Fictionalists Disregard the Dynamic Nature of Scientific Models

Lorenzo Magnani

Abstract In the current epistemological debate scientific models are not only considered as useful devices for explaining facts or discovering new entities, laws, and theories, but also rubricated under various new labels: from the classical ones, as abstract entities and idealizations, to the more recent, as fictions, surrogates, credible worlds, missing systems, make-believe, parables, functional, epistemic actions, revealing capacities. An influential article by John Woods, entitled "Against fictionalism" (Woods 2013), usefully provides a rich argumentation concerning the puzzling problems created by the use of the concept of fiction in philosophy, epistemology, and logic, Woods himself further deepened in the recent book *Truth in Fiction: Rethinking Its Logic* (Woods 2018). By limiting my treatment to the case of models in science, I would like to offer an additional support to this perspective, emphasizing the unsatisfactory character of this intellectual recent trend, and the uselessness of the concept of fiction in illustrating the scientific enterprise. I will contend that it is misleading to analyze models in science by disregarding the dynamic aspects: scientific models in a static perspective (for example when inserted in a textbook) certainly appear fictional to the epistemologist, but their fictional character disappears if a dynamic perspective is adopted. The article also sketches the role of models in science taking advantage of the concept of "epistemic warfare", which sees scientific enterprise as a complicated struggle for rational knowledge in which it is crucial to distinguish epistemic (for example scientific models) from non epistemic (for example fictions, falsities, propaganda) weapons. A reference to the usefulness of Feyerabend's counterinduction in criticizing the role of resemblance in model-based cognition is also provided, to further corroborate the thesis indicated by the article title.

Keywords Fictions · Fictionalism · Scientific models · Model-based reasoning · Dynamics of scientific knowledge · Static view of science

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L. Magnani (✉)
Department of Humanities, Philosophy Section and Computational Philosophy Laboratory, University of Pavia, Pavia, Italy
e-mail: lmagnani@unipv.it

T. Bertolotti (ed.), *Cognition in 3E: Emergent, Embodied, Extended*, Studies in Applied Philosophy, Epistemology and Rational Ethics 56,
https://doi.org/10.1007/978-3-030-46339-7_5

1 Are Scientific Models Fictions?

An influential article by John Woods, entitled "Against fictionalism" (Woods 2013), usefully provides a rich argumentation concerning the puzzling problems created by the use of the concept of fiction in philosophy, epistemology, and logic, Woods himself further deepened in the recent book *Truth in Fiction: Rethinking Its Logic* (Woods 2018). By limiting my treatment to the case of models in science, I would like to offer an additional support to this perspective, emphasizing the unsatisfactory character of this intellectual recent trend, and the uselessness of the concept of fiction in illustrating scientific enterprise. The article also sketches the role of modeling activity in science taking advantage of the concept of "epistemic warfare", which sees scientific enterprise as a complicated struggle for rational knowledge in which it is crucial to distinguish epistemic (for example scientific models) from non epistemic (for example fictions, falsities, propaganda) weapons. However, my main critique will be substantiated by the observation that it is easier to legitimate the role of models as fictions adopting a static view of science, but much more difficult if we adopt a perspective that stresses the dynamic of scientific reasoning.

Scientific models are now not only considered as useful ways for explaining facts or discovering new entities, laws, and theories, but are also rubricated under various new labels: from the classical ones, abstract entities (Giere 1988, 2007, 2009) and idealizations (Mizrahi 2011; Portides 2007; Weisberg 2007), to the more recent, fictions (Contessa 2010; Fine 2009; Frigg 2010a, b, c; Godfrey-Smith 2006, 2009; Suárez 2009, 2010; Woods and Rosales 2010a, b; Woods 2010), surrogates (Contessa 2007), credible worlds (Kuorikoski and Lehtinen 2009; Sugden 2000, 2009), missing systems (Mäki 2009; Thomson-Jones 2010), as make-believe (Frigg 2010a, b, c; Toon 2010), parables (Cartwright 2009b), as functional (Chakravartty 2010), as epistemic actions (Magnani 2004a, b), as revealing capacities (Cartwright 2009a). This proliferation of explanatory metaphors is amazing, if we consider the huge quantity of knowledge on scientific models that had already been produced both in epistemology and in cognitive science. Some of the authors mentioned above are also engaged in a controversy about the legitimacy especially of speaking of fictions in the case of scientific models.

Even if the above studies related to fictionalism have increased knowledge about some aspects of the role of models in science, I am convinced that sometimes they have also generated some philosophical confusion and it seems to me correct (following the suggestion embedded in the title of a recent paper) "to keep quiet on the ontology of models" (French 2010), and also to adopt a more skeptical theoretical attitude. I think that, for example, models can be considered fictions or surrogates, but this just coincides with a common sense view, which appears to be philosophically empty or, at least, delusory. Models are used in a variety of ways in scientific practice, they can also work as mediators between theory and experiment (Portides 2007), as pedagogical devices, for testing hypotheses, or for explanatory functions (Bokulich 2011), but these last roles of models in science are relatively well-known and weakly disputed in the epistemological literature. In this paper I will concentrate on scientific models— seen in a perspective which takes advantage of a distributed cognition framework—in creative abductive cognitive processes, which I still consider the central problem of current epistemological research (Hintikka 1998).

