Scientific Models Are Not Fictions
Model-Based Science as Epistemic Warfare

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> I seem to discern the firm belief that in [natural] philosophizing one must support oneself upon the opinion of some celebrated author, as if our minds ought to remain completely sterile and barren unless wedded to the reasoning of some other person. Possibly he [Lothario Sarsi] thinks that [natural] philosophy is a book of fiction by some writer, like the Iliad or Orlando Furioso, productions in which the least important thing is whether what is written there is true. Well, Sarsi, that is not how matters stand. [Natural] Philosophy is written in this grand book, the universe, which stands continually open to our gaze. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth.
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> Galileo Galilei, *The Assayer*

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. The paper discusses these approaches showing some of their epistemological inadequacies, also taking advantage of recent results in cognitive science. The main aim is to revise and criticize fictionalism, also reframing the received idea of abstractness and ideality of models with the help of recent results coming from the area of distributed cognition (common coding) and abductive cognition (manipulative). The article also illustrates how scientific modeling activity can be better described 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. Finally I will illustrate that it is misleading to analyze models in science by adopting a confounding

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mixture of static and dynamic aspects of the scientific enterprise. Scientific models in a static perspective (for example when inserted in a textbook) certainly appear fictional to the epistemologist, but their fictional character disappears in case a dynamic perspective is adopted. A reference to the originative role of thought experiment in Galileo's discoveries and to 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.

1 Introduction

Current epistemological analysis of the role models in science is often philosophically unproblematic and misleading. 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; Giere, 2009; Giere, 2007] and idealizations [Portides, 2007; Weisberg, 2007; Mizrahi, 2011], to the more recent, fictions [Fine, 2009; Woods, 2010; Woods and Rosales, 2010b; Contessa, 2010; Frigg, 2010a; Frigg, 2010b; Frigg, 2010c; Godfrey-Smith, 2006; Godfrey-Smith, 2009; Woods and Rosales, 2010a; Suárez, 2009a; Suárez, 2010], surrogates [Contessa, 2007], credible worlds [Sugden, 2000; Sugden, 2009; Kuorikoski and Lehtinen, 2009], missing systems [Mäki, 2009; Thomson-Jones, 2010], as make-believe [Frigg, 2010a; Frigg, 2010b; Frigg, 2010c; Toon, 2010], parables [Cartwright, 2009b], as functional [Chakravartty, 2010], as epistemic actions [Magnani, 2004a; Magnani, 2004b], 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 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.[1] In the meantime I aim at substantiating my critique to fictionalism also outlining the first features of my own approach to the role of scientific models in terms of what I call "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, etc.) weapons.[2] I certainly 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 [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. These tricks are very useful and efficient, but one count is the *epistemic* role of reasons scientist takes advantage of, such as scientific models, which for example directly govern the path to provide a new intelligibility of the target systems at hand, another count is the *extra-epistemic* role of propaganda 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 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.[3]

[1] This does not mean that the standard epistemological concept of abstract model is devoid of sense, but that it has to be considered in a Pickwickian sense.

[2] 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.: in this perspective I basically agree with the distinction between epistemic and non-epistemic values as limpidly depicted in [Steel, 2010].

[3] 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. On this issue cf. also [Bertolotti, 2012], this volume.

Suárez [Suárez, 2009a] 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):[4] the problem is that in this 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.[5] Two consequences derive:

- 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;
- 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 special kinds of epistemic actions, as I will illustrate in section 3 below: abstractness and ideality cannot be solely related to empirical inadequacy and/or to theoretical incoherence [Suárez, 2009a, 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 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

[4] It has to be added that Suárez does not certainly conflate scientific modeling with literary fictionalizing. He clearly 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, 2009a; Suárez, 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.

[5] 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], this volume.

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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 Models Are Not Fictions. The Inconsistency of the Argument of Imperfect Fit

Should scientific models be regarded as works of fictions? At the beginning of the previous section 1 said 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. Let us outline in this section the first problem, related to the *fictionalist* nature of models. I will return to this problem in section 3, in which also the problem of the *abstractness* of models will be deeply illustrated: as for now we can note that, in a philosophical naturalistic framework, where all phenomena and thus also cognition, gain a fundamental eco-physical significance, models are always material objects, either when we are dealing with concrete diagrams, physical or computational models, or when we face human "mental models", which at the end "are" particular, unrepeatable, but ever-changing configurations and transformations of neural networks and chemical distributions at the level of human brains. Indeed, defending in this paper an interdisciplinary approach we are simply re-engaged in one of the basic tenets of the philosophical mentality, now enriched by a naturalistic commitment, which acknowledges the relevance of scientific results of cognitive research.

If, ontologically, models are imaginary objects in the way objects of fictions are imaginary objects, I cannot see them as situated in any "location" different from the brain, so that they are imaginary in so far as they are just "mental" models. Like Giere contends:

> In spite of sharing an ontology as imagined objects, scientific models and works of fiction function in different cultural worlds. One indication of this difference is that, while works of fiction are typically a product of a single author's imagination, scientific models are typically the product of a collective effort. Scientists share preliminary descriptions of their models with colleagues near and far, and this sharing often leads to smaller or larger changes in the descriptions. The descriptions, then, are from the beginning intended to be public objects. Of course, authors of fiction may share their manuscripts with family and colleagues, but this is not part of the ethos of producing fiction. An author would not be professionally criticized for delivering an otherwise unread manuscript an editor. Scientists who keep everything to themselves before submitting a manuscript for publication are regarded as peculiar and may be criticized for being excessively secretive [Giere, 2009, p. 251].

Moreover, to consider models as fictions would destroy the well regarded distinction between science and science fiction. This attitude can present cultural dangers: is science just a matter of fictions? Both kinds of fictions (scientific and literary) certainly provide insights on something “real”, that is they aim at *representing* aspects of the world (for example *War and Peace*, Giere says, provides insight into the “human condition”) but often various genres of literary fictions are simply finalized to entertain. Even if both contain imaginary objects, the processes that govern their formation and what from them is derived are very dissimilar, as I will further describe in section 3. Representation in science is always related to criteria of scope, accuracy, precision and detail – Giere says – and further notes: “Remember the many models that were proposed and rejected in the race for the double helix because they failed adequately to represent the structure of DNA molecules. In the realm of fantasy, such criticisms are not appropriate. It is no criticism of the Harry Potter novels that there is no community of genuine wizards. Nor is it a criticism of War and Peace that its main characters did not exist” [Giere, 2009, p. 252]. The fact that a scientific model, relating to the “real” world, seems to be a fiction – that is to say, the fact it does not perfectly fit to any real system – does not authorize us to regard the overall model as a work of fiction, because it does not function like a work of fiction such as novels or so.

Finally, I strongly agree with Giere that “In fact, the argument from imperfect fit to a functionally fictional status for models proves far too much” [Giere, 2009, p. 254], because it is typical of every cognition the involvement of ideal categorization and schematization, so that most of what everyone thinks and perceives should be regarded as fictional:[6]

> 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 (cit.)

