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Scientific cognition based on models as epistemic warfare

Do scientific models serve as epistemic weapons or fictions?

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Abstract. Using the idea of "epistemic warfare", which views scientific exploration as a complex battle for rational knowledge in which it is critical to distinguish between epistemic (such as scientific models) and non-epistemic (such as fictions, falsities, and propaganda) weapons, I will demonstrate in this article how scientific modeling activity can be better described. I will go into more detail about a dynamic perspective on models as well. It is incorrect to evaluate models in research by confusingly combining dynamic and static elements of the scientific research processes. To an epistemologist, scientific models presented from a static perspective (as in a textbook, for example) undoubtedly seem fictitious, but when a dynamic perspective is used, this fictitious quality vanishes.

1 Do scientific models serve as epistemic weapons or fictions?

As a result of current cognitive research, we are aware of the following implicit assumptions that Charles Sanders Peirce made: The way in which nature nourishes the mind is by means of the mind's disembodiment and expansion in nature—a process that might be described as "artificialization"—which in turn influences the mind. In more contemporary terms, models are constructed, for instance, by the scientist's mind, which first assigns "meanings" to external objects of various kinds. In this way, "internal" representations are "extended" in the environment, and subsequently, processes that take place outside will reshape them while also taking into consideration the constraints found in the external representation (a *model*, for example). Following the external model's alteration, the ensuing aspects of those modifications/movements are "picked up" and in turn re-represented in the human brain of the scientist.

This viewpoint allows us to enjoy the speculative Aristotelian prediction that "*nihil est in intellectu quod prius non fuerit in sensu*", now inside a naturalistic context. These modifications can readily coincide with (or lead to) new guesses—either instinctive or reasoned, depending on the brain areas involved—that is, plausible abductive hypotheses about the external

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extra-somatic world. In the case of science, this is because the information flowing from the model allows the scientists' internal models to be rebuilt and further refined. The process may be viewed from the standpoint of the notion of cognitive niches:[1] when the mind constructs the so-called cognitive niches over the history of culture, it grows up with its representational delegates to the outside world. The complex cognitive niche of a scientific laboratory at the same time is an "epistemic" niche, specifically designed to advance knowledge through cognitive processes, in which "people, systems, and environmental affordances" (Chandrasekharan 2009, p. 1076) interact harmoniously. Nersessian and Chandrasekharan's (2009) research on various cognitive processes that characterize a scientific lab focuses on models that heavily refer to movement and ignores models that are not essentially based on it, it still offers a helpful example that highlights the distributed nature of scientific models and the true kind of abstraction and ideality they possess, reinvigorating ideas from the history of philosophy of science.

Recasting Contessa (2010)'s definition of a model as "an actual abstract object that stands for one of the many possible concrete objects that fit the generative description of the model" (p. 228) in the context of the current naturalistic perspective, this perspective would benefit of the analysis of models as material, mathematical, fictional, and "abstract objects." "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", states Frigg (2010), introducing a fictionalist viewpoint. 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, which can easily answer the question "where are models located, from a naturalistic point of view?"

From this angle, scientific models cannot easily be considered fictions because, at least when it comes to the cognitive processes involved in scientific discovery, scientists do not intend to put forth fictions; rather, they provide models as instruments that help reshape a general cognitive niche as an epistemic niche in order to carry out a sincere effort to represent the outside

[1]The cognitive human acts that convert the natural world into a cognitive one are known as representational delegations to the external environment that are configured as elements of *cognitive niches* (some of which may be seen as pregnances; see Magnani, 2022, Lexicon of Discoverability). According to research conducted in the field of biosciences of evolution by Odling-Smee, Laland, and Feldman (Odling-Smee et al., 2003; Laland & Sterelny, 2006; Laland & Brown, 2006), humans have created enormous cognitive niches that are characterized by informational, cognitive, and ultimately computational processes.

world. Models, the war machines employed in this conflict, which I refer to as "epistemic warfare", are only concrete, clear-cut, and well-designed tactical intermediate weapons capable of strategically "attacking" nature (the target systems to be studied) in order to further reveal its structure. They emphasize the determined—strictly epistemic—dynamism of the adopted tools that are at stake. On the other hand, fictions in fiction works aim, for instance, to expose human life and characters from fresh artistic angles and/or to critique them via a moral lesson, whereas fictions and military tactics aim to deceive the adversary and potentially destroy the eco-human targets (the target systems) in order to expose the structure of those targets even more.