I think that models, both in scientific reasoning and in human perception, are neither mere fictions, simple surrogates or make-believe, nor they are unproblematic idealizations; in particular, models are never *abstract*, contrarily to the received view. Indeed, within science the adopted models are certainly constructed on the basis of multiple constraints relating to the abstract laws, principles, and concepts, when clearly available at a certain moment of the development of a scientific discipline. At the same time we have to immediately stress that the same models are always *distributed* material entities, either when we are dealing with concrete diagrams or physical and computational models, or when we face human "mental models", which at the end are indeed particular, unrepeatable, and ever-changing configurations and transformations of neural networks and chemical distributions at the level of human brains. In this perspective we can say that models are "abstract" only in a Pickwickian sense, that is as "mental models", shared to different extents by groups of scientists, depending on the type of research community at stake. This cognitive perspective can therefore help us in getting rid of the ambiguities sparked by the notion of abstractness of models.

In a previous work I have also enriched my critique of fictionalism outlining the first features of my own approach to the role of scientific models in terms of what I call "epistemic warfare" (cf. (Magnani 2017, chapter four)), which sees scientific enterprise as a complicated struggle for rational knowledge in which it is crucial to distinguish epistemic (for example scientific models) from non epistemic (for example fictions, falsities, propaganda, etc.) weapons. The characteristic feature of *epistemic* weapons is that they are value-directed to the aim of promoting the attainment of scientific truth, for example through predictive and empirical accuracy, simplicity, testability, consistency, etc.[1] Indeed we have to consider scientific enterprise a complicated epistemic warfare, so that we could plausibly expect to find fictions in this struggle for rational knowledge. Are not fictions typical of any struggle which characterizes the conflict of human coalitions of any kind? During the Seventies of the last century Feyerabend (1975) clearly stressed how, despite their eventual success, the scientist's claims are often far from being evenly proved, and accompanied by "propaganda [and] psychological tricks in addition to whatever intellectual reasons he has to offer" (p. 65), like in the case of Galileo. Galileo's discussions of real experiments—in the *Dialogo* but also in the *Discorsi*—become rhetorical, to confound the opponents and persuade the readers, and also to fulfil didactic needs, as contended by Naylor (1976).

These tricks are very useful and efficient, but one thing is the *epistemic* role of reasons scientist takes advantage of, such the scientific models I will illustrate in this paper, which for example directly govern the path to provide a new intelligibility of the target systems at hand; another thing is the *extra-epistemic* role of propaganda

[1]In this perspective I basically agree with the distinction between epistemic and non-epistemic values as limpidly depicted in (Steel 2010).

and rhetoric, which only plays a mere—positive or negative—ancillary role in the epistemic warfare. So to say, these last aspects support scientific reasoning providing non-epistemic weapons able for example to persuade other scientists belonging to a rival "coalition" or to build and strengthen the coalition in question, which supports a specific research program, for example to get funds.

I am neither denying that models as idealizations and abstractions are a pervasive and permanent feature of science, nor that models, which are produced with the aim of finding the consequences of theories—often very smart and creative—are very important. I just stress that the "fundamental" role played by models in science is the one we find in the *dynamical* perspective of the the core conceptual discovery processes, and that these kinds of models cannot be indicated as fictional at all, because they are *constitutive* of new scientific frameworks and new empirical domains. In this last sense the capacity of scientific models to constitute new empirical domains and so new *knowability* is ideally related to the emphasis that epistemology, in the last century, put on the theory-ladenness of scientific facts (Hanson, Popper, Lakatos, Kuhn): in this light, the formulation of observation statements presupposes significant knowledge, and the search for new observability in science is guided by scientific modeling.

Suárez (2009) provides some case studies, especially from astrophysics and concerning quantum model of measurement, emphasizing the inferential function of the supposed to be "fictional" assumptions in models: I deem this function to be ancillary in science, even if often highly innovative. Speaking of the Thomson's plum pudding model Suárez maintains that, basically "The model served an essential pragmatic purpose in generating quick and expedient inference at the theoretical level, and then in turn from the theoretical to the experimental level. It articulated a space of reasons, a background of assumptions against which the participants in the debates could sustain their arguments for and against these three hypotheses" (p. 163). In these cases the fact that various assumptions of the models are empirically false is pretty clear and so is the "improvement in the expediency of the inferences that can be drawn from the models to the observable quantities" (p. 165)[2]: the problem is that in these cases models, however, are not fictions—at least in the minimal unequivocal sense of the word as it is adopted in the literary/narrative frameworks—but just the usual idealizations or abstractions, already well-known and well studied, as devices, stratagems, and strategies that lead to efficient results and that are not discarded just because they are not fake chances from the perspective of scientific rationality.[3] Two consequences derive:

[2]It has to be added that Suárez does not conflate scientific modeling with literary fictionalizing and distinguishes scientific fictions from other kinds of fictions—the scientific ones are constrained by both the logic of inference and, in particular, the requirement to fit in with the empirical domain (Suárez 2009, 2010)—in the framework of an envisaged compatibility of "scientific fiction" with realism. This epistemological acknowledgment is not often present in other stronger followers of fictionalism.