The problem is that models help reach success in experimental outcomes, because they instead fit to designated aspects of the world:

> [. . .] the view that scientific models are ontologically like works of fiction in being imaginary creations not only does not uniquely support fictionalism, but is compatible with a moderate realism. There is nothing in this notion of a scientific model that prevents identifying elements of models with things traditionally classified as “unobservable”. On the other hand, as discussed earlier in this chapter, some elements of models may not be identified with anything in the world (cit., p. 256).

[6] Mizrahi [Mizrahi, 2011] seems to support – in the linguistic perspective about the role of “facticity” in scientific cognition – a similar point of view about the coherence of seeing scientific “idealized” models as “quasi-factive”: “[. . .] if [scientific] understanding is (quasi) factive, then we can attribute this sort of cognitive success to scientists when they employ idealizations, such as the Ideal Gas Law, precisely because they mirror the facts to some extent. That is to say, in the case of the Ideal Gas Law, it is precisely because of the agreement between the predictions of the gas laws and the behavior of gases (under specified conditions of temperature and pressure) that we attribute cognitive success to scientists in this case. Otherwise, it seems, we would say that scientists don’t understand the behavior of gases at all”.

I confess that I would not encourage epistemologists to engage in debates about "realism" against "fictionalism", or about problems like "is fictionalism compatible with realism?", etc. [Suárez, 2010], because the adoption of these old pre-Kantian categories is in my opinion philosophically sterile. After all, the same discussions about a privileged *level* of reality (able to demarcate everything else, for example "fictions") could be easily substituted by an equally coherent view about the consistency of various *levels* of reality, where the referents of fictions could be easily included.

It is not that "fictions provide inferential shortcuts in models; and the fact that this is the main or only reason for their use distinguishes them as fictional" [Suárez, 2010, p. 239], even if Vaihinger would agree with this functionalist perspective on fictions.[7] Indeed, even if it is not decisive to say "that the inferential characterisation provides a way to distinguish precisely scientific from non-scientific uses of fiction", models used in non-scientific practices may also trigger inferences, and the problem here is more fundamental. In science, models are not used and intended as fictions, they are just labeled as fictions because of a juxtaposition of some recent philosophers of science, who certainly in this way render the scientific enterprise more similar to other more common modes of human cognition: after all fictions are ubiquitous in human cognition, and science is a cognitive activity like others. Unfortunately science never aimed to provide "fictions" at the basic levels of its activities, so that the recent fictionalism does not add new and fresh knowledge about the status of models in science, and tends to obfuscate the distinctions between different areas of human cognition, such as science, religion, arts, and philosophy. In the end, "epistemic fictionalism" tends to enforce a kind "epistemic concealment", which can obliterate the actual gnoseological finalities of science, shading in a kind of debate about entities and their classification that could remind of medieval scholasticism.

3 Models Are Distributed and Never Abstracts: Model-Based Science as Epistemic Warfare

At the beginning of the previous section I advanced the hypothesis that models, both in scientific reasoning and in human perception, are neither mere fictions, simple surrogates or make-believe, nor they are unproblematic idealizations, and I also specifically contended that models are never *abstract* or *ideal*, contrarily to the received view: they do not live – so to say – in a kind of mysterious Popperian *World*

[7] Suárez's approach to scientific models as fictions is actually more sophisticated than it may appear from my few notes. Basically, Suárez does not defend the view according to which models are fictions: even if he defends the view that models contain or lead to fictional assumptions, he explicitly rejects the identification of models and fictions, preferring instead to stay "quietist" about the ontology of models, and focusing rather on modeling as an activity – see in particular his introduction to the 2009 Routledge volume he edited entitled *Fictions in Science* [Suárez, 2009b].

3. Let us deepen this second problem concerning the abstract and ideal nature of models in scientific reasoning.

First of all, 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.

I contend that the so-called *abstract model* can be better described in terms of what Nersessian and Chandrasekharan [Nersessian and Chandradekharan, 2009] call *manifest model*: when the scientific collective decides whether the model is worth pursuing, and whether it would address the problems and concepts researchers are faced with, it is an internal model and it is manifest because it is shared and "[. . .] allows group members to perform manipulations and thus form common movement representations of the proposed concept. The manifest model also improves group dynamics" [Chandrasekharan, 2009, p. 1079]. Of course the internal representation presents slight differences in each individual's brain, but this does not impede that the various specific representations are clearly thought to be "abstract" insofar as they are at the same time "conceived" as referring to a unique model. This model, at a specific time, is considered "manifest", in an atmosphere of common understanding. Nevertheless, *new* insights/modifications in the internal manifest model usually occur at the individual level, even if the approach to solve a determinate problem through the model at stake is normally shared by a specific scientific collective: the singular change can lead to the solution of the problems regarding the target system and so foster new understanding. However, new insights/modifications can also lead to discard the model at stake and to build another one, which is expected to be more fruitful and which possibly can become the new manifest model. Moreover, some shared manifest models can reach a kind of stability across the centuries and the scientific and didactic communities, like in the case of the ideal pendulum, so that they optimally reverberate the idea of high *abstractness* of scientific models.

If we comply with a conception of the mind as "extended", we can say that the mind's guesses – both instinctual and reasoned – can be classified as plausible hypotheses about "nature" because the mind grows up *together with* the representational delegations to the external world that mind itself has made throughout the history of culture by constructing the so-called cognitive niches.[8] Consequently, as I have already anticipated few lines above, not only scientific models are never abstracts/ideal, they are always distributed. Indeed, in the perspective of distributed (and embodied) cognition [Hutchins, 1999] a recent experimental cognitive research [Chandrasekharan, 2009] further provides deep and fresh epistemological insight into the old problem of abstractness and ideality of models in scientific reasoning. The research illustrates two concrete external models, as functional and behavioral approximations of neurons, one physical (in-vitro networks of cultured neurons) and the other consisting in a computational counterpart, as recently built and applied in a neural engineering laboratory.[9] These models are clearly recognized as external systems – external artifacts more or less intentionally[10] prepared, exactly like concrete diagrams in the case of ancient geometry – interacting with the internal corresponding models of the researchers, and they aim at generating new concepts and control structures regarding target systems.

The external models in general offer more plasticity than the internal ones and lower memory and cognitive load for the scientist's minds. They also incorporate constraints imposed by the medium at hand that also depend on the intrinsic and immanent cognitive/semiotic delegations (and the relative established conventionality) performed by the model builder(s): artificial languages, proofs, new figures, examples, computational simulations, etc.[11] It is obvious that the information (about model behavior) from models to scientists flow through perception (and not only through visualization as a mere representation – as we will see below, in the case of common coding also through "movements in the visualization are also a way of generating equivalent movements in body coordinates" [Chandrasekharan, 2009, p. 1076]).

Perception persists in being the vehicle of model-based and motor information to the brain. We see at work that same perception that Peirce speculatively analyzed as that complicated philosophical structure I illustrated in my book on abductive cognition.[12] Peirce explains to us that some basic human model-based ways of knowing, that is *perceptions*, are abductions, and thus that they are hypothetical

[8] The concept of cognitive niche is illustrated in detail in [Odling-Smee *et al.*, 2003].

[9] An analysis of the differences between models in biology and physics and of the distinction between natural, concrete, and abstract models is illustrated in [Rowbottom, 2009]; unfortunately, the author offers a description of abstract models that seems to me puzzling, and falls under the criticism I am illustrating in the present paper.

