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ON THE NATURE OF MODELS: THE UNFINISHED DEBATE

Emiliano Ippoliti, Fabio Sterpetti a Thomas Nickles, eds. *Models and Inferences in Science*. Cham: Springer, 2016, 256 pages.

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Models now play a dominant role in contemporary science, where they form the bulk of scientific output, the development of which has been precipitated by the massive influx of computers into virtually all scientific disciplines. Cutting-edge fields, such as genetics or particle physics, in particular would be unthinkable without the large-scale deployment of computers, which crunch big experimental data and allow researchers to validate or construct new models.

Models also entered the popular imagination when they were blamed for their part in triggering the 2008 financial crisis[1] – or, more precisely, it was the *hubris* of economic modelers (*alas* often coming to econom-ics from the natural sciences), who created the flawed depictions of economic reality and exaggerated the predictive powers of their petty creations.

But the dalliance between models and science is much older than that and goes back to the time-honored experiments of Galileo and even earlier. It is thus no wonder that philosophy of science has been reflecting upon the subject matter. Since the foundational work of Tarski[2] in the 1930s and then the revived interest spurred by Semantic View adherents, the attention to models has been steadily growing in the past decades, when fundamental contributions to their understanding have been made.

The volume under review was published as the 25th book in the SAPERE series in which books on models are regularly issued,[3] such as the recent comprehensive *Handbook of Model-Based Science,*[4] which offers a number of theoretical contributions together with detailed accounts

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[1] An honest reflection on modeling punditry gone wrong is given, for example, in Emanuel Derman, *Models. Behaving. Badly: Why Confusing Illusion with Reality Can Lead to Disaster on Wall Street and in Life* (New York: Free Press, 2011).

[2] Alfred Tarski, "The Concept of Truth in Formalized Languages," in *Logic, Semantics, Metamathematics; Papers from 1923 to 1938* (Oxford: Oxford University Press, 1956), 152–278.

[3] *Studies in Applied Philosophy, Epistemology and Rational Ethics*, which aims to present works at the crossroads of philosophy and scientific and technological disciplines.

[4] Lorenzo Magnani and Tommaso Bertolotti, eds. *Handbook of Model-Based Science* (Cham: Springer, 2017).

of the multitudes of uses of models and simulations in the sciences. Similar collections from other publishers include *Models, Simulations, and Representations*,[5] or the older, now classical title such as *Models as Mediators: Perspectives on Natural and Social Science*[6] and numerous other books by individual authors.

Models and Inferences in Science provides a panoramic view of the topic, in which both theoretical and practical chapters give a glance into the world of scientific modeling. Apart from this main focus, there is no central theme to the volume, so it contains rather loosely related contributions addressing various issues about modeling, including the roles models assume in mathematics, biology, astronomy, physics, and psychiatry or such practical disciplines as petroleum engineering.

As the editors, Emiliano Ippoliti, Fabio Sterpetti and Thomas Nickles, astutely remark in the opening chapter "Modeling and Inferring in Science" (1–9) models come in all shapes and sizes and cannot be conveniently covered by a single definition, and this volume attests to that fully – in contempo-

rary science, models range from inanimate physical objects through sets of mathematical equations to living beings (e.g. mice in medicine). This variety makes it difficult to attain any unifying definition of the concept, and the same holds true for the philosophical underpinnings of the models. Philosophers of science have not reached any agreement as to the nature of models, so they can, for example, be conceived in an instrumental sense as heuristical devices, or in a model-theoretical sense as vehicles in the search of true scientific knowledge, or in any other sense. As a result, any attempt to exactly pinpoint the concept of model or to appropriate it for a particular use invites objections from opposing philosophical camps. This leads the editors to the rather skeptical conclusion that some of these differences simply create unsurmountable obstacles – for example the Received View is clearly not compatible with the Semantic View, and so they can never be reconciled.

Sorin Bangu in his chapter "On 'The Unreasonable Effectiveness of Mathematics in the Natural Sciences'" (11–29) ponders the question originally spelled out by physicist Eugen Wigner almost 60 years ago,[7]

[5] Paul Humphreys and Cyrille Imbert, eds. *Models, Simulations, and Representations* (New York: Routledge, 2011).

[6] Margaret Morrison and Mary Morgan, eds. *Models as Mediators: Perspectives on Natural and Social Science* (Cambridge: Cambridge University Press, 1999).