[3]I discussed the role of chance-seeking in scientific discovery in (Magnani 2007). For a broader discussion on the role of luck and chance-seeking in abductive cognition see also (Bardone 2011), and (Bardone 2012).

– the role of models as "expediency of the inferences" in peripheral aspects of scientific research, well-known from centuries in science, does not have to be confused with the *constitutive* role of modeling in the central creative processes, when new conceptually revolutionary perspectives are advanced: in this case the *dynamic* role of models in scientific cognition dominates;
– models are—so to say—just models that idealize and/or abstract, but these last two aspects have to be strictly criticized in the light of recent epistemologico/cognitive literature as related to special kinds of epistemic actions, as I have illustrated in (Magnani 2012): abstractness and ideality cannot be solely related to empirical inadequacy and/or to theoretical incoherence (Suárez 2009, p. 168), in a *static* view of the scientific enterprise.

In sum, I will illustrate that there is no need of reframing—in the new complicated and intellectualistic lexicon of fictions (and of the related metaphors)—what is already well-known thanks to the tradition of philosophy of science. We have to remorselessly come back to the spirit of Newton's famous motto "hypotheses non fingo", which has characterized for centuries the spirit of modern science: "I have not as yet been able to discover the reason for these properties of gravity from phenomena, and I do not feign hypotheses. For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction" (Newton 1999, p. 493).

2 Are the In-Vitro Model or a Geometrical Diagram Fictions? Dynamic Versus Static View of Scientific Models

Peirce, speaking of the model-based aspects of deductive reasoning, hypothesized there is an "experimenting upon this image [the external model/diagram] in the imagination", so showing how human geometrical imagination is always triggered by a kind of prosthesis, the external model as an "external imagination". Analogously, taking advantage of a fictional view on models and of the pretence theory Frigg (2010c, p. 266 ff.) interestingly sees imagination as an authorized intersubjective game of make-believe sanctioned by the "prop" (an object, for example material models, movies, paintings, plays, etc.) and its rules of generation. This theory also works as a metaphor of abductive processes (that is cognitive processes that lead to hypotheses), in terms of some concepts taken from the theory of literary and artistic fictions. Again, I think that it is neither necessary to adopt a fictionalist view in the case of science, nor the pretence theory adds something relevant to the issue. For example, scientists that build models in a lab do not pretend anything and are not engaged in the relative make-believe process, if not in the trivial sense that almost every human intersubjective interplay can be seen as such. For instance, in-vitro networks of cultured neurons or the Peircean Euclidean diagram used by the ancient Greek geometers are just the opposite of a mere fiction or of a generic make-believe interplay, they are instead more or less mimetic (possibly creative) "distributed" external models which are expected to provide reliable information about the target system. They aim at discovering some new representations about the neurons in the first case and about the pure concepts of geometry in the second.

The reason of my skepticism can be illustrated taking advantage of some theses derived from classical Kantian philosophy and Thom's mathematical semiophysics. Immanuel Kant was clearly aware of the interplay between internal and external models, exemplified in the case of a formal science like mathematics. In his transcendental terms, Kant says that in geometrical construction "[…] I must not restrict my attention to what I am actually thinking in my concept of a triangle (this is nothing more than the mere definition); I must pass beyond it to properties which are not contained in this concept, but yet belong to it" (Kant 1929, A718-B746, p. 580). Hence, for Kant models in science (in this case, of geometry) are first of all *constructions* that go beyond what the researcher simply "thinks". The same situations can be now illustrated as a case of what I called manipulative abduction (Magnani 2001, 2009), that is is a kind of, usually model-based, abduction that exploits external models endowed with delegated (and often implicit) cognitive roles and attributes: 1. The model is external and the strategy that organizes the manipulations is unknown a priori. 2. The result achieved is new (if we, for instance in this geometrical case, refer to the constructions of the first creators of geometry), and adds properties not contained before in the concept (the Kantian to "pass beyond" or "advance beyond" the given concept (Kant 1929, A154-B194, p. 192)).[4]

Iconicity of models is central for Peirce, who analogously to Kant, maintains that "[…] philosophical reasoning is reasoning with words; while theorematic reasoning, or mathematical reasoning is reasoning with specially constructed schemata" (Peirce 1931–1958, 4.233); moreover, he uses diagrammatic and schematic as synonyms, thus relating his considerations to the Kantian tradition where schemata mediate between intellect and phenomena. Schematism, a fruit of the imagination is, according to Kant, "[…] an art concealed in the depths of the human soul, whose real modes of activity nature is hardly likely ever to allow us to discover, and to have open to our gaze" (Kant 1929, A141-B181, p. 183).