[10] I have to note that manipulative abduction – see below subsection 3.1 – also happens when we are *thinking through doing* (and not only, in a pragmatic sense, about doing). This kind of action-based cognition can hardly be intended as completely intentional and conscious.

[11] On the cognitive delegations to external artifacts see [Magnani, 2009, chapter three, section 3.6]. A useful description of how formats also matter in the case of external hypothetical models and representations, and of how they provide different affordances and inferential chances, cf. [Vorms, 2010].

[12] The complicated analysis of some seminal Peircean philosophical considerations concerning abduction, perception, inference, and instinct, which I consider are still important to current cognitive and epistemological research, is provided in [Magnani, 2009, chapter five].

and withdrawable. Moreover, given the fact that judgments in perception are fallible but indubitable abductions, we are not in any psychological condition to conceive that they are false, as they are unconscious habits of inference. Hence, these fundamental – even if non scientific – model-based ways of cognizing are constitutively intertwined with inferential processes. *Unconscious* cognition legitimately enters these processes (and not only in the case of some aspects of perception – remind the process, in scientific modeling, of "thinking through doing", I have just quoted above in footnote 10), so that model-based cognition is typically performed in an unintentional way. The same happens in the case of emotions, which provide a quick – even if often highly unreliable – abductive appraisal/explanation of given data, which is usually anomalous or inconsistent. It seems that, still in the light of the recent results in cognitive science I have just described, the importance of the model-based character of perception stressed by Peirce is intact. This suggests that we can hypothesize a continuum from construction of models that actually *emerge* at the stage of perception, where models are operating with the spontaneous application of abductive processes to the high-level model activities of more or less intentional modelers ([Park, 2011], and [Bertolotti, 2012], this volume), such as scientists.[13] Finally, if perception cannot be wrong, given the fact that judgments in perception are fallible but indubitable abductions, as I have just illustrated, then these judgments should not be regarded as *fictional*.

3.1 Perception-Action Common Coding as an Example of "On-Line" Manipulative Abduction

The cognitive mechanism carefully exploited and illustrated in [Chandrasekharan, 2009] takes advantage of the notion of *common coding*,[14] recently studied in cognitive science and closely related to embodied cognition, as a way of explaining the special kind of "internal-external coupling", where brain is considered a control mechanism that coordinates action and movements in the world. Common coding hypothesizes

[13] On the puzzling problem of the "modal" and "amodal" character of the human brain processing of perceptual information, and the asseveration of the importance of grounded cognition, cf. [Barsalou, 2008a; Barsalou, 2008b].

[14] "The basic argument for common coding is an adaptive one, where organisms are considered to be fundamentally action systems. In this view, sensory and cognitive systems evolved to support action, and they are therefore dynamically coupled to action systems in ways that help organisms act quickly and appropriately. Common coding, and the resultant replication of external movements in body coordinates, provides one form of highly efficient coupling. Since both biological and nonbiological movements are equally important to the organism, and the two movements interact in unpredictable ways, it is beneficial to replicate both types of movements in body coordinates, so that efficient responses can be generated" [Chandrasekharan, 2009, p. 1069]: in this quoted paper the reader can find a rich reference to the recent literature on embodied cognition and common coding.

> [...] that the execution, perception, and imagination of movements share a common representation (coding) in the brain. This coding leads to any one of these three (say perception of an external movement), automatically triggering the other two (imagination and execution of movement). One effect of this mechanism is that it allows any perceived external movement to be instantaneously replicated in body coordinates, generating a dynamic movement trace that can be used to generate an action response. The trace can also be used later for cognitive operations involving movement (action simulations). In this view, movement crosses the internal/external boundary *as movement*, and thus movement could be seen as a "lingua franca" that is shared across internal and external models, if both have movement components, as they tend to do in science and engineering [Chandrasekharan, 2009, p. 1061].

Common coding refers to a representationalist account, but representation supports a motor simulation mechanism "which can be activated across different timescales – instantaneous simulation of external movement, and also extended simulations of movement. The latter could be online, that is, linked to an external movement (as in mental rotations while playing Tetris, see [Kirsh and Maglio1994]), or can be offline (as in purely imagined mental rotation)" [Chandrasekharan, 2009, p. 1072]. Furthermore

1. given the fact models in science and engineering often characterize phenomena in terms of bodies and particles, motor simulations are important to understand them, and the lingua franca guarantees integration between internal and external models;
2. the manipulation of the external models creates new patterns that are offered through perception to the researchers (and across the whole team, to possibly reach that shared "manifest model" I have illustrated above), and "perturbs" (through experimentation on the model that can be either intended or random) their movement-based internal models possibly leading "[...] to the generation of nonstandard, but plausible, movement patterns in internal models, which, in combination with mathematical and logical reasoning, leads to novel concepts" (cit., p. 1062);
3. this hybrid combination with mathematical and logical reasoning, and possible other available representational resources stored in the brain, offers an example of the so-called multimodality of abduction.[15] Not only both data and theoretical adopted hypotheses, but also the intermediate steps between them – i.e. for example, models – can have a full range of verbal and sensory representations, involving words, sights, images, smells, etc. and also kinesthetic and motor experiences and feelings such as satisfaction, and thus all sensory modalities. Furthermore, each of these cognitive levels – for example the mathematical ones, often thought as presumptively *abstract* [does this authorize us to say they are fictional?] – actually consist in intertwined and flexible models (*external* and *internal*) that can be analogically referred to the Peircean concept of the "compound conventional sign", where for example sentential and logical aspects coexist with model-based features. For Peirce, *iconicity hybridates log-*

[15] On the concept of multimodal abduction see chapter four of [Magnani, 2009].

icality: the sentential aspects of symbolic disciplines like logic or algebra coexist with model-based features – iconic. Indeed, sentential features like symbols and conventional rules[16] are intertwined with the spatial configuration, like in the case of "compound conventional signs". Model-based iconicity is always present in human reasoning, even if often hidden and implicit;[17]

4. it is the perturbation I have described above that furnishes a chance for change, often innovative, in the internal model (new brain areas can be activated creating new connections, which in turn can motivate further manipulations and revisions of the external model): it is at this level that we found the scientific cognitive counterpart of what has been always called in the tradition of philosophy and history of science, scientific imagination.[18]

It is worth to note that, among the advantages offered by the external models in their role of perturbing the internal ones, there are not only the unexpected features that can be offered thanks to their intrinsic materiality, but also more neutral but fruitful devices, which can be for example exemplified thanks to the case of externalized mathematical symbols: "Apparently the brain immediately translates a positive integer into a mental representation of its quantity. By contrast, symbols that represent non-intuitive concepts remain partially semantically inaccessible to us, we do not reconstruct them, but use them as they stand" [De Cruz and De Smedt, 2011]. For example, it is well-known that Leibniz adopted the notation *dx* for the infinitesimals he genially introduced, and called them *fictions bien fondées*, given their semantic paradoxical character: they lacked a referent in Leibnizian infinitesimal calculus, but

[16] Written natural languages are intertwined with iconic aspects too. Stjernfelt [Stjernfelt, 2007] provides a full analysis of the role of icons and diagrams in Peircean philosophical and semiotic approach, also taking into account the Husserlian tradition of phenomenology.

