture of the actual state of the universe and its evolution can be drawn. This picture is in accordance with virtually all cosmological observations, amongst them the expansion of the universe, the cosmic microwave background and its temperature fluctuations. For this picture, however, a price is to be paid: the existence of dark matter and dark energy has to be accepted.

In his commentary on Bartelmann's paper, *Andreas Bartels* (University of Bonn) uses the standard model of cosmology as a striking example to illustrate three main tasks that models can fulfill: Firstly, they provide a net of theoretical relations that can be used to interpret data. Secondly, these data thus integrated in the model may fulfill an evaluative function by confirming or disconfirming theoretical relations of the model. Thirdly, there is also an explorative function of models that is responsible for the research dynamics of cosmology. Models do not only describe reality, they are also instruments for exploring reality.

What happens when a phenomenon is to be investigated for which no detailed and precise mathematical model can be derived or for which a model is available but too complicated to be analyzed? These are the questions addressed in the contribution by *Martin Golubitsky* (The Ohio State University). The author shows that in such cases the existence of symmetry can nevertheless help understanding certain patterns of the system in question and enable new predictions and explanations. He illustrates this point by discussing three examples: In his first example, he discusses symmetry and symmetry breaking with respect to patterns in burner flames. The second example refers to the symmetry description of locomotor central pattern generators. Golubitsky argues that this description allows several new predictions to be made. In particular, it makes possible to predict the existence of an unexpected but natural gait shown by mammals of different species: the jump. The third example refers to the experimentally determined symmetry of the primary visual cortex. Golubitsky outlines how, through symmetry breaking arguments, an unexpected correlation between this symmetry and a variety of geometric visual hallucinations can be predicted. He thereby refers to hallucinations experienced by test persons who have taken certain drugs.

In his commentary on Golubitsky's paper, *Thomas Reydon* (Leibniz University Hannover) addresses the epistemic virtues of general mathematical models. More specifically, he asks how symmetries (as well as broken symmetries) help scientists to understand patterns exhibited by various physical and biological systems. He argues that the role of these models is more heuristic in nature: they only provide "how possibly" explanations that – when applied to biological systems – have to be supplemented by "how and why actually" explanations of functional, developmental and evolutionary biology.

In his essay, *Dirk Helbing* (ETH Zurich) is concerned with the modeling of complex systems, especially those complex systems that we find in the social sciences. This endeavor raises a number of challenging methodological questions that Helbing addresses on the basis of an analysis of a number of case studies from his own research. Helbing is especially interested in the epistemological status of multiple models for the same phenomenon. How are these different models related? Are they all true, or is none of them true? And: Is there one true model that we will develop at some point, or do we have to be content with a plurality of models? So far, science certainly confronts us with a plurality of models, and the question arises, for example, whether averaging the predictions of all such models leads to a better prediction. Helbing concludes that a paradigm shift towards a pluralistic or possibilistic modeling approach, i.e. an approach that integrates multiple world views, is overdue and argues that it can be useful to combine many different modeling approaches to obtain a good picture of reality, even though they may be inconsistent.

Stephan Hartmann (LMU Munich) discusses Helbing's insights and ideas from the point of view of contemporary philosophy of science. More specifically, Hartmann distinguishes between different kinds of pluralism and elaborates on the question under which conditions the availability of multiple models is advantageous from an epistemological point of view.

In his essay, *Uskali Mäki* (University of Helsinki) focuses on the role of models in economics and the methodological questions that the practice of modeling raises. Mäki starts off by observing that models are a central tool in economics and any policy recommendation an economist gives is based on a model. At the same time, models are highly idealized and abstract from many features of the system under consideration. This prompts the question how economic models relate to the world. Or, to put it more philosophically, how does an economic model represent its target system? To address these questions, Mäki presents a detailed theory of how economic models represent an economic system and shows how this theory fits into a realist philosophy of economics that he has been defending and elaborating for many years. Furthermore, he extends his framework by distinguishing three broad ways in which modeling can be, and actually is, contested in the controversial discipline of economics. These correspond to three kinds of possible failures of modeling.

In his commentary, *Julian Reiss* (Durham University) provides a detailed criticism of Mäki's account and makes a number of suggestions for how to fix it. Reiss is especially concerned with the application of economic models to practical and policy-related problems, which raises additional methodological problems.

In their essay, *Peter König, Kai-Uwe Kühnberger* and *Tim Kietzmann* (University of Osnabrück) consider models of the function of the mind. This is an especially complicated task as the human mind is probably the most complex system on earth. Their ambitious goal is to present a unified model of low- and high-level cognitive systems. Such a unified model seems reasonable as high- and low-level cognitive systems implement similar structures despite their functional differences.