I argue that even while the "military" character of various cognitive processes is not immediately apparent in different features and applications of syntactilized human natural language and in abstract knowledge, epistemologists do not need to ignore it. It is challenging to identify this "military intelligence"[2] in the various *epistemic* roles that natural language serves. For instance, it is difficult to observe this "military intelligence" at work when language is merely used to transmit scientific results in a classroom setting or when we obtain weather information from the Internet that is expressed in linguistic terms and numbers. However, we must not lose sight of the fact that information packages entrenched in certain language use—and in hybrid languages, such as mathematics, which includes a substantial amount of symbolic language—even with their more abstract nature, still have a tremendous impact in modifying the moral behavior of human collectives. In human social groupings, for instance, the creation and dissemination of new scientific knowledge involves not only the operation of information but also the implementation and distribution of roles, talents, limitations, and action options. This process has intrinsic moral value because it produces precise distinctions, powers, duties, and opportunities that may either modify pre-existing conflicts or lead to the emergence of new, violent, intragroup conflicts.

Allow me to give an example. Two opposing moral/social effects are typically associated with new theoretical biomedical knowledge about pregnancy and fetuses: (1) improved social and medical management of childbirth and related diseases; and (2) possible escalation or modification of conflicts regarding the legitimacy of in-vitro fertilization, abortion, and other related practices. All things considered, even the most abstract bodies of knowledge and seemingly harmless bits of information are subject to the semio/social

[2]I am borrowing this expression from René Thom (1988), who links "military intelligence" to language and cognition's role in "coalition enforcement," or the level of their complementary effects in confirming morals and associated behaviors and, ultimately, carrying out potential violent penalties. It is clear that the term "military" has in this case a metaphorical meaning.

processes that determine the identities of groups and their capacity for aggression when forming coalitions. Argumentative, deontological, rhetorical, and dialectic elements are all too often present in declarative knowledge and deductive reasoning. It is difficult, for instance, to distinguish between the argumentative or deontological function of language and a type of "pure" (such as deductive) inferential one in an eco-cognitive context. It is clear that the deductive function of language, for instance, may simultaneously play an associated argumentative role. But the arguments that are conventionally acknowledged as "fallacious" are the ones that help us better understand the military character of human language and, in particular, and especially of some hypotheses reached through fallacies.

Therefore, we must recognize that while science positions itself as a paradigm for generating knowledge in a particular "decent" way, it also inadvertently participates in the cross-disciplinary conflict that is the hallmark of modernity. Science engages in conflict with other non-scientific fields, as well as with literature, magic, religion, and other non-scientific fields. It also subtly orders and norms societies through the use of technological products that enforce morality and behavior. Of course, propaganda plays a role in scientific cognitive processes—sensu strictu, inside scientific groups as coalitions—as Feyerabend notes (Feyerabend 1975). For example, propaganda may be used to persuade colleagues about a hypothesis or a method. However, propaganda also plays an external role, reaching out to other private and public coalitions as well as the general public in order to obtain funding—a crucial issue that is frequently ignored in modern science is the cost of producing new models—or to persuade about the value of scientific knowledge. However, when the creation of its own regimen of truth is at risk, its core cognitive processes are based on avoiding fictional and rhetorical devices. Ultimately, science is precisely that endeavor that generates the types of realities that articulate the paradigms for distinguishing fictions and, hence, "irrational" or "arational" modes of knowing.

I am aware that epistemological fictionalism views fictions as something "we cherish" (Frigg 2010, p. 249) and something "far from being execrable"; however, to say that literary and scientific fictions are equally "good" fictions would be oversimplifying the problem a little bit, as science is the one that created new types of models that go beyond poetry and literature and are dedicated to a particular production of a rational truth, *constitutively* aiming at *not being fictional*. Admittedly, I fail to understand how the perfect pendulum could be discussed in the same vein as 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.

