THEORETICAL MODELS AS REPRESENTATIONS

My aims here are, firstly, to suggest a minor amendment to R.I.G. Hughes' DDI account of modeling, so that it could be viewed as a plausible epistemological "model" of how scientific models represent and secondly, to distinguish between two epistemological kinds of models that I call "descriptive" and "constitutive". This aim is achieved by criticizing Michael Weisberg's distinction between models and abstract direct representations and by following, at the same time, his own methodological flair for such a distinction.

Key words: descriptive and constitutive kinds of models, inferential conception of scientific representation, scientific models, surrogative reasoning.

1 A few words about Models in Science

The nature and cognitive features of scientific models lately enjoys an increasing philosophical interest (Giere 1999; Teller 2001; Bailer-Jones 2003; Suárez 2004; Potochnik 2007; McAllister 2007; Weisberg 2007; Contessa 2007; Goodwin 2010). It is focused both on models as used in scientific practice, and on their formal epistemological presentation within the so called semantic conception of scientific theories.

Models in science are usually and broadly defined to be visual or abstract theoretical *representations* of putatively real objects, relations, and processes, constituting some fragment of the world. Their basic cognitive role is usually identified with their inferential aspect, i.e. with the possibility of knowledge to be obtained from the model and transferred to the state of affairs within the modeled subject of research. It seems, however, that there is little agreement concerning the details how models are representational and how they are inferential and even concerning the types of scientific representations. Marx Wartofsky referred once to this lack of agreement as “the model muddle” (Wartofsky 1979, 1), while Carl Craver admits that “the term “model” is notoriously ambiguous” (Craver 2002, 65).

My aim here is, firstly, to suggest an amendment to R.I.G. Hughes’ DDI account of modeling, so that it could be viewed as a plausible epistemological “model” of how scientific models represent; and secondly, to differentiate two epistemological kinds of models that I call “descriptive” and “constitutive” kinds of models. This aim is achieved by criticizing Michael Weisberg’s distinction between models and abstract direct representations, and by following, at the same time, his own methodological flair for such a distinction.

2 Surrogative Reasoning

The inferential aspect of theoretical models is expressed in the opportunity *to infer* states of affairs (properties, relations, dependences, etc.) on the basis of formal operations within the model *from the model towards the modeled system*. To this effect, the recently accepted methodological jargon for a modeled system is being nailed as ‘target system’; and the inferential procedure has recently been called ‘surrogative reasoning’. Mauricio Suárez uses the expression “surrogate reasoning” (2004, 769), but I’ll make use of the first expression in the rest of the paper.

> “Surrogative reasoning” is the expression introduced by Chris Swoyer (…) to designate those cases in which someone uses one object, the *vehicle* of representation, to learn about some other object, the *target* of representation. A good example of a piece of surrogative reasoning is the case in which someone uses a map of the London Underground to find out how to get from one station on the London Underground network to another. The map and the network are clearly two distinct objects. One is a

piece of glossy paper on which colored lines and names are printed; the other is an intricate system of, among other things, trains, tunnels, rails, and platforms (Contessa 2007, 51).

The just quoted lines are "a good example", indeed, of surrogative reasoning how someone could infer the correct moves from the model (the map) on a real Underground trip. Theoretical models, understood in the same way as "vehicles of representation", allow for similar inferential steps (via surrogative reasoning) and in so doing provide valuable knowledge about the target system (often taken to be some fragment of the world). Thus, an astronomical model of the solar system, no matter whether it is based on the Ptolemaic or the Copernican paradigm, is supposed to predict, for instance, when a solar eclipse would occur, and of what kind it would be, total or partial. Specific knowledge is obtained from the theoretical model for the purpose of describing and predicting the real movements of heavenly bodies, near the Earth.

Non-linguistic theoretical models like the structural formulas in organic chemistry "are used as models in chemists' attempts to explain the chemical or physical properties of compounds in terms of their structural characteristics" (Goodwin 2010, 622).