[17] It is from this perspective that [sentential] syllogism and [model-based] perception are seen as rigorously intertwined. Consequently, there is no sharp contrast between the idea of cognition as perception and the idea of cognition as something that pertains to logic. Both aspects are inferential in themselves and fruit of sign activity. Taking the Peircean philosophical path we return to observations I always made when speaking of the case of abduction: cognition is basically *multimodal*.

[18] In a perspective that does not take into account the results of cognitive science but instead adopts the narrative/literary framework about models as make-believe, Toon [Toon, 2010] too recognizes the role of models in perturbing mental models to favor imagination: "Without taking a stance in the debate over proper names in fiction, I think we may use Walton's analysis to provide an account of our prepared description and equation of motion. We saw [. . .] that these are not straightforward descriptions of the bouncing spring. Nevertheless, I believe, they do represent the spring, in Walton's sense: they represent the spring by prescribing imaginings about it. When we put forward our prepared description and equation of motion, I think, those who are familiar with the process of theoretical modelling understand that they are to imagine certain things about the bouncing spring. Specifically, they are required to imagine that the bob is a point mass, that the spring exerts a linear restoring force, and so on" (p. 306).

were at the basis of plenty of new astonishing mathematical results.[19] De Cruz and De Smedt call this property of symbols "semantic opacity", which renders them underdetermined, allowing further creative processes as symbols that can be relatively freely exploited in novel contexts for multiple cognitive aims. Semantic opacity favors a kind of reasoning that is unbiased by the intuitive aspects possibly involving stereotypes or intended uncontrolled interpretations, typical of other external models/representations.

Peirce too was clearly aware, speaking of the model-based aspects of deductive reasoning, that there is an "experimenting upon this image [the external model/diagram] in the imagination", where the idea that human imagination is always favored by a kind of prosthesis, the external model as an "external imagination", is pretty clear, even in case of classical geometrical deduction: "[. . .] namely, deduction consists in constructing an icon or diagram the relations of whose parts shall present a complete analogy with those of the parts of the object of reasoning, of experimenting upon this image in the imagination and of observing the result so as to discover unnoticed and hidden relations among the parts" [Peirce, 1931-1958, 3.363].

Analogously, in the case at stake, the computational model of neuronal behavior, by providing new chances in terms of control, visualizations, and costs, is exactly the peculiar tool able to favor manipulations which trigger the new idea of the "spatial activity pattern of the spikes" [Chandrasekharan, 2009, p. 1067].

3.2 Fictions or Epistemic Weapons?

Thanks to the cognitive research I have illustrated in the previous subsection, we are faced with the cognitive modern awareness of what also implicitly underlies Peircean speculations: nature fecundates the mind because it is through a disembodiment and extension of the mind in nature (that is, so to say, "artificialized") that in turn nature affects the mind. Models are built by the mind of the scientist(s), who first delegate "meanings" to external artifacts: mind's "internal" representations are "extended" in the environment, and later on shaped by processes that are occurring through the constraints found in "nature" itself; that is that external nature that consists of the "concrete" model represented by the artifact, in which the resulting aspects and modifications/movements are "picked up" and in turn re-represented in the human brain. It is in this perspective that we can savor, now in a naturalistic framework, the speculative Aristotelian anticipation that "nihil est in intellectu quod prius non fuerit in sensu". In such a way – that is thanks to the information that flows from the model – the scientists' internal models are rebuilt and further

[19] To confront critiques and suspects about the legitimacy of the new number dx, Leibniz prudently conceded that dx can be considered a fiction, but a "well founded" one. The birth of non-standard analysis, an "alternative calculus" invented by Abraham Robinson [Robinson, 1966], based on infinitesimal numbers in the spirit of Leibniz's method, revealed that infinitesimals are not at all fictions, through an extension of the real numbers system $\mathbb{R}$ to the system $\mathbb{R}^*$ containing infinitesimals smaller in the absolute value than any positive real number.

refined and the resulting modifications can easily be seen as guesses – both instinctual and reasoned, depending of the brain areas involved, that is as plausible abductive hypotheses about the external extra-somatic world (the target systems). I repeat, the process can be seen in the perspective of the theory of cognitive niches: the mind grows up together with its representational delegations to the external world that has made itself throughout the history of culture by constructing the so-called cognitive niches. In this case the complex cognitive niche of the scientific lab is an *epistemological* niche, expressly built to increase knowledge following rational methods, where "*people, systems, and environmental affordances*" [Chandrasekharan, 2009, p. 1076] work together in an integrated fashion.

Even if Chandrasekharan and Nersessian's research deals with models which incorporate movement, and so does not consider models that are not based on it, it provides an useful example able to stress the distributed character of scientific models, and the true type of abstractness/ideality they possess, so refreshing these notions that come from the tradition of philosophy of science. The analysis of models as material, mathematical, and fictional – and as "abstract objects" – provided by Contessa [Contessa, 2010], where "a model is an actual abstract object that stands for one of the many possible concrete objects that fit the generative description of the model" (p. 228) would take advantage of being reframed in the present naturalistic perspective. The same in the case of Frigg [Frigg, 2010c], who contends a fictionalist view and says "Yet, it is important to notice that the model-system is not the same as its [verbal] description; in fact, we can re-describe the same system in many different ways, possibly using different languages. I refer to descriptions of this kind as model-descriptions and the relation they bear to the model-system as *p*-representation" (pp. 257–258). Indeed, Contessa's reference to models as "actual abstract objects" and Frigg's reference to models as abstract "model-systems" would take advantage of the cognitive perspective I am presenting here: where are they located, from a naturalistic point of view? Are they mental models? If they are mental models, like I contend, this should be more clearly acknowledged.

Hence, in my perspective models cannot be considered neither abstract (in the traditional ambiguous sense) nor fictional: scientist do not have any intention to propose fictions, instead they provide models as tools that reshape a generic cognitive niche as an epistemological niche to the aim of performing a genuine struggle for representing the external world. Models, the war machines used in this struggle, which I call *epistemic warfare*, to stress the determined – strictly epistemic – dynamism of the adopted tools that are at stake, are not illusional fictions or stratagems used for example to cheat nature or swindle human beings, but just concrete, unambiguous, and well disposed tactical intermediate weapons able to strategically "attack" nature (the target systems) to further unveil its structure. Contrarily, fictions in works of fictions are for example meant to unveil human life and characters in new esthetic perspectives and/or to criticize them through a moral teaching, while fictions and stratagems in wars are meant to trick the enemy and possibly destroy the eco-human targets.

I contend that epistemologists do not have to forget that various cognitive processes present a "military" nature, even if it is not evident in various aspects and uses of syntactilized human natural language and in abstract knowledge.[20] It is hard to directly see this "military intelligence"[21] in the many *epistemic* functions of natural language, for example when it is simply employed to transmit scientific results in an academic laboratory situation, or when we gather information from the Internet – expressed in linguistic terms and numbers – about the weather. However, we cannot forget that even the more abstract character of knowledge packages embedded in certain uses of language (and in hybrid languages, like in the case of mathematics, which involves considerable symbolic parts) still plays a significant role in changing the moral behavior of human collectives. For example, the production and the transmission of new scientific knowledge in human social groups not only operates on information but also implements and distributes roles, capacities, constraints and possibilities of actions. This process is intrinsically moral because in turn it generates precise distinctions, powers, duties, and chances which can create new between-groups and in-group violent (often) conflicts, or reshape older preexistent ones.