Frigg and Nguyen (2017) wrote a lengthy piece discussing models and representations, just released in the *Handbook of Model-Based Science* (Magnani and Bertolotti 2017): the many ideas of scientific models (structuralist, inferential, fictionalist, representational, in terms of stipulative fiat or of similarity) are shown in great detail by the writers. When discussing fictionalism, they cite my article (Magnani 2012) and note that I reject the fictionalist view because it misinterprets the role that models play in the process of scientific discovery. I contend that these models cannot be indicated at all as fictional because they are the foundation of new empirical domains and scientific frameworks. They claim that because falsities are unable to contribute to the formation of new empirical domains, my criticism appears to be predicated on the idea that fiction is false. Finally, they respond that the fiction perspective is not subject to my issue since it is not devoted to the "fiction as falsity" account.

I can agree that fictions do not always contain falsity and that within literary frameworks, fictions can be understood as imaginations rather than as falsities—presumably carrying some sort of truth or at least potential truth—but that is precisely the problem. I continue to believe that there is a distinct difference between what are referred to as works of fiction (literature, for example) and non-fiction (science, for example), and that even if we choose to attribute to both types of knowledge some positive cognitive functions, we are dealing with very different kinds of cognitive processes that cannot be completely confused.

Furthermore, I read the article "Models and explanation" by Bokulich (2017) that was also included in the quoted *Handbook of Model-Based Science*. It illustrates numerous instances of the constructive and unavoidable roles that idealizations—as well as those that are considered "fictions"—play in science, not only in cognitive creative processes. She contends that certain so-called fictions are actually informative and produce true scientific cognition because they are able to authentically depict real patterns of structural interdependence in the real world in their fictional representations. However, as I said I came to the conclusion that it is strange to adopt the term "fiction" in epistemology. The reader should consult this text in order to receive a concise and well-written response to the remaining queries: do some highly abstract and mathematical models exhibit a non-causal form of scientific explanation? How can a "how-actually" model explanation be distinguished from an exploratory "how-possibly" model explanation? Do modelers have to make trade-offs such that, for example, a model that is best at producing explanatory outcomes may not be the most accurate predictor, and vice versa?

The in-vitro model and Anna Karenina are quite unlike. In true scientific practice, a model only qualifies as fictitious when it is acknowledged as such by the research community due to its *inability* to fruitfully describe the target systems. In these situations, a model is just abandoned within the changing context of scientific research.

Instead, Tolstoy would have replaced Anna Karenina with another, equally fictional character who would have remained unreal forever. Tolstoy might have rejected Anna Karenina as an inappropriate fiction for some modern aesthetic—not scientific—purpose, for example, had she failed, in her author's opinion, to authentically represent a female member of Russia's high society at the end of the 19th century. Giere helpfully points out that, conversely, "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).

2 A dynamic perspective on scientific models as fictions

A scientific model can be abandoned, as I mentioned a few lines above if it is unable to effectively reflect the target systems and contribute to scientific cognitive processes. It is simpler to recognize that a scientific model can be more accurately classified as "fictional" in a cognitive (sometimes creative) process when it is determined to be ineffective by applying the *negation as failure* (Clark 1978; Magnani 2001). This is because a scientific model becomes fictional in the sense that it is falsified (even if "weakly" falsified by failure), and as a result, it ceases to be relevant in the "rational" life of scientific cognition. Regarding the compelling and cohesive examination of relationships between theories, which encompasses the issue of inaccurate model representation—as well as the replacement or modification of models—and the incompleteness of scientific representation, concerning partial structural similarity, see (Bueno and French 2011) and the seminal work (da Costa and French 2003).

The process of eliminating something through negation is methodologically similar to what Freud describes when constructions (the stories the analyst creates about the patient's past psychic life) are dropped because they do not advance the therapeutic psychoanalytic process: if the patient does not offer new "material" that expands the suggested construction, "if," as Freud states, "[...] 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 1974, vol, 23, 1937, p. 262).

Similar to this, in the process of scientific discovery, for instance, the old model is buried in the necropolis of the no longer useful—dead—models and is simply eliminated and labeled as "false" because "new material has come to light" to provide a better model that in turn will lead to new knowledge that supersedes or refines the previous one. However, in the entire scientific endeavor, a successful scientific model (like the ether model) may also be arbitrarily eliminated along with the theory that supported it. As a result, the outdated model is buried in yet another necropolis—that of the abandoned "historical" models—and in this instance, it is indeed plausible to reclassify it as a fiction.[3]

Woods and Rosales (2010) provide a thorough and convincing logico-philosophical investigation of the issue at hand, leading them to a conclusion that is in line with my suspicions regarding the fictional nature of scientific models. They argue that applying the notion of literary and creative fictions to science and other branches of cognition is incredibly perplexing. There is "nothing true of them in virtue of which they are literary fictions", regardless of what we say about the fictions of science and mathematics (p. 375). "Saying that scientific stipulation is subject to normative constraints is already saying something quite different from what should be said about literary stipulation" as they properly point out.