Now we have at our disposal, thanks to epistemology and cognitive science, a lot of knowledge about the cognitive processes which correspond to Kantian schematism[5]. The following is the famous related passage in the *Critique of Pure Reason* ("Transcendental Doctrine of Method"):

> Suppose a philosopher be given the concept of a triangle and he be left to find out, in his own way, what relation the sum of its angles bears to a right angle. He has nothing but the concept of a figure enclosed by three straight lines, and possessing three angles. However

[4]Of course in the case we are using diagrams to demonstrate already known theorems (for instance in didactic settings), the strategy of manipulations is often already available and the result is not new.

[5]On models as epistemic mediators in mathematics cf. (Boumans 2012).

> long he meditates on this concept, he will never produce anything new. He can analyse and clarify the concept of a straight line or of an angle or of the number three, but he can never arrive at any properties not already contained in these concepts. Now let the geometrician take up these questions. He at once begins by constructing a triangle. Since he knows that the sum of two right angles is exactly equal to the sum of all the adjacent angles which can be constructed from a single point on a straight line, he prolongs one side of his triangle and obtains two adjacent angles, which together are equal to two right angles. He then divides the external angle by drawing a line parallel to the opposite side of the triangle, and observes that he has thus obtained an external adjacent angle which is equal to an internal angle—and so on. In this fashion, through a chain of inferences guided throughout by intuition, he arrives at a fully evident and universally valid solution of the problem (Kant 1929, A716-B744, pp. 578–579).

Here "intuition" is the Kantian word that expresses our present reference to what we call "external—scientific, geometrical in this case—model".

We can depict the situation of the philosopher described by Kant at the beginning of the previous passage taking advantage of some ideas coming from Thom's catastrophe theory. As a human being who is not able to produce anything new relating to the angles of the triangle, the philosopher experiences a feeling of frustration (just like the Kölher's monkey which cannot keep the banana out of reach). The bad affective experience "deforms" the organism's regulatory structure by complicating it and the cognitive process stops altogether. The geometer instead "at once constructs the triangle" [the scientist constructs the model] that is, he makes an external representation of a triangle and acts on it with suitable manipulations. Thom thinks that this action is triggered by a "sleeping phase" generated by possible previous frustrations which then change the cognitive status of the geometer's available and correct internal idea of triangle (like the philosopher, he "has nothing but the concept of a figure enclosed by three straight lines, and possessing three angles", but his action is triggered by a sleeping phase). Here the idea of the triangle is no longer the occasion for "meditation", "analysis" and "clarification" of the "concepts" at play, like in the case of the "philosopher". Here the inner concept of triangle—symbolized as insufficient—is amplified and transformed thanks to the sleeping phase (a kind of Kantian imagination active through schematization) in a prosthetic triangle to be put outside, in some external support. The instrument (here an external diagram) becomes the extension of an organ:

> What is strictly speaking the end—to take the banana [in our case, to find the sum of the internal angles of a triangle]—must be set aside in order to concentrate on the means of getting there. Thus the problem arises, a sort of vague notion altogether suggested by the state of privation. […] As a science, heuristics does not exist. There is only one possible explanation: the affective trauma of privation leads to a folding of the regulation figure. But if it is to be stabilized, there must be some *exterior form* [italics added] to hold on to. So this anchorage problem remains whole and the above considerations provide no answer as to why the folding is stabilized in certain animals or certain human beings whilst in others (the majority of cases, needless to say!) it fails (Thom 1988, pp. 63–64).[6]

[6] A full analysis of the Kölher's chimpanzee getting hold of a stick to knock a banana hanging out of reach in terms of the mathematical models of the perception and the capture catastrophes is given in (Thom 1988, pp. 62–64). On the role of emotions, for example frustration, in scientific discovery cf. (Thagard 2002).

3 The Epistemic Danger of Disregarding the Dynamic Aspects of the Scientific Enterprise

Taking advantage of Thom's considerations, we can clearly see that the constructed external scientific model in the case of creative processes is exactly the opposite both of a fiction and of a generic process of make-believe (neither is a mere surrogate (Contessa 2007) or a bare credible world (Sugden 2000, 2009). It is instead a *regulatory* tool *stabilized* in "some exterior form", a kind of a reliable anchorage, not intentionally established as fiction, as a romance writer could intentionally do, assessing the fictional character of Anna Karenina. In the epistemological fictionalism about models the use of the label "fiction" is usually legitimated by the fact that there are no empirical systems corresponding for example to the ideal pendulum (and its equation).

Unfortunately the label sets up a paradox we can clearly see taking advantage of the case of scientific models seen as "missing systems", another new metaphor that echoes the fictional one— indeed the description of a missing system might be a fiction. Thomson-Jones (2010) emphasizes that science is full of "descriptions of missing systems", that at the end are thought as *abstract models*.[7] Further, Mäki (2009) usefully acknowledges that scientific models are "pragmatically and ontologically constrained representations", and further complicates the missing systems framework adding a supplementary metaphoric conceptual apparatus: missing systems are also "surrogate" systems expressed as credible worlds, as models. Similar arguments are advanced by Godfrey-Smith (2009, p. 114): "To say that talk of model systems is a psychologically exotic way of investigating conditionals (and the like) is not itself to solve the problem. It is natural to think that the useable output we get from modeling is generally a conditional—a claim that if such-and such a configuration existed, it would behave in a certain way. The configurations in question, however, are usually known *not* to exist, so the problem of explaining the empirical usefulness of this kind of knowledge reappears".