New theoretical biomedical knowledge about pregnancy and fetuses usually has two contrasting moral/social effects, 1) a better social and medical management of childbirth and related diseases; 2) the potential extension or modification of conflicts surrounding the legitimacy of abortion. In sum, even very abstract bodies of knowledge and more innocent pieces of information enter the semio/social process which governs the identity of groups and their aggressive potential as coalitions: deductive reasoning and declarative knowledge are far from being exempt from being accompanied by argumentative, deontological, rhetorical, and dialectic aspects. For example, it is hard to distinguish, in an eco-cognitive setting, between a kind of "pure" (for example deductive) inferential function of language and an argumentative or deontological one. For example, the first one can obviously play an associated argumentative role. However, it is in the arguments traditionally recognized as fallacious, that we can more clearly grasp the military nature of human language and especially of some hypotheses reached through fallacies.

Hence, we have to be aware that science imposes itself as a paradigm of producing knowledge in a certain "decent" way, but at the same time it de facto belongs to the cross-disciplinary warfare that characterizes modernity: science more or less conflicts with other non scientific disciplines, religions, literature, magic, etc., and also implicitly orders and norms societies through technological products which impose behaviors and moral conducts. Of course scientific cognitive processes – *sensu strictu*, inside scientific groups as coalitions – also involve propaganda, like Feyerabend says, for instance to convince colleagues about a hypothesis or a method, but propaganda is also externally addressed to other private and public coalitions and common people, for example to get funds (a fundamental issue often disregarded

[20] I extendedly treated the relationship between cognition and violence in my [Magnani, 2011].

[21] I am deriving this expression from René Thom [Thom, 1988], who relates "military intelligence" to the role played by language and cognition in the so-called *coalition enforcement*, that is at the level of their complementary effects in the affirmation of moralities and related conducts, and the consequent perpetration of possible violent punishments.

in the contemporary science is the cost of producing new models) or to persuade about the value of scientific knowledge. Nevertheless the core cognitive process of science is based on avoiding fictional and rhetorical devices when the production of its own regimen of truth is at stake. Finally, science is exactly that enterprise which produces truths that establish themselves as the paradigms for demarcating fictions and so "irrational" or "arational" ways of knowing.

I am aware of the fact that epistemological fictionalism does not consider fictions forgery or fake, that is something "far from being execrable", instead, something "we cherish" [Frigg, 2010c, p. 249], but to say that scientific and literary fictions are both "good" fictions is a bit of a theoretical oversemplification, because it is science that created, beyond literature and poetry, *new* kinds of models committed to a specific production of truth, constitutively aiming at not being fictional.[22] I confess I cannot see how we can speak of the ideal pendulum in the same way we speak of Anna Karenina: it seems to me that we are running the risk of inadvertently opening the gates of epistemology to a kind of relativistic post-modernism *à la mode*, even if fictionalists seem to avoid this possible confusion by producing – often useful – taxonomies about the slight differences between fictions in science and in other cognitive practices.

In overall, I am convinced that introducing the word fiction in epistemology adds a modest improvement to the analysis of topics like inference, explanation, creativity, etc., but just an attractive new lexicon, which takes advantage of some seductive ideas coming for example from the theory of literary fictions. Anna Karenina and the in-vitro model[23] are very different. In actual scientific practice, a model becomes fictional only *after* the community of researchers has recognized it as such, *because* it has *failed* in fruitfully representing the target systems. In these cases a model is simply discarded. Tolstoy might have discarded the character of Anna Karenina as an inappropriate fiction for some contemporary esthetic purpose (for instance, had she failed, in her author's opinion, to veraciously represent a female member of Russia's high society at the end of XIX century), but he would have substituted her with yet another – just as fictional – character, doomed to *remain* fictional for ever.[24]

As I already said, conversely a scientific model is recognized as fictional in a cognitive (often creative) process when it is assessed to be unfruitful, by applying a kind of *negation as failure* [Clark, 1978; Magnani, 2001a]: it becomes fictional in the mere sense that it is falsified (even if "weakly" falsified, by failure).[25] Methodologically, negation as failure is a process of elimination that parallels what

[22] Cf. below, subsection 3.6.

[23] Indeed, in the recent epistemological debate about fictions, even the whole "experimental systems" are reframed as "materialized fictional 'worlds'" [Rouse, 2009, p. 51].

[24] Giere usefully notes that "Tolstoy did not intend to represent actual people except in general terms" and that, on the contrary, a "primary function [of models in science], of course, is to represent physical processes in the real world" [Giere, 2007, p. 279].

[25] On the powerful and unifying analysis of inter-theory relationships, which involves the problem of misrepresenting models – and their substitution/adjustement – and of incompleteness of scientific representation, in terms of partial structural similarity, cf. [Bueno and French, 2011] and the classic [da Costa and French, 2003].

Freud describes in the case of constructions (the narratives the analyst builds about patient's past psychic life) abandoned because they do not help to proceed in the therapeutic psychoanalytic process: if the patient does not provide new "material" which extends the proposed construction, "if", as Freud declares, "[...] nothing further develops we may conclude that we have made a mistake and we shall admit as much to the patient at some suitable opportunity without sacrificing any of our authority". The "opportunity" of rejecting the proposed construction "will arise" just "[...] when some new material has come to light which allows us to make a better construction and so to correct our error. In this way the false construction drops out, as if it has never been made; and indeed, we often get an impression as though, to borrow the words of Polonius, our bait of falsehood had taken a carp of truth" [Freud, 1953-1974, vol, 23, 1937, p. 262].

Similarly, for example in a scientific discovery process, the scientific model is simply eliminated and labeled as "false" , because "new material has come to light" to provide a better model which in turn will lead to a new knowledge that supersedes or refines the previous one, and so the old model is buried in the necropolis of the unfruitful/dead models. Still, similarly, in the whole scientific enterprise, also a successful scientific model is sometimes simply eliminated (for example the ether model) together with the theory to which that model belonged, and so the old model is buried in yet another necropolis, that of the abandoned "historical" models, and yes, in this case, it can be plausibly relabeled as a fiction.

A conclusion in tune with my contention against the fictional character of scientific models is reached by Woods and Rosales [Woods and Rosales, 2010a], who offer a deep and compelling logico-philosophical analysis of the problem at stake. They contend that it is extremely puzzling to extend the theory of literary and artistic fictions to science and other areas of cognition. Whatever we say of the fictions of mathematics and science, there is "nothing true of them in virtue of which they are *literary fictions*" (p. 375). They correctly note that "Saying that scientific stipulation is subject to normative constraints is already saying something quite different from what should be said about literary stipulation":

> We also see that scientific stipulation is subject to a *sufferance* constraint, and with it to factors of timely goodness. A scientist is free to insert on his own sayso a sentence ϕ in T's model of M on the expectation that T with it in will do better than T with it not in, and subject in turn to its removal in the face of a subsequently disappointing performance by T. This is a point to make something of. Here is what we make of it:
>
> - The extent to which a stipulation is held to the sufferance condition, the more it resembles a *working hypothesis*.
> - The more a sentence operates as a working hypothesis, the more its introduction into a scientific theory is conditioned by *abductive considerations*.
>
> Accordingly, despite its free standing in M, a stipulationist's ϕ in T is bound by, as we may now say, *book-end* conditions, that is to say, conditions on *admittance* into T in the first place, and conditions on its *staying* in T thereafter. The conditions on going in are broadly abductive in character. The conditions on *staying in* are broadly – sometimes very broadly – confirmational in character. Since there is nothing remotely

> abductive or confirmational in virtue of which a sentence is an $\mathscr{F}$-truth [fictive truth] on its author's sayso, radical pluralism must be our verdict here [Woods and Rosales, 2010a, pp. 375-376].