The inferential aspect of theoretical models, the already introduced surrogative reasoning, is the possibility of new knowledge to be inferred from the model towards the target system. It appears at that that the inferential and the representational aspects of models are inter-dependent. The very feasibility of surrogative reasoning is an intrinsic feature of theoretical models because if this feature is absent, they would fail to provide an adequate representation of the target system, as well. For the hope for a model to enable surrogative reasoning, when it does not adequately represent the target system, does not rest on reasonable grounds. The inferential and the representational aspects of models go hand in hand and this seems to have incited Suárez and Contessa to introduce and to elaborate a view under the name *'inferential conception of scientific representation'*.

3 How Theoretical Models Represent Their Target Systems?

Expressions such as 'scientific representation', 'this model adequately represents…', and the like, are quite often used unproblematically. But what is it for a model, the "vehicle of representation", *to represent* its target system?

The complexity of this question is a tacit excuse for the lack of a widely accepted answer, replaced by various suggestions.

It is near to commonsense to admit that a material (static or mechanical) model bears a *resemblance* to its target system. Models of architectural ensembles or of car accidents for example, are designed to keep a close resemblance with the envisaged structure, or with the studied dynamic event, for different purposes: educational, safety providing technical solutions, etc. Unlike material models, theoretical models fall short of this requirement. They represent by involving abstract concepts, usually constructed through formal languages. An author, who is otherwise sympathetic to the idea of isomorphism in scientific representation, takes it for granted that "we do not simply model a phenomenon, we model it *as* something. Thus, for example, we model a gas as a system of billiard balls" (French 2003, 1478). Gravitation is modeled as a mathematically presented central force, mutually attracting material bodies to one another, in classical science, and as an effect due to the local curvatures of space-time in the general theory of relativity. For similar reasons S. French readily supports the claim that *"models denote and do not resemble"* (ibid.).

That models denote their target systems is something widely accepted by many philosophers. R.I.G. Hughes for example has taken the notion of denotation and has combined it with two other notions, in order to offer “a model” of how theoretical models apply to parts of the physical world. His conception bears the name *DDI account of modeling* (Hughes 1997). In his account a model represents a physical system if: (1) it denotes it, (2) it is able to demonstrate appropriate conclusions, and (3) these conclusions are interpreted in terms of the system to provide new knowledge about its features. Thus a theoretical model involves three components: denotation, demonstration, and interpretation, the naming of which prompts the abbreviated name of his account.

R.I.G. Hughes insures himself against the claim that theoretical models are similar to, or resemble, their target systems (1997, S329) – a claim which S. French has later also denied. But *what is it for a model to denote its subject?* We know what it is for a term to denote something (from semantics), or to have a referent (from general epistemology). The notion of denotation is not kept the same, however, if we apply it to a model as a whole. And it changes for two reasons.

The first one comes from the mere fact of the altered usage of the term. We are not inclined to say that models denote in the same sense, which we have in mind, when saying that a separate word denotes.

The second reason is that the meaning of ‘model denotation’ is not (or at least is not always) additive, that is to say, theoretical models do not denote by summarizing the denotations of the separate terms which enter their vocabulary. Because some terms, even playing a key role in a model, may not denote anything. Let me turn to a successful model in physics, often taken as an example for different purposes – Bohr’s model of the hydrogen atom. (I call it “successful”, because it marks, in the words of Imre Lakatos, the beginning of a progressive research programme in quantum physics.) It is metaphorically named a planetary model of the atom, since the electron orbits the hydrogen nucleus – the proton, just like a planet revolves around the Sun. Thus the concept of the orbiting electron plays a central role in the model. It is also involved in the explanation of why a photon of light, with a definite frequency, is emitted out of the atom whenever the electron "jumps" from an outer to an inner orbit. This concept, however, has later proved to be inapplicable, since the electron within the hydrogen atom (in a free state) is really situated around the nucleus but does not follow the path of some definite spatial orbit.