I contend that, at least in the dynamics of a discovery cognitive process, the missing system (Thomson-Jones) is not, paradoxically, the one represented by the "model", but instead the target system itself, still more or less largely unknown and un-schematized, which will instead appear as "known" in a new way only after the acceptation of the research process results, thus admitted into the theory T and considered worth to *staying* in T thereafter. The same can be said of models as configurations (Godfrey-Smith), which certainly are conditional, but at the same time do not have to be considered as "known *not* to exist", in Godfrey-Smith's sense, because simply in the moment in which a scientific model is introduced in a discovery process it is instead exactly the only object we plausibly *know* to exist (for example a diagram in a blackboard, or a in-vitro artifact, or a mental imagery). Only in the framework of an oldfashioned strong metaphysical realism we can state that, once a final scientific result has been achieved, together with the description of the related

[7] Cartwright (1983), more classically and simply, speaks of "prepared description" of the system in order to make it amenable to mathematical treatment.

experimental side, everything that does not fit that final structure is a fiction, and so models that helped reach that result itself. Morrison is pretty clear about the excessive habit of labeling fictional scientific models simply because they are superficially seen as "unrealistic": "Although there is a temptation to categorize any type of unrealistic representation as a 'fiction', I have argued that this would be a mistake, primarily because this way of categorizing the use of unrealistic representations tells us very little about the role those representations play in producing knowledge" (Morrison 2009, p. 133).

In the framework of an account of scientific representation in terms of partial structures and partial morphisms Bueno and French (2011, p. 27) admit that they agree in fact that an important role for models in science is to allow scientists to perform the so-called "surrogative" reasoning, but they add the following constraint: "Indeed, we would claim that representing the 'surrogative' nature of this reasoning effectively rides on the back of the relevant partial isomorphisms, since it is through these that we can straightforwardly capture the kinds of idealizations, abstractions, and inconsistencies that we find in scientific models". So to say, we can speak of surrogates, fictions, credible worlds, etc., but it is only through the suitable partial isomorphism we can detect after a success of the model, that we can be assured to be in presence of a scientific representation or model.

Further, Kuorikoski and Lehtinen (2009, p. 121) contend that: "The epistemic problem in modelling arises from the fact that models always include false assumptions, and because of this, even though the derivation within the model is usually deductively valid, we do not know whether our model-based inferences reliably lead to true conclusions". However, the false premises (also due to the presence in models of both substantive and auxiliary assumptions) are not exploited in the cognitive process, because, in various heuristic processes, only the *co-exact* ones are exploited. The notion of co-exact properties, introduced by Manders (2008), is worth to be further studied in fields that go beyond the realm of discovery processes of classical geometry, in which it has been nicely underscored. Mumma (2010, p. 264) illustrates that Euclid's diagrams contribute to proofs only through their co-exact properties. Indeed

> Euclid never infers an exact property from a diagram unless it follows directly from a co-exact property. Exact relations between magnitudes which are not exhibited as a containment are either assumed from the outset or are proved via a chain of inferences in the text. It is not difficult to hypothesize why Euclid would have restricted himself in such a way. Any proof, diagrammatic or otherwise, ought to be reproducible. Generating the symbols which comprise it ought to be straightforward and unproblematic. Yet there seems to be room for doubt whether one has succeeded in constructing a diagram according to its exact specifications perfectly. The compass may have slipped slightly, or the ruler may have taken a tiny nudge. In constraining himself to the co-exact properties of diagrams, Euclid is constraining himself to those properties stable under such perturbations.

Moreover, some false assumptions are considered as such *only if* seen in the light of the still "to be known" target system, and so they appear false only in a *post hoc* analysis, but they are perfectly true in the model itself in its relative autonomy during the smart heuristic cognitive process related to its exploitation. So various aspects of the model are the legitimately true basis for the subsequent exploration of its behavior and performance of the abductions to plausible hypotheses concerning the target system. I agree with Morrison: “I see this not as a logical problem of deriving true conclusions from false premises but rather an epistemic one that deals with the way false representations transmit information about concrete cases” (Morrison 2009, p. 111)[8].

In sum, I think it is misleading to analyze models in science by disregarding the dynamic aspects of the scientific enterprise. Scientific models in a static perspective (for example when inserted in a textbook) certainly appear—but just appear—fictional, because they are immediately compared with the target systems and their complicated experimental apparatuses: in this case also the *ideal* character of models becomes manifest and so the *explanatory* function of them (cf. (Weisberg 2007)). Contrarily, scientific models seen inside the living dynamics of scientific creativity, which is the key topic of epistemology at least since Karl Popper and Thomas Kuhn, appear *explicit* and *reproducible* machineries intentionally built and manipulated to the gnoseological aims of increasing scientific knowledge *not yet available*.