In conclusion, after having proposed a distinction between predicates that are load-bearing in a theory and those that are not, Woods and Rosales maintain that a predicate that is not load-bearing in a theory is a *façon de parler*: "For example, everyone will agree that the predicate 'is a set' is load-bearing in the mathematical theory of sets and that 'is an abstract object', if it occurs there at all, is a *façon de parler*. 'Is an abstract object' may well be load-bearing in the philosophy of mathematics, but no work-a-day mathematician need trouble with it. It generates no new theorems for him. Similarly, 'reduces to logic' is not load-bearing in number theory, notwithstanding the conviction among logicists that it is load-bearing in mathematical epistemology" [Woods and Rosales, 2010a, pp. 377–378]. Unfortunately the predicate "is a fiction" is non-load-bearing, or at best a *façon de parler*, in any scientific theory. At this point the conclusion is obvious, and I agree with it, since there is no concept of scientific fiction, the question of whether it is assimilable to or in some other way unifiable with the concept of literary fiction does not arise.

Elsewhere [Magnani, 2009, chapter three] I called the external scientific models "mimetic",[26] not in a military sense, as camouflaged tools to trick the hostile eco-human systems, but just as structures that mimic the target systems for epistemic aims. In this perspective I described the centrality of the so called "disembodiment of the mind" in the case of semiotic cognitive processes occurring in science. Disembodiment of the mind refers to the cognitive interplay between internal and external representations, *mimetic* and, possibly, *creative*, where the problem of the continuous interaction between on-line and off-line (for example in inner rehearsal) intelligence can properly be addressed. In the subsection 3.4 below, we will see that this distinction parallels the one illustrated by Morrison between models which idealize (mirroring the target systems) and abstract models (more creative and finalized to establish new scientific intelligibility).

As I am trying to demonstrate in this whole section with the description of the above models based on common coding, I consider this interplay critical in analyzing the relation between meaningful semiotic internal resources and devices and their dynamical interactions with the externalized semiotic materiality already stored in the environment (scientific artifactual models, in this case). This external materiality plays a specific role in the interplay due to the fact that it exhibits (and operates through) its own cognitive constraints. Hence, minds are "extended" and artificial in themselves. It is at the level of that continuous interaction between on-line and off-line intelligence that I underlined the importance of what I called *manipulative abduction*.

Manipulative abduction, which is widespread in scientific reasoning [Magnani, 2009, chapter one] is a process in which a hypothesis is formed and evaluated resorting to a basically extra-theoretical and extra-sentential behavior that aims at cre-

[26] On the related problem of resemblance (similarity, isomorphism, homomorphism, etc.) in scientific modeling see below subsection 3.5.

ating communicable accounts of new experiences to integrate them into previously existing systems of experimental and linguistic (theoretical) practices. Manipulative abduction represents a kind of redistribution of the epistemic and cognitive effort to manage objects and information that cannot be immediately represented or found internally. An example of manipulative abduction is exactly the case of the human use of the construction of external models in the neural engineering laboratory I have outlined in the previous subsections, useful to make observations and "experiments" to transform one cognitive state into another to discover new properties of the target systems. Manipulative abduction also refers to those more unplanned and unconscious action-based cognitive processes I have characterized as forms of "thinking through doing" (cf. footnote 10 above).

3.3 Are the In-Vitro Model or a Geometrical Diagram Fictions? Dynamic vs. Static View of Scientific Models

In subsection 3.1 I have contended that 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 [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, 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. In the example I am illustrating in this section scientists in the 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. The in-vitro networks of cultured neurons of our case 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) 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 its 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". We have seen that manipulative abduction 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]).[27]

Iconicity 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.[28] 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 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 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 the catastrophe theory. As a human being who is not able to produce anything new relating to

[27] 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.

[28] 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. On models as epistemic mediators in mathematics cf. [Boumans, 2012].

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 [...] [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 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].[29]

3.4 *Confounding Static and 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; Sugden, 2009]). It is instead a *regulatory* tool *stabilized* in "some exterior form", a kind of a reliable anchorage, not intentionally established as fiction, like a romance writer could intentionally do, assessing the character of Harry Potter. 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).

[29] 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].

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 [Thomson-Jones, 2010] emphasizes that science is full of "descriptions of missing systems", that at the end are thought as *abstract models*.[30] Further, Mäki [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 argumentations are advanced by Godfrey-Smith [Godfrey-Smith, 2009, pp. 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 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.[31] The same can be said of models as configurations (Godfrey-Smith), which certainly are conditional, but at the same time not "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 a strong metaphysical realism we can state that, once a final scientific result has been achieved, together with the description of the related 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 [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

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

[31] Cf. the previous subsection, on the problem of scientific model stipulation as subject to a *sufferance* constraint.

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 [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.[32] 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].[33]

[32] The notion of co-exact proprieties, introduced by Manders [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 [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".

[33] Further information about the problem of the mapping between models and target systems through *interpretation* are provided by Contessa [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]).

In sum, I think it is misleading to analyze models in science by adopting a confounding mixture of static and 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 [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 [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 system" (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 (cf. below, subsection 3.6). 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 intelligibilty 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 [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*.

3.5 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.[34]

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 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].

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

In this perspective we paradoxically face the opposite of the received view, it is the newly known target system that resembles the model, which itself originated that resemblance.[35] 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".[36]

In *Against Method* [Feyerabend, 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, but that do not play any epistemic role in the restricted cognitive process of scientific discovery, I have called "epistemic" warfare.[37]

Coming back to the problem of models as surrogates, Mäki [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.

[35] I endorse many of the considerations by Chakravartty [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.

[36] 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 [Sugden, 2000; Sugden, 2009].

[37] On Galileo's mental imagery, cf. below, subsection 3.6.

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 I already quoted above (section 2) who suspects fictionalists are paradoxically obsessed by "the truth, the whole truth, and nothing but the truth": scientific theories would reflect this hyper-truth that in turn would reflect true reality (curious! Is not science the realm or self-correcting truths?)[38]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 scientific theories. In this spirit Sugden [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 model [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

[38] 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].

> 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 perspective 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 "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 exploration 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*.[39]

[39] An example is furnished by the precarious condition of various parts of psychological research. Miller [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.

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3.6 Galileo's Modeling Vindicated

Weisberg [Weisberg, 2007, p. 642][40] maintains that "Galilean idealization is the practice of introducing distortions into theories with the goal of simplifying theories in order to make them computationally tractable. One starts with some idea of what a non-idealized theory would look like. Then one mentally and mathematically creates a simplified model of the target". I would like to advance a suspect about this canonical treatment of Galileo, and provide some reasons that explain my perplexity.