[7] Eugene Wigner, "The Unreasonable Effectiveness of Mathematics in the Natural Sciences," *Communications in Pure and Applied Mathematics* 13, no. 1 (1960): 1–14.

which eventually gave rise to an avalanche of responses from scientists and philosophers big and small. If we sum up Wigner's original thesis into several simple maxims, they would go like this: The first mystery is the role that mathematics plays in the natural sciences, which also quite naturally engenders the contention that the laws of nature are formulated in mathematical language. And equally mysterious is the capacity of the human mind to discover ("divine") these laws. Before opening his own polemics with Wigner, Bangu enumerates six main objections to Wigner's article by his opponents. Unlike Wigner, the critical voices find the alliance of mathematics and natural sciences quite natural – from the view that this happy accident can be attributed to chance, through the view that physical systems presuppose modeling, which makes them fit for mathematical treatment, to the opinion that our mathematical sense is an evolutionary tool. Bangu recaps the discussion, laying out arguments both for and against Wigner's thesis, with multiple digressions to related topics such as the history of mathematics, its divine/human nature, the anthropic principle, and even intelligent design. To illustrate his own objection, Bangu introduces a fictional physicist Neinstein who presented his theory of General Theory of Relativity *before* the necessary mathematical apparatus (Riemannian geometry) was in place, and then raises a crucial question: Would such a theory (or, better said, vision, since without the mathematical formalism, we can hardly call it a theory) be taken seriously? Bangu's answer is a resolute no,[8] and he uses this line of argument to do away with Wigner's thesis: there is no miracle in operation here and everything can be explained by the normal workings of the scientific environment, which leads him to the conclusion that "Wigner's riddle can be (dis)solved."

Scientific decisions almost always seem rational – in retrospect. Science, once considered a prime example of rationality, lost some of its gilding after the peculiarities of scientific decision making have been brought to light in the works of Simon, Kuhn, Gigerenzer and others, who have pointed to various kinds of non-rationalities that are deeply moored in scientific thinking. In "Fast and Frugal Heuristics at Research Frontiers" (31–54) Thomas Nickles explores the potential of fast and frugal heuristics (f&f), first proposed by Gigerenzer

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[8] Here we can disagree, this conclusion is perhaps valid for some theories, but is not universal. Many theories in physics were at first expressed only in qualitative terms (e. g., electricity) before being formalized into mathematical language.

and his colleagues[9] in the context of frontier research characterized by sparse and badly organized domain knowledge, where decision-making is often done under extreme uncertainty. Despite the fact that in frontier research a lack of information typically occurs, Nickles astutely observes that the current difficulty in science is exactly the opposite – there is too much information, such as articles, experimental data etc. – the situation he calls the *knowledge pollution problem*. He then probes the possibility of applying some practical recommendations and "rules of thumb" to facilitate research – but reports mixed results.

Fabio Sterpetti devotes his chapter "Scientific Realism, the Semantic View and Evolutionary Biology" (55–76) to the appraisal of the suitability of applying the Ontic Structural Realism (OSR) framework to population biology. The specific link between this particular branch of Structural Realism, compounded with Semantic View, and mathematical models in population genetics was established by Steven French in *The Structure of the World*.[10] Sterpetti, nevertheless, challenges this view and levels

a good amount of criticism against Frenche's proposal. The central issue here is the relation of models and mathematics to the world: lumping together Structural Realism with the Semantic View, he maintains, imminently leads to the conception that physical reality is understood in terms of mathematical structures, although this view is beset with a number of problems such as the *collapse problem*.[11] The first attempts to apply the Semantic View to biological theories dates back to the 80s, when this approach seemed more appropriate for biology than the Syntactic View (Received View) with its requirements for axiomatized theory. The choice of evolutionary biology was also not random, because this field belongs to the most formalized in biology. But the Semantic View in biology is vulnerable too, as Sterpetti warns. He reiterates some points of contention, among them that the current formulation of population genetics is not adequate, because models in population genetics are mostly of a statistical nature and provide mere descriptions without capturing causal links or providing explanations and predictions. In ad-

[9] Gerd Gigerenzer, Peter M. Todd, and the ABC Research Group, *Simple Heuristics That Make Us Smart* (New York: Oxford University Press, 1999).

[10] Steven French, *The Structure of the World* (Oxford: Oxford University Press, 2014).

[11] The assumption of a relation between the world and theories implies that the (physical) world is also a mathematical structure – a dubious proposition that is by no means universally shared by current philosophers of science.

dition, as he also points out, models (theories) in biology are much more complicated than in physics, as are the phenomena they describe. Based on these arguments, he concludes that Frenche's proposal to adopt OSR in biology doesn't bring any desired advantage over previous efforts.