Morrison (2009) is certainly not inclined to see models as fictions because she emphasizes that in science they are specifically related to (“finer graded”) ways of understanding and explaining “real systems”, far beyond their more collateral predictive capabilities and their virtues in approximating. She indeed further clarifies that the models which is appropriate to label as *abstract* resist—in the so-called process of de-idealization—corrections or relaxing of the unrealistic assumptions (such as in the case of mathematical abstractions or when models furnish the sudden chance for the applicability of equations), because they are “necessary” to arrive to certain results. The fact that in these models “relevant features” are subtracted to focus on a single—and so isolated—set of properties or laws, as stressed by Cartwright (1989), is not their central quality, because what is at stake is their capacity to furnish an overall new depiction of an empirical (and/or theoretical, like in case of mathematics or logic) framework: “[…] We have a description of a physically unrealizable situation that is required to explain a physically realizable one” (p. 130).

Other models, easier to define, which is better to classify as *idealizations*, allow “[…] for the addition of correction factors that bring the model system closer (in representational terms) to the physical system being modelled or described” (Morrison 2009, p. 111). It is for example the case of simple pendulum, where we know how to add corrections to deal with concrete phenomena. Idealizations distort or omit properties, instead abstractions introduce a specific kind of representation “that is not amenable to correction and is necessary for explation/prediction of the target sys-

[8]Further information about the problem of the mapping between models and target systems through *interpretation* are provided by Contessa (2007, p. 65)—interpretation is seen as more fundamental than surrogative-reasoning: “The model can be used as a generator of hypotheses about the system, hypotheses whose truth or falsity needs to be empirically investigated”. By using the concept of interpretation (analytically and not hermeneutically defined) the author in my opinion also quickly adumbrates the creative aspects in science, that coincide with the fundamental problem of model-based and manipulative abduction (cf. (Magnani 2009, chapters one and two)).

tem" (p. 112), and which provides information and transfer of knowledge. Morrison's characterization of scientific models as abstract is in tune with my emphasis on models as *constitutive*, beyond the mere role played by models as idealizations, which instead allow corrections and refinements. In this perspective, "abstract" models, either related to prepare and favor mathematization or directly involving mathematical tools, have to be intended as poietic ways of producing new intelligibility of the essential features of the target systems phenomena, and not mere expedients for facilitating calculations. If idealization *resembles* the phenomena to be better understood, abstract models *constitute* the resemblance itself, as I will illustrate in the following subsection.

When Mäki (2009, p. 31) contends that "It may appear that a fantastically unreal feature is added to the model world, but again, what happens is that one thereby removes a real-world feature from the model world, namely the process of adjustment", I have to note that, at least in various creative processes, the model is not necessarily implemented through "removal" or "neutralization" of real-world features, because some features of the target system—that is the supposed to be real world—have simply not been discovered yet, and so, paradoxically, they are the ones still "missing". Consequently it is impossible to imagine that some aspects of the model derive from a removal of features of the real world, that can just be those features that will derive later on exactly thanks to that cognitive process that constructed the model itself to reach that objective. At the same time, and for the same reason, it is difficult to always state that models depict a "surrogate" systems, because the systems we want to subrogate *are largely not yet known*.

3.1 Resemblance and Feyerabend's Counterinduction

Even the concept of resemblance (similarity, isomorphism, homomorphism, etc.) as it is employed in the epistemological framework of missing systems (and related topics, fictions, surrogate systems, credible world, make-believe models, etc.) is in part misleading. "M resembles, or corresponds to, the target system R in suitable respects and sufficient degrees. This second aspect of representation enables models to serve a useful purpose as representatives: by examining them as surrogate systems one can learn about the systems they represent" (Mäki 2009, p. 32): I contend that resemblance is constitutively partial *also* because it is basically impossible to appropriately resemble things that are not yet known.[9]

It is not always acknowledged in the current literature that isomorphism, homomorphism and similarity with the target systems *are not* necessarily established—so to say—a priori, because the target system has still to be built. Actually—this is an important point—it is just the work of models that of creating, in a poietic way, the "resemblance" to the target system. Some discovered features of the target system

[9]On the puzzling relationships between similarity and representations, in the framework of intentionality, cf. (Giere 2007).

resemble the model not because the model resembled them a priori but only *post hoc*, once discovered thanks to the modeling activity itself, in so far as resemblance has been *instituted* by the model: the new features appear well-defined only in the static analysis of the final developed theory. It is at this stage that resemblance acquires the actual status of resemblance, in the common sense of the word: similarity of two *given* entities/structures. Morrison too contends that "To say that fictional models are important sources of knowledge in virtue of a particular kind of similarity that they bear to concrete cases or systems is to say virtually nothing about how they do that. Instead what is required is a careful analysis of the model itself to uncover the kind of information it yields and the ways in which that information can be used to develop physical hypotheses" (Morrison 2009, p. 123).