When Galileo illustrates an imaginary model concerning the problem of falling bodies, he provides a kind of smart mental modeling. Let us religiously follow the text of the creator of modern science on this subject:

> SALV. But, even without further experiment, it is possible to prove clearly, by means of a short and conclusive argument, that a heavier body does not move more rapidly than a lighter one provided both bodies are of the same material and in short such as those mentioned by Aristotle. But tell me, Simplicio, whether you admit that each falling body acquires a definite speed fixed by nature, a velocity which cannot be increased or diminished except by the use of force [violenza] or resistance.
>
> SIMP. There can be no doubt but that one and the same body moving in a single medium has a fixed velocity which is determined by nature and which cannot be increased except by the addition of momentum [impeto] or diminished except by some resistance which retards it.
>
> SALV. If then we take two bodies whose natural speeds are different, it is clear that on uniting the two, the more rapid one will be partly retarded by the slower, and the slower will be somewhat hastened by the swifter. Do you not agree with me in this opinion?
>
> SIMP. You are unquestionably right.
>
> SALV. But if this is true, and if a large stone moves with a speed of, say, eight while a smaller moves with a speed of four, then when they are united, the system will move with a speed less than eight; but the two stones when tied together make a stone larger than that which before moved with a speed of eight. Hence the heavier body moves with less speed than the lighter; an effect which is contrary to your supposition. Thus you see how, from your assumption that the heavier body moves more rapidly than the lighter one, I infer that the heavier body moves more slowly.
>
> SIMP. I am all at sea because it appears to me that the smaller stone when added to the larger increases its weight and by adding weight I do not see how it can fail to increase its speed or, at least, not to diminish it.
>
> SALV. Here again you are in error, Simplicio, because it is not true that the smaller stone adds weight to the larger.
>
> SIMP. This is, indeed, quite beyond my comprehension. [Galilei, 1914, pp. 62–63].

Gendler nicely summarizes this kind of Galilean mental modeling stressing that we are dealing with an admirable example of *Gedankenexperiment*

[40] Weisberg distinguished between various kinds of idealization: Galilean, minimalist (still devoted to reveal the most important causal powers at stake), and multiple-models (devoid of a single representation ideal, widespread for example in biology and social science).

(thought experiment) in which we imagine that a heavy and a light body are strapped together and dropped from a significant height:

> What would the Aristotelian expect to be the natural speed of their combination? On the one hand, the lighter body should slow down the heavier one while the heavier body speeds up the lighter one, so their combination should fall with a speed that lies between the natural speeds of its components. (That is, if the heavy body falls at a rate of 8, and the light body at a rate of 4, then their combination should fall at a rate between the two [. . .]. On the other hand, since the weight of the two bodies combined is greater than the weight of the heavy body alone, their combination should fall with a natural speed greater than that of the heavy body. (That is, if the heavy body falls at a rate of 8 and the light body with a rate of 4, their combination should fall at a rate greater than 8.) But then the combined body is predicted to fall both more quickly, and more slowly, than the heavy body alone. The way out of this paradox is to assume that the natural speed with which a body falls is independent of its weight: "both great and small bodies [. . .] are moved with like speeds" [Gendler, 1998, p. 403].

Is this modeling a fiction, a surrogate, an idealization, an abstraction, a credible world of the target system? Surely these attributes do not appropriately characterize this Galileo's epistemic act, which cognitively attacks the Aristotelian views on motion. Let us explain why. For the Aristotelian, the daily experience seems to confirm that heavier bodies fall faster than the lighter ones. Nevertheless, when the Aristotelian sees two stones of different weights fall the ground with similar speeds, this requires an explanation.[41] Two auxiliary assumptions can be provided, the Galilean one in terms of air resistance, the Aristotelian one which complains that the bodies have not been dropped from a height sufficiently great. What the Galilean thought experiment provides to the Aristotelian is not a new empirical knowledge of the external world but a sudden new belief, or a "conceptual reconfiguration", concerning the independency between speed and weight of falling bodies, and the *kind of thing* natural speed might be as a new *physical property*, like Gendler says [Gendler, 1998, pp. 408–409].

Given the fact the modeling activity provided by this thought experiment is not posterior to the conceptual "reconfiguration" of the empirical data, it could hardly be classified as fictional or as a surrogate of them, it is instead *constitutive* of the possible reconfiguration itself: "Prior to contemplation of the case, there was no room on the Aristotelian picture for the thought that natural speed might be constant, not varying – that it might be dependent not on some specific features of the body in question, but only on the fact that it is a body at all" [Gendler, 1998, p. 412]. The old Aristotelian idea of natural speed does not make sense anymore "like phlogiston, it disappears into the ether of abandoned concepts" (cit.) The model provided by the thought experiment is not a simple way of modifying the Aristotelian perception of falling bodies, but a transformation of the "schematization" of the percepts themselves, to use the Kantian efficacious word, which makes them intelligible in a novel way. And, like experiments in science, this good thought experiment is not

[41] Philosophy of science has often stressed that theories are undetermined by evidence, like for example the conventionalist tradition teaches us [Magnani, 2001b, chapter five].

evanescent and fuzzy, but clear, *repeatable*, and *sharable*, in so far as it can involve unambiguous constructive representations in various human agents.

In this case the model is "crucial": "There will, no doubt, be many cases where the role of the imagery is simply heuristic. But there will also be cases where the role of the imagery is [...] epistemically crucial" [Gendler, 2004, p. 1161].[42] This "crucial" creative role is also stressed by Nersessian [Nersessian, 1993, p. 292] who, describing Mach's seminal ideas on the *Gedankenexperiment*, reminds us that "[...] while thought experimenting is a truly creative part of scientific practice, the basic ability to construct and execute a thought experiment is not exceptional. The practice is highly refined extension of a common form of reasoning [...] by which we grasp alternatives, make predictions, and draw conclusions about potential real-world situations" [Nersessian, 1993, p. 292].

Instead of seeing Galilean model as a fiction, it has to be considered an *actual* representation,[43]which helps discover – and justify – in this case in a precise *model-based* non-propositional way, what sorts of motions (and objects) we think plausible in the world. The door that provides access to further mathematical refinement and experimental research concerning the target system is finally opened. It will be only after having fruitfully built the complete Galilean mathematized theory of motion that the mental model provided by the thought experiment in question can appear fictional, a surrogate, and so on. Moreover, it is only at this later stage that also a clear concept of approximation (and, in turn, of de-idealization) of related models will acquire a rigorous and complete sense.[44] No distortions are present in the presupposed "idealization" of this Galilean thought experiment, simply because, the new schematization of the target is the fruit itself of the modeling activity, and we cannot provide a distortion of objects/targets that are not yet available. If still we want to say that the model shows itself as an idealization, this is simply because it belongs to modern physics, which on the whole, Galileo teaches us, idealizes.

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[42] The basic epistemological and cognitive aspects of thought experiments are nicely illustrated by Arcangeli [Arcangeli, 2010], who stresses their role in producing new knowledge and the useful distinction between their icastic or recreative character.