In my previous research, I always emphasized what I called "mimetic" external scientific models: in the case of semiotic cognitive processes occurring in science, the external scientific models are *mimetic*, to emphasize the fact that the mind disembodies itself, performing a cognitive interplay between internal and external representations, and possibly, *creative* (in this last case, they are not necessarily mimetic). This distinction reflects the one Morrison made between idealized (mirroring the target systems) and abstract models (more creative and finished to generate new scientific intelligibility), as we will see in Section 4 below.

I find this interplay crucial for analyzing the relationship between meaningful semiotic internal resources and devices and their dynamic interactions

[3]The importance of "understanding" in science is also connected to this issue in contemporary literature: de Regt (2015, p. 3782) addresses the perplexing relationship between scientific understanding, false models, and realism. The author claims that understanding can be—and frequently is—achieved through models that are unrealistic, highly idealized representations of the target system, or on the basis of theories that are, in and of themselves, false, or through models and theories that, despite being disproven now, did not stop them from adding to our understanding of phenomena. These insights are supported by the practice and history of science.

with the externalized semiotic materiality already stored in the environment (scientific artifactual models, in this case), as I am attempting to demonstrate in this article through the description of an intellectual framework that considers models material and distributed. Because this outward materiality shows (and functions within) its own cognitive limits, it plays a particular role in the interaction. Therefore, minds are artificial and "extended" in nature. It is in this perspective that I also have to emphasize the significance of what I dubbed manipulative abduction at the level of that ongoing interplay between online and offline intelligence.

As I have explained (Magnani, 2001), manipulative abduction is a process that is commonly used in scientific reasoning to form and evaluate hypotheses. It primarily involves extra-theoretical and extra-sentential behavior that aims to create communicable accounts of new experiences in order to integrate them into systems of experimental and linguistic (theoretical) practices that have already been established. As I have stated, manipulative abduction is a sort of redistribution of the cognitive and epistemic effort to handle things and data that are not readily represented or located internally. The building of external models by humans with the intention of performing observations and "experiments" that might change one's cognitive state in order to reveal new characteristics of the target systems is precisely an example of manipulative abduction. The more impromptu and unconscious action-based cognitive processes that I have described as types of "thinking through doing" are also included in the definition of manipulative abduction.

3 We do not need to mix up static and dynamic aspects of the scientific enterprise

At this point, I may also argue that, in the case of creative processes, the produced external scientific model is precisely the opposite of a fiction as well as a general process of make-believe (neither is a barely credible world (Sugden 2000, 2009) nor a mere surrogate, as (Contessa 2007) puts it). Instead, it is a *regulatory* tool *stabilized* in "some exterior form", a sort of reliable anchorage, and it is not purposefully constructed as fiction, unlike a romance author who may purposefully create the character of Harry Potter. The usage of the term "fiction" in epistemological fictionalism about models is typically justified by the absence of empirical systems that match, for instance, the ideal pendulum (and its equation).

The label creates a paradox that is easy to comprehend by using the example of scientific models that are seen as "missing systems," which is a fresh metaphor that resembles the fictional one. In fact, the description of a missing system might be a fiction. According to Thomson-Jones (2010), science is rife with "descriptions of missing systems," which are ultimately regarded as abstract models. Furthermore, Mäki (2009) expands on the missing systems framework by providing an additional metaphoric conceptual apparatus: missing systems are also "surrogate" systems expressed as credible worlds, as models. Mäki (2009) acknowledges that scientific models are "pragmatically and ontologically constrained representations." Godfrey-Smith (2009, p. 114) makes similar arguments: "To claim that talking about model systems is a psychologically unusual way of looking into conditionals (and the like) is not enough to overcome the problem by itself. It is normal to assume that the useful output that comes from modeling is often a conditional, i.e., a statement that would be true if a specific configuration existed. The challenge of elucidating the empirical use of this type of information resurfaces, nevertheless, as the configurations in issue are often known not to exist".