Emily Grosholz in her contribution "Models of the Skies" (77–94) takes the reader on a tour of historical models of the sky (the solar system, our galaxy and the cosmos in general). Along the way, Groszholz discusses several general topics such as ampliative reasoning and the referential versus analytical purpose of models. The journey begins with Brahe's *Rudolphine Tables* and Kepler's laws of motion, and continues through Galileo's observations of the sky, Newton's revolutionary contributions, and 19th century achievements, to the apex of modern astronomy with its currently hotly debated topic – the unexplained phenomenon of galactic movements, which currently divides the community of physicists and astronomers. The stakes are high, since the deep discord between measurements and theory calls either for the revision of fundamental gravitational laws (e.g. the MOND theory), or the introduction of "dark matter" into the picture, a sort of *ad hoc* theory which makes the data fit the observation. From this case, the takeaway – although not expressed explicitly by Groszholz – is that no matter how reliable the theories or models we develop are and how well they match the existing data there is always the possibility of a discovery that threatens to upend established physical knowledge.

Carlo Cellucci in his "Models of Science and Models in Science" (95–112) postulates an important distinction between *models of science* and *models in science*, the former denoting models of scientific activity, the latter models that are used in individual sciences (for example, those used in biology, physics or other sciences). He recognizes four individual *models of science*: the analytic-synthetic model (Aristotle's), the hypothetico-deductive model (Carnap's and others), the semantic model (van Fraassen's), and the analytic model (Pólya's). Without going into much detail, Cellucci sketches the distinctive features of these models and how they generalize the functioning of science, and discusses their relation to *models in science,* i.e. how they account for models used in various sciences. From the *models of science*, Cellucci selected the analytical model as the one which best accounts for both theory formation and theory change. Thanks to its unique features, he maintains, the analytical model is highly congruent with one of the essential characteristic of sci-

ence – the potentially infinite quest for more general theories. However, in my opinion, this suggested primacy is disputable, because there are also other alternative models that account for scientific changes (i.e. Kuhn's, Laudan's, Hull's, Giere's etc.) and it is not quite clear why this particular model favored by Cellucci should come ahead of the others. Even if Cellucci's descriptions of the individual *models of science* are too brief and simplifying, he inadvertently brings up an important question – how is it possible that up to now, the philosophy of science has not reached a consensus regarding the *model of science* of how science works?

The use of models to explain various kinds of disorders has become a common occurrence in the life sciences and medicine – and psychiatry is no exception. In "Mechanistic Models and Modeling Disorders" (113–32) Raffaella Campaner takes the example of ADHD (Attention Deficit Hyperactivity Disorder) which she approaches with a mechanistic and "neo-mechanistic" framework. Simply put, the mechanistic framework takes mechanism as the basis from which behavior is explained, in which the mechanism is an organized system of interacting parts that perform certain activities. The important feature of these models is the knowledge of the underlying mechanism

that produces a given behavior, so they shouldn't be understood only as "black-box" types of models. Typically, in neuropsychiatric models, in addition to biological factors we encounter all sorts of other interactions, for example cultural and social, which take on an explanatory role. That said, it is obvious that these models are complex networks of causal mechanisms, rather than simple causal links, because psychiatric disorders tend to be "messy", meaning that their causes are still poorly understood by contemporary science. This is the case of ADHD[12] too, for which the etiology is still only vaguely determined, with both genetic and environmental factors at play. Camparer discusses two theoretical models of ADHD – the executive dysfunction model, and the motivational model – positing a single underlying neurobiological mechanism, which provides some explanation. However, these models do not exhaust the whole spectrum of possible mechanisms and a combined "multiple pathway" model (in addition to other models) has been suggested. Camparer remarks on an interesting aspect of this. When model builders try to incorporate factors other than biological (higher

[12] ADHD is defined as "a persistent pattern of inattention and/or hyperactivity-impulsivity that interferes with development" (DSM V classification).

level factors such as cultural, psychological and social) to unravel the causes, they are forced to model various other interdependencies, which makes the overall system even more comprehensive. As a result, they also end up with a different conception of the disorder itself.

The discovery of deterministic chaos, known by the general public as the popular *butterfly effect,* has had a wide-range impact on the philosophy of science as well. Chaos theory states that a small change in the initial conditions of a deterministic system can cause far-reaching effects. This was perceived by some as a lethal blow to the hardline determinism of the Laplacian kind. Sergio Caprara and Angelo Vulpiani in "Chaos and Stochastic Models in Physics: Ontic and Epistemic Aspects" (133–46) try to clarify some common misconceptions about determinism, predictability and related concepts such as reductionism, mechanism, stochasticity and causality. They make an effort to emphasize that the concepts of determinism and predictability especially should be clearly distinguished and that the former doesn't imply the latter. In doing so, they correct some widely circulated errors, for instance Popper's statements about determinism and prediction, which Popper confuses.