In this perspective we paradoxically face the opposite of the received view, in the result of a considerable part of scientific discovery processes it is the newly known target system that resembles to the model, which itself originated that resemblance.[10] Often models are useful to discover new knowledge just because they do not—or scarcely— resemble the target system to be studied, and are instead built to the aim of finding a new general capacity to make "the world intelligible".[11]

In *Against Method* (1975), Feyerabend attributes a great importance to the role of contradiction, against the role of similarity. He establishes a "counterrule" which is the opposite of the neopositivistic one that it is "experience" (or "experimental results") which measures the success of our theories, a rule that constitutes an important part of all theories of corroboration and confirmation. The counterrule "[…] advises us to introduce and elaborate hypotheses which are inconsistent with well-established theories and/or well-established facts. It advises us to proceed counterinductively" (Feyerabend 1975, p. 20). Counterinduction is seen more reasonable than induction, because appropriate to the needs of creative reasoning in science: "[…] we need a dream-world in order to discover the features of the real world we think we inhabit" (p. 29). We know that counterinduction, that is the act of introducing, inventing, and generating new inconsistencies and anomalies, together with new points of view incommensurable with the old ones, is congruous with the aim of inventing "alternatives" (Feyerabend contends that "proliferation of theories is beneficial for science"), and very important in all kinds of creative reasoning. Feyerabend stresses the role of "dreaming", but these dreams are Galileo's dreams, they are not fictions: as I have already pointed out Feyerabend clearly distinguished between scientific dreams (as modeling) and propaganda, that can instead be organized thanks to fictions, inconsistent thought experiments, mistakes, aggressive fallacies, and so on,

[10] I endorse many of the considerations by Chakravartty (2010), who stresses the unwelcome division between informational and functional perspective on models and representations in science, which negatively affects the epistemology of scientific modeling.

[11] I am convinced that knowledge about concepts such as resemblance, imaginability, conceivability, plausibility, persuasiveness, credit worthiness (Mäki 2009, pp. 39–40) would take advantage of being studied in the framework of the rigorous and interdisciplinary field of abductive cognition (Magnani 2009), which surprisingly is largely disregarded in the studies of the "friends of fiction", with the exception of Sugden (2000; 2009).

but that do not play any epistemic role in the restricted cognitive process of scientific discovery, I have called "epistemic" warfare (Magnani 2017, chapter four).

Coming back to the problem of models as surrogates, Mäki (2009, p. 35) says:

> The model functions as a surrogate system: it is construed and examined with a desire to learn about the secrets of the real world. One yearns for such learning and sets out to build a model in an attempt to satisfy the desire. Surrogate models are intended, or can be employed to serve, as bridges to the world.

First, I would add some auxiliary notes to the expression "secrets of the real world". I would warn about the preferability of being post-Kantian instead than pre-Kantian by admitting that, through science, we are *constructing* our rational knowledge of the world, which consequently is still an objective world independent of us, but constructed. If we say we build surrogate systems to learn about the secret of nature, a strong realist assumption seems to be presupposed: the models would be surrogates because they are not "reliably reflecting the true reality of the world we are discovering". We rejoin Giere's observation who suspects fictionalists are paradoxically obsessed by "the truth, the whole truth, and nothing but the truth":

> It seems to me that the assimilation of scientific models to works of fiction presupposes an exaggerated conception of nonfiction. On this conception, a genuine work of nonfiction has to provide "the truth, the whole truth, and nothing but the truth". Thus, the realization that scientists are mostly in the business of constructing models that never provide a perfect fit to the world leads to the unwarranted conclusion that scientists are in the business of producing fictional accounts of the world (2009, p. 254).

Scientific theories would reflect this hyper-truth that in turn would reflect true reality (curious! Is not science the realm or self-correcting truths?)[12] In this way it becomes easy to say that everything else in science different from complete established true theories—which would reflect "real world"—is fiction, surrogate, belief, mere credible world, etc.

I would reserve the label of surrogate models to those models employed in some "sciences" that fail in providing satisfactory knowledge about target systems. "There is a long tradition in economics of blaming economists for failing in just this way: giving all their attention to the properties of models and paying none to the relations of the model worlds to the real world" (Mäki 2009, p. 36). Mäki calls the systems described by such models "substitute systems": I will just reserve for them the expression "surrogate systems", because they fake a scientific knowledge that is not satisfactorily achieved, from various perspectives.