Similar arguments are advanced by 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".

In my view, the missing system (Thomson-Jones)—at least in the creative scientific cognitive processes—is not the one represented by the "model," but rather the target system itself, which is still essentially unidentified and un-schematized. This system will only appear to be "known" in a novel way upon acceptance of the research process results, which are then admitted into the theory T and considered worth *staying* in T thereafter. The same is true of models, which Godfrey-Smith refers to as configurations. While they are undoubtedly conditional, models do not necessarily need to be regarded as "known *not* to exist" in Godfrey-Smith's sense because at the very moment a scientific model is introduced during a discovery process, it is the only thing we can reasonably know to exist (for example, a diagram on a blackboard, an in-vitro artifact, or a mental imagery).

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. This is an exaggeration which Morrison corrects when she is pretty clear about the excessive habit of labeling fictional scientific models simply because they are superficially seen as "unrealistic": "Although there is a temptation to categorize any type of unrealistic representation as a 'fiction', I have argued that this would be a mistake, primarily because this way of categorizing the use of unrealistic representations tells us very little about the role those representations play in producing knowledge" (Morrison 2009, p. 133).

In the framework of an account of scientific representation in terms of partial structures and partial morphisms Bueno and French (2011, p. 27) admit that they agree in the fact that an important role for models in science is to allow scientists to perform the so-called "surrogative" reasoning, but they add the following constraint: "Indeed, we would claim that representing the 'surrogative' nature of this reasoning effectively rides on the back of the relevant partial isomorphisms, since it is through these that we can straightforwardly capture the kinds of idealizations, abstractions, and inconsistencies that we find in scientific models". We can therefore talk about surrogates, fictions, plausible worlds, etc., but we cannot be certain that we are in the presence of a "scientific" representation or model until we can identify the appropriate partial isomorphism following the model's success.

Furthermore, Kuorikoski and Lehtinen (2009, p. 121) assert 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, since only the *co-exact* premises are used in various heuristic processes, the incorrect premises (also caused by the existence of models of both substantive and auxiliary assumptions) are not used in the cognitive process. Manders (2008) presented the idea of co-exact characteristics in geometrical cognition, and it is worth studying in areas outside of traditional geometry discovery procedures, where it has been beautifully highlighted. In turn, Mumma (2010, p. 264) provides an example of how Euclid's diagrams only add co-exact characteristics to proofs.

In conclusion, I believe it is erroneous to examine scientific models by embracing a foundational confusion of static and dynamic elements of the scientific endeavor. When scientific models are placed in a static context, such as a textbook, they do appear fictional at first because they are immediately compared to the target systems and their intricate experimental apparatuses. However, this also highlights the ideal nature of the models and their explanatory power (cf. Weisberg 2007). On the contrary, scientific models observed within the dynamic processes of scientific creativity—the central theme of epistemology at least since Karl Popper, Thomas Kuhn, and Imre Lakatos—appear to be *explicit* and *reproducible* mechanisms purposefully constructed and altered to further the gnoseological goals of expanding the body of knowledge *not yet available*.

Morrison (2009) makes it clear that models are not fictions, emphasizing that in science they are specifically related to ("finer graded") ways of understanding and explaining "real systems," which go far beyond their approximation benefits and more collateral predictive capabilities. She does, in fact, go on to clarify that because they are "necessary" to arrive at certain results, models that are appropriate to refer to as abstract resist corrections or relaxing of the unrealistic assumptions (as in the case of mathematical abstractions or when models furnish the sudden chance for the applicability of equations) in the so-called process of de-idealization.

According to Cartwright (1989), the main characteristic of these models is not that "relevant features" are removed in order to concentrate on a single, isolated set of properties or laws; rather, what matters is their ability to provide a comprehensively new representation of an empirical (and/or theoretical, as in the 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). Similarly, Woods (2013) concludes that the development of non-probative premiss-conclusion connections in model-based science plays a major role in empirically forlorn representations, preparing links in ways that set up their conclusions for empirical negotiation at the checkout counter.

Certain other models are more appropriately categorized as *idealizations* since they are simpler to define and permit the inclusion of corrective variables that allow "[...] for the addition of correction factors that bring the model system closer (in representational terms) to the physical system being modeled or described" (Morrison 2009, p. 111). It is, for example, the case of a 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 explanation/prediction of the target system" (p. 112), and which provides information and transfer of knowledge.