For all these reasons the notion of denotation is ambiguous, except for notifying that a model stands for its subject and is directed to it with the pretension to represent it. Following Goodman, R.I.G. Hughes is satisfied to say about denotation that it means that the model is 'a symbol' for its subject (1997, S330). *Denotation is merely the intentional aspect of representation.* Without using the term, Suárez includes this aspect in a scant definition of his inferential conception of scientific representation (ICSR):

> *A* represents *B* only if (i) the representational force of *A* points towards *B*, and (ii) *A* allows competent and informed agents to draw specific inferences regarding *B* (Suárez 2004, 773).

In this definition the notion of denotation falls entirely into the requirement (i). We can agree to this effect, that *it is only an element of what should be understood under the broader notion of representation*; and the DDI account of representation clearly claims that. Yet I pay attention to the notion of denotation, not to reach the conclusion that (in terms of the ICSR) *A* denotes *B*, if "the representational force of *A* points towards *B*". It seems to me that *the cognitive function of denotation is not completely reducible to a sheer symbolization* of a putatively real object *B* by a model *A*. If this were so, then one could hardly distinguish between a scientific representation and a mere sign, taken conventionally, to represent an object. Turning back to the example, used by G. Contessa, the logo of the London Underground denotes the latter in the same way as its map does, but it does not *represent* it in the same way as the map. Unlike the logo, the map offers an *epistemic representation*, i.e. a user of the map can perform valid surrogative inferences from the map to the underground network (Contessa 2007, 52).

We can conclude from here, that if *A* is a theoretical model of *B* (i.e. if it is an epistemic representation of *B*), then *A*'s denoting has to mean *something more* than signifying *B*, or merely 'pointing towards *B*'. The first element of Hughes' DDI account must obtain a broader construal. What is it?

To answer the question, let me briefly consider the model analyzed by Hughes himself, in order to show how his DDI account works. This is Galileo's kinematical model of 'naturally accelerated motions'. The model is geometrical and aims at describing how the speed of a uniformly accelerated body (falling down from a given height to the Earth's surface) changes in comparison to the speed it has, when it moves uniformly (without acceleration). Time is denoted in the model by a vertical straight line, divided by equal intervals, and the speeds of the falling body at the end of each time interval – by lengths of horizontal lines (Hughes 1997, S326-S328). The geometrical model allows Galileo to attain regularities concerning paths covered by the falling body in equal time intervals.

At first glance, one finds the representation of time to be a practical way of signification allowing, after a demonstration, a useful interpretation of the geometrical ratios in terms of the modeled physical system (or in ICSR language – a valuable surrogative inference). But at a second glance, one may ask whether Galileo's way of representing time and equal time intervals, is conventional, produced merely for the sake of convenience? And here the answer, I think, is negative. The reason why Galileo represents time by a straight line could be found in a preliminary grasping of time as one dimensional and linear and the possibility to be divided into equal time intervals – in a preliminary grasping of time to be homogeneous and to flow uniformly. All this presupposes, however, an implicit interpretation of time prior to the way it is represented in the model. I claim further that denotation, as an element of DDI account of model representation, is not exhausted solely by the trivial function for a model to signify, but is also provided with a *set of implicit assumptions*, sneaking through some general theoretical framework making possible the construction of the very model. This set of implicit assumptions (in our case geometrical and inner qualities of time) is then transferred into the interpretation which secures new knowledge about the target object, after a suitable demonstration.

With the notion of denotation thus specified, can the DDI account be taken as a plausible account of modeling, i.e. of scientific representation? Its second component – demonstration – can hardly raise any problems in its quality of a procedure, directed by formal rules and convenient approximations. What, then, about the third component, the interpretation? Hughes' requirement is that it must be given in terms of the target system. This is a necessary condition, of course, since without it the model would be devoid of its predictive power.

Comparing the DDI account of modeling and the ICSR, one can easily notice that in Suárez's definition interpretation is hidden in clause (ii), requiring for "competent and informed agents to draw specific inferences regarding *B*". This is an exact, though general requirement.