After this introduction, they focus on the ontic and epistemic

character of chaos: in their view, determinism is an ontic concept, whereas predictability refers to our knowledge of the system and is therefore epistemic; chaos stands between the two because it exhibits both ontic and epistemic aspects.

Emiliano Ippoliti in "Ways of Advancing Knowledge. A Lesson from Knot Theory and Topology" (147–72) takes on the long-standing problem of how mathematical problems are posed, treated and solved and how this is related to ampliative reasoning and the nature of mathematical objects. His first example are knots:[13] the first attempts at the classification of knots date back to 19th century, when it was suggested that they could lay the mathematical foundation to Lord Kelvin's vortex theory of the atom.[14] More attempts at finding an appropriate approach to their representation followed (graph theory etc.). However, a new impulse for the theory came when the connection between knot theory and braid theory was established, in which knots have braid representation. This move made it possible to study them with algebra (group theory), which opened new vistas for the whole field. In the second example, Ippoliti shows how

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[13] A mathematical knot is a closed non-self-intersecting curve in three dimensions.
[14] They made a comeback in the 20th century physics (quantum theory).

topological objects – 3-manifolds, can be studied with algebraic tools after they have been associated with algebraic structures.

These above-mentioned instances serve Ippoliti as model cases that underscore the unique role that representation of objects in mathematics assumes. Mathematicians often make a great effort to link one object to another, already existing object, so that the connection can be utilized for the advancement of knowledge. In this process, mathematicians employ a number of heuristics for conceptualization, representations and manipulations with mathematical objects. However, the choice of representation singles out some features of the objects and suppresses others, to the effect that these unique representations reveal different properties. One can immediately perceive how this process is related to modeling in general. The objects studied are "enriched" step-by-step with new information, new knowledge is generated and some puzzles are solved, but new problems usually arise too. The bottom line here, according to Ippoliti, is that a "mathematical object is simply a hypothesis put forward to solve a given problem which can always be conceptualized in new ways" (168). In addition, this process is historic, that is, codetermined by the state of the existing knowledge, and holistic, that is, dependent on,

but also influencing, knowledge in other fields. Hence, even mathematical knowledge is dynamic and not static, as it is sometimes presented.

For decades, theories have been a fixture in the scientific realism debate, but the entry of models into the natural and social sciences has somewhat shifted interest in favor of models. Unlike theories, we often work with multiple models of the same phenomenon because they highlight its distinctive features, which makes analysis even more complicated from the realist point of view. In his contribution "Models, Idealisations, and Realism" (173–89) Juha Saatsi examines the ramifications of this proposition. Idealized depictions of various phenomena (frictionless planes, point masses, isolated systems, and omniscient agents etc.) are hallmarks of the scientific approach, however, no one should be surprised that this approach comes at a price, since the way in which science interacts with idealizations can be always brought into question. As Saatsi explains, there are several answers to this challenge: idealizations can be considered as "simplifying suppositions", or approached from the semantic point of view, or adapted for the realist framework – the job that Saatsi himself undertakes in his article. After his exposition of the topic, he deals with the issues of

the falsity/veracity of models, their predictive success, the way in which models exactly models latch onto the reality and how they relate to scientific explanations.

In "Modelling Non-empirical Confirmation" (191–205) Richard Dawid correctly observes that many times the conditions for empirical confirmation of theories are not ideal and face many challenges – for instance, in anthropology (or other historical disciplines), in which scientists typically base their (dis)confirmation of theories only on patchy evidence as a result of missing data, or in physics, where scientist rely in their reasoning on some unobservable entities (as is the recent case of Higgs boson), or they work on theories such as string theory or cosmological theories which lack even a speck of empirical confirmation (and such confirmation may not even be available in the future). Consequently, in such cases, the decision about a particular empirical confirmation is important for the theoretical framework as a whole. However, the issues related to these decisions have more than once caused a good deal of controversy within the scientific community. Non-empirical confirmation, which Dawid understands as an extension of empirical confirmation, thus amounts to observations about the research context of the theory. In his article, Dawid embarks on a mission

to formalize this non-empirical theory confirmation within the Bayesian framework, with the help of the example of the empirical confirmation of the Higgs particle. However, in my view, the execution of such a plan can easily run into difficulties and we have to ask how viable such proposals are for formalization when "confirmation situations" in science typically vary in many, often unrelated, aspects? Unfortunately, we can hardly expect that any kind of a formalized model can account for all the vagaries of the confirmation process in real-life science.