Morrison's description of scientific models as abstract aligns with my focus on models as constitutive, going beyond the function of models as idealizations allowing for adjustments and improvements. According to this viewpoint, "abstract" models—whether they have to do with mathematization preparation and support or directly involve mathematical tools—must be conceived of as poietic means of generating fresh insights into the salient characteristics of the phenomena under study, rather than as simple means of making cognitive processes easier. If idealization *resembles* the phenomena to be better understood, abstract models can *constitute* the resemblance itself, as I will illustrate in the following section.

The argument made by Mäki (2009, p. 31) 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" is something I must draw attention to because, at least in some creative processes, the adopted model (for instance, in the case of creative thought experiments) is not necessarily implemented through the "removal" or "neutralization" of real-world features.

This is because, ironically, some features of the target system—that is, the supposed real world—have not yet been discovered, so they are the ones that are still "missing." As a result, it is hard to envision that some parts of the model come from the removal of real-world traits; instead, those qualities may come from the cognitive process that created the model in the first place in order to achieve that goal. However, because the systems we wish to subrogate *are mainly unknown*, it is challenging to consistently claim that models represent a "surrogate" system.

4 Resemblance and Feyerabend's counterinduction

In the epistemological context of missing systems (and related subjects, fictions, surrogate systems, credible world, make-believe models, etc.), even the idea of resemblance (similarity, isomorphism, homomorphism, etc.) is debatable. "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 argue that resemblance, at least in scientific discovery processes, is inherently partial because it is very hard to suitably resemble things that are not yet known. Actually, it is just the work of models that of creating, in a poietic way, the "resemblance" to the target system. Some discovered properties of the target system resemble the model not because the model resembled them a priori but only post hoc, once discovered thanks to the creative modeling activity itself: the new properties appear well-defined only in the static analysis of the final assessed theory. Morrison also asserts 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).

From this angle, the received view is ironically reversed; we may argue that the newly discovered target system is the one that bears similarities to the model, which is the source of those similarities. Often models are fruitful in discovering new knowledge just because they do not—or narrowly—resemble the target systems to be studied, and are instead built with the aim of finding a new general capacity to make "the world intelligible".[4]

[4] I think that a better understanding of ideas like similarity, imaginability, conceivability, plausibility, persuasiveness, and creditworthiness (Mäki 2009, pp. 39–40) would benefit from being examined within the rigorous and multidisciplinary context of abductive cognition, which is overlooked in the studies of the "friends of fiction" except for Sugden (2000, 2009).

Feyerabend (1975) places a strong emphasis on the function of contradiction in contrast to the role of resemblance in his book *Against Method*. He develops a "counterrule" that is the antithesis of the neopositivistic one according to which "experience" or "experimental results" determine the viability of our theories. This last one is a crucial rule that is at the basis of all theories of confirmation and corroboration. The counterrule suggests that we put out and develop theories that contradict accepted theories and/or accepted facts. Feyerabend emphasizes the importance of "dreaming," but these are Galileo's dreams, not fictions. As I have already mentioned, Feyerabend made a clear distinction between scientific tools (as modeling) and propaganda, which can instead be organized through fictions, inconsistent thought experiments, mistakes, aggressive fallacies, and so on, but that do not play any epistemic role in the specific cognitive process of scientific discovery. I have framed this type of propaganda under the wider concept of "epistemic" warfare.

Returning to the issue of models serving as surrogates, Mäki (2009, p. 35) states: "The model functions as a surrogate system: it is construed and examined with a desire to learn about the secrets of the real world. One yearns for such learning and sets out to build a model in an attempt to satisfy the desire. Surrogate models are intended, or can be employed to serve, as bridges to the world".

First, I would expand on the phrase "secrets of the real world" by adding a few auxiliary remarks. I would warn about the preferability of being post-Kantian by admitting that, through science, we are *constructing* our rational knowledge of the world, which is still objective and apart from us, but it is built. If we say we build surrogate systems to learn about the secret of nature, a debatable 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".