I argued above about the epistemological poverty of the concept of model as make-believe: indeed I have already said that make believe processes trivially occur in almost every human intersubjective interplay. Here I can further stress that the idea of credible world is very wide: every cognitive process that aims at providing scientific—but also non scientific—knowledge aims at the same time at providing credible worlds. The problem in science is how to construct the subclass of *epistemologically* credible worlds, that is, *scientific* models, which successfully lead to

[12] We should not forget what Morrison reminds us: "Laws are constantly being revised and rejected; consequently, we can never claim that they are true or false" (Morrison 2009, p. 128).

scientific theories. In this spirit Sugden (2009, p. 10) usefully suggests that an epistemologically "good" credible world would have to be provided by models that are able to trigger hypotheses about the "causation of actual events", that is in cases in which "the fictional world of the model is one that *could* be real". Cartwright's classical perspective (Cartwright 2009a) concerning capacities is fruitfully adopted:

> For her, the function of a model is to *demonstrate the reality* of a capacity by isolating it—just as Galileo's experiment demonstrates the constancy of the vertical component of the acceleration of a body acted on by gravity. Notice how Cartwright speaks of *showing that* C has the capacity to produce E, and of deriving this conclusion from *accepted principles*. A satisfactory isolation, then, allows a real relationship of cause and effect to be demonstrated in an environment in which this relationship is stable. In more natural conditions, this relationship is only a latent capacity which may be switched on or off by other factors; but the capacity itself is stable across a range of possible circumstances. Thus, the model provides a "theoretical grounding" for a general hypothesis about the world (Sugden 2009, p. 20).

Sugden prudently considers too strong these perspectives on models as tools for *isolating* the "capacities" of causal factors in the real world, and provides other conceptual devices to save various aspects of epistemological—supposed to be weak—"sciences", for example some parts of biology, psychology, or economics, which not ever fulfill the target of revealing capacities. To save these sciences he says that models can simply provide "conceptual explorations", which ultimately contribute to the development of genuinely explanatory theories or credible counterfactual worlds which can trigger inductive (or, better, "abductive") inferences to explain the target systems. I think that it is virtuous to be prudent about strong methodological claims such as the ones advanced by Cartwright, but the epistemological problem remains open: in the cases of models as conceptual explorations are they used to depict credible worlds able to reach satisfactory theorization of target systems, or are they just providing ambitious but unjustified hypotheses, devoid of various good epistemological requisites?

Adopting Cartwright's rigid demarcation criterium clearly and recently restated in "If no capacities then no credible worlds" (Cartwright 2009a), it would seem that no more citizenship is allowed to some post-modern exaggeration in attributing the label "scientific" to various proliferating areas of academic production of knowledge, from (parts of) psychology to (parts of) economics, and so on, areas which do not—or scarcely—accomplish the most common received epistemological standards, for example, the *predictivity* of the phenomena that pertain the explained systems. Are we sure that this demarcation is too rigid or it is time to criticize some excess in the proliferation of models supposed to be "scientific"? It is in this perspective that the epistemological use of the so-called credible worlds appears theoretically suspect, but ideologically clear, if seen in the "military" framework of the academic struggle between disciplines, dominated—at least in my opinion—by a patent proliferation of "scientific" activities that just produce bare "credible" or "surrogate" models, looking aggressively for scientificity, when they actually are, at the best, fragments of bad philosophy.

An example is furnished by the precarious condition of various parts of psychological research. Miller (2010, p. 716) explores three contentions: "[…] that the dominant discourse in modern cognitive, affective, and clinical neuroscience assumes that we know how psychology/biology causation works when we do not; that there are serious intellectual, clinical, and policy costs to pretending we do know; and that crucial scientific and clinical progress will be stymied as long as we frame psychology, biology, and their relationship in currently dominant ways". He further rigorously illustrates the misguided attempts to localize psychological function via neuroimaging and the misunderstandings about the role of genetics in psychopathology, sadly intertwined with untoward constraints on health-care policy and clinical service delivery.

4 Conclusions

In this paper I have contended that scientific models are not fictions. I have illustrated that it is misleading to analyze models in science by disregarding the dynamic aspects of the scientific enterprise: indeed the static perspective leads to an overemphasis of the possible fictional character of models because the creative/factive role of modeling is candidly or intentionally disregarded. I have also argued that other various related epistemological approaches to model-based scientific cognition (in terms of surrogates, credible worlds, missing systems, make-believe) present severe inadequacies, which can be detected taking advantage of a dynamic view of science and of the concept of manipulative abduction. A further way of delineating a more satisfactory analysis of fictionalism and its discontents has been sketched by proposing the concept of "epistemic warfare", which sees scientific enterprise as a complicated struggle for rational knowledge in which it is crucial to distinguish epistemic (for example scientific models) from extra-epistemic (for example fictions, falsities, propaganda) weapons. Finally, I have adopted some thoughts of a classical author, which are of help in dealing with scientific modeling: Feyerabend's useful concept of counterinduction in criticizing the role of resemblance in model-based cognition. In this perspective I have paradoxically reached the opposite of the received view: in the result of a considerable part of scientific discovery processes it is the newly known target system that resembles to the model, which itself originated that resemblance.

Acknowledgements Parts of this article were excerpted from L. Magnani (2012), Scientific models are not fictions. Model-based science as epistemic warfare, in L. Magnani and Li Ping (eds.) (2012), *Philosophy and Cognitive Science. Western and Eastern Studies*, Series "Sapere", Vol. 2, Springer, Heidelberg/Berlin, pp. 1–38, and originally published in chapter two and three of L. Magnani, *The Abductive Structure of Scientific Creativity An Essay on the Ecology of Cognition*. Copyright (2017), Springer, Cham, Switzerland.

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