"What is mathematics?" asks Reuben Hersh in "Mathematics as an Empirical Phenomenon, Subject to Modeling?" (207–18). The well-known author[15] then presents his answer in a fast-paced text devoid of the usual philosophical lingo. So far, mathematics, be it content or activity, has been analyzed separately by logicians, historians, psychologists, philosophers, neuroscientists and others, with each of these disciplines contributing its own model of the subject. But what the "modeling business" teaches us in general is that these models only select out *some* properties of the modeled phenomenon and they never encompass

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[15] Hersh is the award winning author and co-author of popularizing books on mathematics.

its entirety. As far as mathematical content is concerned, here, too, philosophers have suggested various competing "positions" or theories, such as nominalism, intuitionism, logicism etc. – but if we regard them as models, each of which captures selected features, the idea of plurality suddenly seems quite natural. Thus Reuben Hersh falls into the liberal category of philosophers and advocates an overall "ecumenical" approach, in which models are considered mutually non-exclusive alternatives under the aegis of unified mathematics studies.

Lorenzo Magnani in "Scientific Models Are Distributed and Never Abstract: A Naturalistic Perspective" (219–40) constructs his critical stance towards factionalism in the philosophical debate on the nature of the scientific model with the support of cognitive science, which allegedly reveals inadequacies in the concept of models as abstractions or idealizations. The gist of Magnani's argument revolves around the idea that scientific models are indispensable tools in the rational and empirical process of scientific discovery and therefore cannot be fictional. In other words, models are also weapons in what Magnani calls "*epistemic warfare*" with nature (target systems, the structure of which they should uncover). And not only that, the fictional account of models obfuscates the distinction between various domains such as science, religion and the arts, which should have clear-cut boundaries. As much as it is bolstered by argumentative terminology, I feel that Magnani's objections fail to provide enough of the evidence which should lead us to discard the fictional account of scientific models (in which he also includes all the concepts of models as "surrogates", "credible worlds", "missing systems", "make-believe" and the like). On the contrary, there is obviously a strong affiliation between scientific and literary fictions as idealization.[16] Scientific models and literary fictions share a great many characteristics, and a study of their deeper connections can yield significant knowledge, although there are, of course, obvious differences that shouldn't be downplayed: it is true, for example, that models and literary characters vary according to their use. But Magnani's sweeping critique doesn't seem to be well founded enough to dismiss the whole business of models as fictions.

Petroleum engineering emerged as a relatively new field in Earth and Mineral Sciences. Kahindo Kamau and Emily Grosholz in the final chapter "The Use of Models in Petroleum and Natural Gas

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[16] As outlined for instance in Roman Frigg "Models and Fiction," *Synthese* 172, no. 2 (2010): 251–68.

Engineering" (241–53) reflect upon the role of models in this field. They offer a refreshing look at the use of models in real-life situations which contrasts with the occasionally sometimes stuffy theorizing present in other chapters of the book. The authors build on van Fraassen's typology of models, which consists of data models, surface models and theoretical models, but suggest enlarging it with a new category that would account for the distinctly human purpose of some of the models. In petroleum engineering, students are typically exposed to physical, mathematical (e.g. Darcy's Law)[17] and technological models (i.e. models of technological equipment), roughly in that order. In this case, model building doesn't appeal only to epistemological needs but also clearly to pragmatic ones – the maximum economic yield, of course, is a strong motivational force behind most of these models – a force that can also result in their distortions and simplifications.

In conclusion, this volume shows the widespread uses of models in contemporary science and reveals their multifaceted nature. As I remarked in the opening paragraphs, the general availability of vast computational power makes the proliferation of complex models in all scientific disciplines even more pronounced. At the same time, philosophers of science have been for decades attempting to establish a solid base upon which the theory of models could be built, but up to now theoreticians remain embroiled in factious squabbles and a common denominator is hard to find. Perhaps then, what is true for *models in science* (if we use Cellucci's terminology), holds true as well for *models of science* – there is no optimal model of science and the search for one is futile. The answer to this puzzle could be quite simple if philosophers of science take heed of the advice Hersh gives to philosophers of mathematics: There is a number of possible approaches and if we look at them as models (both competitive and complimentary at the same time) we can accept the fact that they can happily co-exist. Philosophers of science should embrace this diversity that naturally leaves room for alternative interpretations and should not try to win the game by imposing a single dominating view.

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[17] Darcy's Law is an equation to compute the ability of a fluid to flow through a porous material such as reservoir rock (permeability).