In my opinion, the term "surrogate models" should only be applied to models used in some "sciences" that are unable to produce adequate knowledge about the 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 the term "surrogate systems" for them, as they fake a scientific knowledge that is not satisfactorily attained from a variety of angles.[5]

[5]It is important to remember what Morrison says: "Laws are constantly being revised and rejected; consequently, we can never claim that they are true or false"(Morrison 2009, p. 128).

As I mentioned before, there are epistemological issues with the idea of a model as make-believe. In fact, make-believe processes are present in practically all human intersubjective interplay. Here, I may emphasize once again how broad the concept of a credible world is: every cognitive process that seeks to provide information that is both scientific and non-scientific likewise seeks to provide credible worlds. Building scientific models, or the subclass of *epistemologically* credible worlds that effectively lead to scientific ideas, is the dilemma facing science. In this vein, Sugden (2009, p. 10) suggests that an epistemologically "good" credible world would have to be provided by models that are able to trigger hypotheses about the "cause of actual events," that is, in situations where "the fictional world of the model is one that *could* be real". It is beneficial to use Cartwright's classical approach (Cartwright 2009) on capacities:

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

In his cautious analysis, Sugden views these overly optimistic viewpoints on models as instruments for separating the "capacities" of causal factors in reality. He also offers alternative conceptual frameworks to preserve other supposedly weaker aspects of epistemological "sciences," such as certain areas of economics, psychology, or biology, which are never able to achieve the goal of revealing capacities.

In order to rescue these disciplines, he claims that models can only offer "conceptual explorations," which in turn help create plausible counterfactual worlds or really explanatory theories that can lead to inductive (or "abductive") inferences that explain the target systems. Strong methodological claims like those made by Cartwright should, in my opinion, be approached with caution, but there is still an open epistemological question: in the case of models used as conceptual exploration, are they used to depict plausible worlds that can reach a satisfactory theorization of target systems, or are they just providing ambitious but unjustified hypotheses that lack various sound epistemological requirements?

Using Cartwright's strict demarcation criteria, which is restated in "If no capacities then no credible worlds" (Cartwright 2009), it would seem that no more citizenship is allowed to some post-modern exaggeration in attributing the label "scientific" when referring to proliferating fields of academic production of knowledge, from (parts of) psychology to (parts of) economics, and so on, areas which do not—or scarcely—respect the most common accepted epistemological requisites, for example, the *predictivity* of the phenomena that regard the explained systems.

Are we certain that this line is excessively strict, or is it time to call out certain excesses in the abundance of models deemed to be "scientific"? In the "military" framework of the academic struggle between disciplines, which is 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 are, at best, fragments of "bad philosophy", the epistemological use of the so-called credible worlds appears theoretically suspect but ideologically clear.

The unstable state of several areas of psychological study provides an illustration. Miller (2010, p. 716) examines three claims: "[...] 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 also provides a thorough illustration of the misguided or epistemologically perplexing attempts to localize psychological functions[6] through neuroimaging, as well as the misconceptions surrounding the contribution of genetics to psychopathology, sadly intertwined with untoward constraints on healthcare policy and clinical service delivery.

5 Conclusion

I have argued in this work that scientific models are not fictions. I have maintained that there are serious inadequacies in other related epistemological approaches to model-based scientific cognition (in terms of surrogates, credible worlds, missing systems, and make-believe), which can be identified by utilizing the idea of manipulative abduction and recent cognitive research conducted in scientific labs. The concept of "epistemic warfare," which views scientific enterprise as a complex struggle for rational knowledge in which it is crucial to distinguish between epistemic (such as scientific models) and extra-epistemic (such as fictions, falsities, and propaganda) weapons, has been proposed as a further means of outlining a more satisfactory analysis of fictionalism and its discontents. I come to the conclusion that when models in scientific contexts are fictions, it is because they were merely thrown out as heuristic steps gone wrong, dismissed thanks to a form of negation

[6] Cf. for example (Glymour and Hanson 2016).

as failure. By confusing the static and dynamic aspects of the scientific enterprise I have also demonstrated how misleading it is to analyze models in science. In fact, the static perspective overemphasizes the potential fictional nature of models because the creative/active role of modeling is openly or purposefully ignored. I have finally taken a look at Feyerabend's helpful concept of counterinduction, which challenges the significance of resemblance in model-based cognition. This viewpoint has led me to paradoxically arrive at the opposite of the received view: it is the newly known target system that resembles to the model, which itself originated that resemblance.

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