Probably for this reason, *Gabriele Contessa developed ICSR in the direction of specifying different cases of surrogative reasoning*. He distinguishes cases in which a competent user can perform valid surrogative inferences (from the vehicle of representation to the target) from those in which the inferences are not only valid, but also "sound". The latter are cases in which the inferences are true for the target or, in his terms, when the scientific representation is assessed to be a "faithful representation" (Contessa 2007, 53-56).

My intention in referring to his work is connected with the fact that it contains an *explication of the interpretation* of the vehicle in terms of the target, based on a set-theoretical approach. Non-empty sets of objects, relations and functions in the model are identified with relevant objects and properties in the target system (ibid. 57-8). The explication is analytic, but even if it is assessed to be a "classical approach" for reconstructing the notion of interpretation, it is undoubtedly an appropriate suggestion of how results of surrogative inferences could be construed in terms of the target, i.e. a suggestion how to look at the interpretational component of the DDI account.

It is my claim that the DDI account, furnished with the conceptual amendments of its first and third components, denotation and interpretation, may be admitted to be an arguable epistemological model of how theoretical models represent.

4 Descriptional and Constitutive Kinds of Models

Theoretical models used in scientific practice differ in various respects: the way of modeling (linguistic, visual, or both), the richness of the representation, the goal of modeling (for educational, presentational, or research purposes), etc. From an epistemological point of view, I'll differentiate two kinds of models: descriptive and constitutive models.

In his interesting essay *'Who Is a Modeler?'* Michael Weisberg (2007) takes up the task of making a distinction between two kinds of theorizing: *modeling* and *abstract direct representation* (ADR). Without referring to the DDI account he suggests a general definition of modeling, depicting the same three stages – denotation, demonstration, and interpretation, though he does not use the same terminology:

> Modeling, I will argue, is the indirect theoretical investigation of a real world phenomenon using a model. This happens in three stages. In the first stage, a theorist constructs a model. In the second, she analyzes, refines, and further articulates the properties and dynamics of the model. Finally, in the third stage, she assesses the relationship between the model and the world if such an assessment is appropriate. If the model is sufficiently similar to the world, then the analysis of the model is also, indirectly, an analysis of the properties of the real-world phenomenon (Weisberg 2007, 209).

In contrast to DDI account of modeling, presenting three requirements that a theoretical scheme has to satisfy in order to count as a model, Weisberg suggests a dynamical account of modeling, by considering it as comprised of three stages. But the three components of DDI account can be seen not only as aspects, but also as stages in a modeling procedure. Let me recall that R. I. G. Hughes considers denotation, demonstration, and interpretation not only as components of his account of representation, but also as "three activities" providing a representation, or in other words, for building a model (Hughes 1997, S329).

One can easily see that Weisberg's "first stage" corresponds to denotation as the first component of the DDI account of modeling; the "second" – to demonstration, as its second component; and, with a stipulation, "the third stage" coincides with interpretation as its third component. The stipulation concerns the required possibility for the model to be “sufficiently similar to the world”. As we know from the previous section, similarity to “real-world phenomena” is not a necessary requirement for a model to fulfill its cognitive task. It seems that Weisberg is well aware of this, although he still insists on the vague concept of similarity, introducing a refined construal of the model-world relationship, including “fidelity criteria” for a model to represent some real phenomenon (Weisberg 2007, 218-9).

This comparison is a start, not an aim. I was in need of it only to be sure that by ‘theoretical model’ Weisberg does not mean anything (very) different from the notion already considered, covered by the amended DDI account.

As mentioned above, Weisberg draws a distinction between models and ADRs. As an example of a theoretical model he attracts Vito Volterra’s formal representation of a simple biological system composed of two populations, one of sea predators – sharks, and the other of prey – e.g. cod-fish. Volterra’s model is based on two differential equations, describing the rates of change of the predator and the prey populations respectively, and involving parameters of intrinsic grow rate, intrinsic death rate of the predators, as well as of enhancing the number of the latter because of the captured prey. The construction of this model has a curious history: after the end of the First World War a shortage of fish was witnessed in the Adriatic, which had a reflection on the fish markets. The expectation was just the opposite. Because during the war fishing had considerably diminished, so the populations of cod, squid, and lobster were supposed to enlarge. The result, however, was not as expected, and Volterra’s theoretical model succeeded in explaining the strange tendency.