[43] "[...] the person conducting the experiment asks herself: 'What would I say/judge/expect were I to encounter circumstances XYZ?' and then finds out the (apparent) answer. This technique is common in linguistics, where the methodology is used to ascertain the grammaticality of sentences, the meanings of phrases, the taxonomic categories of words, and so on. And it is, on one view at least, a central element of moral reasoning: we think about particular imaginary cases, observe the judgements that they evoke in us, and use these judgements as fixed points in developing our moral theories" [Gendler, 1998, p. 414].

[44] A deep analysis of the relationships between idealization, approximation (and de-idealization), which is also in part in tune with my observations above, is provided by Portides [Portides, 2007, p. 708]: "I employ this analysis of the process of construction of representational models to demonstrate that idealisation, and its converse process of de-idealisation, is present at every level of scientific theorising whereas the concept of approximation becomes methodologically valuable, and epistemically significant, either when a tractable mathematical description of a de-idealising factor is needed or after a certain point in the process is reached when a given theoretical construct (i.e. a scientific model) may be proposed for the representation of a physical system".

A further remark which takes advantage of Cartwright's epistemology of models and capacities can be useful to grasp the point about Galilean mental modeling. Treating Sugden's problem of models as credible worlds (that I have quoted above in subsection 3.4), Cartwright contends that "[...] the license to move from the results in the model about what happens when a cause is exercised without impediment to a contribution that the cause will make in all situations of some designated category depends on the assumption that the cause has a stable contribution to make, and that assumption must be supported by evidence from elsewhere. This is part of the way in which Sugden's own view relies on the logic of capacities" [Cartwright, 2009a, pp. 53–54]. It is very easy for Cartwright to add that capacities in science are characterized by some additional "premises": 1) "stable contribution" of the envisaged cause (eventually to be measured) in the real-world situation is not necessarily the same it does in the model, when we know that some other cause can have generated the effect in question; 2) the contribution the capacity makes in the model, the result "is exported to understand or predict in real-world situations where the cause that carries that capacity operates even when we do not expect the overall results to be the same in those situations that have results similar to those situations as they are in the models" (p. 54).

This is an important point, Cartwright says, because Sugden's account based on credible worlds simply looks at the real-world situation that presents results similar to those in the model and then infers by *abduction* that the causes are the same. Here a bad example of the fallacy of affirming the consequent is committed: we face the cognitive activity of inferring from the same effect to the same cause (pp. 54–55) and not, on the contrary, the fact that "whenever the same cause appears as in the model, the same effect will appear", because we can do this given the fact the model is based on a robust hypothesis about the complex relationships between cause and effect. Indeed, in this case, the abduction as "inferring from same effect to same cause" is highly uncertain, and it does not tell us that the model furnishes a stable contribution, which instead should only be related to the level of abstraction at which to describe the cause and the effect. It is the presence of more abstract concepts which describe the causes and effects that qualifies the epistemological quality of the model.

In the Galilean thought experiment I have illustrated above the bodies are envisaged as masses, and gravity is implied: this is exactly what is at the basis of the fertile exportation of conclusions from the model to the world, and of the possibility of finding a suitable schematization through mathematization. In the Galilean case, and by adopting a dynamic perspective on science, abduction is good and creative *because* we deal with the abductive process that concerns the *first* construction of modern physics. Otherwise, if we already possess the complete laws of Galilean physics – by adopting in this case a static perspective – a related model exports to the real situation thanks to a causal explanation through de-idealization. Indeed, Cartwright observes "Say we have a model about the planetary system. In the model we deduce that planets are caused by gravitational attraction to accelerate towards the sun. Is the motion of cannonballs towards the earth a similar effect so that we might do an abduction to similar causes? It is if we describe both the cannonballs and the planets as compact masses. Otherwise the abduction is farfetched" (p. 57). In the perspective of this important distinction the epistemological divergence between static and dynamic aspects of science is still at stake.

In the case discussed above, of the model as a generic credible world, the model is instead "shallow" – as it happens in the case of simple analogue economic models – because it does not lead to discover proper capacities – in Cartwright's sense – and unfortunately basic principles are neither available nor "foreseeable" through a working discovering modeling process, to which the model itself eventually strategically belongs. In the case of these shallow models Cartwright nicely concludes "the worry is not just that the assumptions are unrealistic; rather, they are unrealistic in just the wrong way" (p. 57). In this case models certainly are isolating devices, but they isolate in the wrong way, and induction – in Sugden'sense, even if cautious – from the model to a real situation results to be a clear hasty generalization. This does not mean that these shallow models do not provide knowledge about target systems, but this knowledge is very limited and unsatisfactory in the light of the decent epistemological standards in terms of Cartwright's capacities.

To conclude, coming back to the problem of fictionalism and its discontents, Galileo is explicitly clear about the distinction between science (he calls "philosophy" in the following celebrated passage) and literary fiction:

> In Sarsi[45] I seem to discern the firm belief that in philosophizing one must support oneself upon the opinion of some celebrated author, as if our minds ought to remain completely sterile and barren unless wedded to the reasoning of some other person. Possibly he thinks that philosophy is a book of fiction by some writer, like the Iliad or Orlando Furioso, productions in which the least important thing is whether what is written there is true. Well, Sarsi, that is not how matters stand. Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth [Galilei, 1957, pp. 237–238].[46]

[45] Lothario Sarsi of Siguenza is the pseudonym of the Jesuit Orazio Grassi, author of *The Astronomical and Philosophical Balance*. In *The Assayer*, Galileo weighs the astronomical views of Orazio Grassi about the nature of the comets, and finds them wanting [Galilei, 1957, p. 231].

[46] As Bertolotti [Bertolotti, 2012] in this volume observes, the quotation obviously should not be used as an authority weapon against those who advocate the fictional nature of scientific models, because we would commit a fallacy, given the fact that to affirm that scientific models are fictions does not coincide with saying that the whole scientific endeavor has a fictional nature. Thus, the use of this quotation does not aim at getting definitively rid of fictionalism through the authority of one of the founding fathers of modern science.

4 Conclusion

In this paper I have contended that scientific models are not fictions. I have argued that also 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 recent cognitive research in scientific labs and of the concept of manipulative abduction. In the meantime the illustrated critique, also performed in the light of distributed cognition, offered new insight on the analysis of the two main classical attributes given to scientific models: abstractness and ideality. A further way of delineating a more satisfactory analysis of fictionalism and its discontents has been constructed 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. I conclude that, in scientific settings, when models are fictions, it is because they were simply discarded, as heuristic failed steps, abandoned by applying a kind of negation as failure. I have also illustrated that it is misleading to analyze models in science by confounding static and 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 adopted some thoughts of two classical authors, 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 has been considered. In this perspective I have paradoxically reached the opposite of the received view: it is the newly known target system that resembles to the model, which itself originated that resemblance. Finally, to pleasantly try to give rid of fictionalism, the authoritative "voice" of Galileo is exploited: 1) the Galileo's thought experiment I have illustrated shows how modeling in science (natural philosophy, for Galileo) is *constitutive* of central aspects of the target system that is studied, and surely it is not a fiction; 2) Galileo also explicitly says in *The Assayer* that we do not have absolutely to think that science "is a book of fiction by some writer, like the Iliad or Orlando Furioso, productions in which the least important thing is whether what is written there is true".

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