In their commentary, *Markus Werning* (Ruhr University Bochum), *Michela C. Tacca* (University of Düsseldorf) and *Alexandra Mrocko-Wąsowicz* (National Yang-Ming University Taipei) provide a detailed criticism of the specific model that König and collaborators suggested and discuss alternative accounts. To do so, they focus on the visual domain and draw on the theory of neuroframes.

The essay by *Reinhold Kliegl* and *Ralf Engbert* (University of Potsdam) presents an example of a model, located at the interface between experimental psychology, cognitive neuroscience, and computational neuroscience: a model for eye-movement control in reading. At a very basic level, reading can be described as an alternation between quick eye movements (saccades) and periods of relative rest (fixations). In cognitive modeling of this process, two prototypical approaches are distinguished. The serial processing approach assumes that attention moves from one word to the next, contingent on access of the meaning of a word. Also saccade programs are contingent on the completion of some lexical subprocess. In contrast, the parallel processing approach assumes that lexical and oculomotor processes are only loosely coupled and that sometimes more than one word can be processed simultaneously. The SWIFT model is an implementation of the second approach. Following up previous proposals, the authors demonstrate that the sole reliance of a criterion of goodness of fit is not sufficient for a differentiated evaluation and ranking of competing models. They illustrate the application of three additional criteria – model strictness, reliability of data, and unexpected model predictions – for the evaluation of the SWIFT model.

In his commentary, *Martin Hoffmann* (University of Hamburg) focuses on the relation between specialized models and more general and comprehensive empirical theories. He argues that Kliegl and Engbert's SWIFT model provides an example of a model that was developed largely independently of any more general psychological theory. By referring to the unexpected model predictions criterion, which Kliegl and Engbert apply to evaluate the SWIFT model, and relating this criterion to Lakatos' methodology of research programmes, Hoffmann analyzes differences in the evaluation of models and theories. He finally proposes two necessary conditions that must be fulfilled in order to turn models into useful instruments for the development of a more general underlying theory.

Within the realm of the natural sciences, models and simulations are primarily used in order to better understand and quantitatively explain natural phenomena. By contrast, modeling in the engineering sciences focuses on designing, operating and controlling artificial systems and processes. In his contribution, *Wolfgang Marquardt* (RWTH Aachen) describes the outlines of a general methodology for modeling complex kinetic phenomena that govern the behavior of chemical and biological process systems. Marquardt explains how modeling and simulation techniques, as well as techniques for model identification and discrimination are combined with high resolution-measurements in the methodology in a highly elaborate way. In an iterative process, models and underlying (real or simulated) experiments are used for mutual refinement. This methodology enables the successive design of more detailed models that are tested by an expanding data basis. Marquardt concludes by illustrating this methodology by three typical chemical engineering problems: reaction kinetics, multi-component diffusion in liquids, and energy transport in falling film flow.

Robin Findlay Hendry (University of Durham) starts his commentary on Marquardt's paper by comparing the role of models in chemical engineering with the role models play in 'pure' chemistry. He then focuses on kinetic models of chemical reactions and discusses the mutual refinement of kinetic models and the corresponding experimental set-ups. He argues that the identification of the mechanisms that lead from the reactants to the products can be seen as a case of eliminative induction. Theoretical models of molecular structure and reaction mechanisms provide the starting point for this process insofar as they delimit the set of mechanisms that have to be considered.

In the last essay collected in this volume, *Valerio Lucarini* (University of Hamburg) concerns the role of models and simulations in climate science. He explains why the use of these tools faces specific problems in this field of research. One problem is related to our imperfect knowledge of the initial conditions, another to the imperfect representation of the processes of the system. Together, both deficits severely limit the possibility of providing realistic simulations and predictions. Considerable difficulties are caused by the fact that climate science does not have laboratories where models and simulations could be tested against experiments. Finally, serious methodological problems are generated by the fact that the relevant processes of climate change occur on a large variety of spatial and temporal scales. Lucarini argues that the macroscopic theory of non-equilibrium thermodynamics provides a relevant framework for improving our understanding of climate change and our ability to model it. At the same time, he rejects the expectation that there will be fundamental progress in climate science in the next few decades simply because computers become more and more powerful.

In his commentary, *Gregor Betz* (Karlsruhe Institute of Technology) distinguishes between those aspects of climate change that are known independently of any global climate model, and those that cannot be estimated without these models. He argues against the assumption that the chaotic nature of weather automatically – without any further empirical evidence – implies that the climate system is also chaotic. Given the plurality of global climate models, he then turns to the question whether and how one can empirically test, compare, and rank these rival models. To illustrate his points, Betz focuses on Lucarini's proposal of a process-oriented metrics for model evaluation, which puts special emphasis on a better understanding of the key causal processes in climate systems.

In closing, we hope that this volume will encourage the reader to reflect upon the fascinating role of models in science and that it will stimulate further discussions between scientists and philosophers of science.

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