M. Weisberg offers Mendeleev's construction of the periodic system of the chemical elements as an example of ADR. He claims that what Mendeleev has really achieved is a theory. But, in contrast to model representations, this theory is a kind of direct representation of the properties of the chemical elements and thus it is an ADR. "Volterra's achievement was quite different. He engaged in *indirect* representation and analysis of predator-pray phenomena via the construction and analysis of a model" (Weisberg 2007, 216). In this sense *Volterra is a modeler, while Mendeleev is not.*

I accept this distinction of terms. I also accept Weisberg's arguments to defend Mendeleev's discovery of the Periodic Table against the assessment that it is merely a classification device. As he argues, it is a theoretical achievement, because: (1) the Periodic Law is one of the bedrock principles of chemistry, allowing for a systematic description of the features of chemical elements, and (2) important *predictions* have been made on its base – the previously unknown elements Gallium, Scandium, and Germanium. Explanatory and predictive powers are, undoubtedly, essential virtues for every scientific theory. What is interesting here is the resulting distinction that, being a modeler, Volterra may also be called a theorist (since modeling is a theoretical activity), but Mendeleev, being a theorist, is not a modeler.

This distinction is supported by appropriate arguments. If we agree that the ways of theorizing are different, that modeling is always an indirect theoretical representation and ADRs are direct theoretical representations, we must agree that Mendeleev is not a modeler.

What bothers me here is not Weisberg's intention to draw a distinction between scientific representations on the basis of the way in which they represent real-world phenomena. He deploys a nice argument for this. A representation is indirect, when it involves the construction of a model and a model might have different, though not equivalent, descriptions. Thus Volterra's differential equations are not the model itself but its description, despite the fact that some authors do not make this distinction.

What bothers me is *the clarity of the demarcational criterion*. According to Weisberg, in ADR, there is no intermediary stage. It is avoided, because no model is needed to mediate, but real world phenomena are directly represented. However, if the representation is *abstract*, this means that some characteristic features of the phenomenon ought to be outlined as important and others as unimportant, and that a theoretical language must be appealed to, so that a representation is produced, rather than a mere observational picture. Insisting that Volterra's and Mendeleev's styles of theorizing are distinct, Weisberg admits at the same time that

> Mendeleev examined elemental properties, *worked out which properties were essential* and which ones could be abstracted away, and then *constructed* a representational system that elucidated important patterns and structures among the elements (2007, 215, my italics).

Did Volterra not make analogous steps when constructing his model? Before writing down his equations, he reduced the complexity of interpopulation relations by choosing a simple case of only one predator and only one prey population and by abstracting the main parameters of their survival, just as Mendeleev has chosen atomic weights and valences to be essential features of chemical elements. Thus I can say that the difference between the two theoretical activities may refer to the specific character of the preparatory work (idealizations, abstractions, and the like), but bears no qualitative differences, concerning the nature of a theoretical representation *per se*. And if this is the case, then the distinction between a modeler and a (mere) theorist is blurred; since for the results of both theoretical activities one may easily point to their denotation and their inferential aspects, i.e. they both display the main features that a theoretical structure must possess in order to be qualified as a model. In other words, an ADR is characterized by the same three stages that Weisberg himself has outlined (as we have seen in the former quotation) for the presentation of a model.

If Weisberg were right, then one has to refer to the historically well-known models for the structure of the atomic nucleus, like the liquid drop models and the shell models, as ADRs, and *not as models*, as they are admitted to be. Because just like the constructor of the Periodic Table the authors of these theoretical representations worked out which properties of the particles within the nucleus were essential and which ones could be abstracted away, and then constructed a representational system (a vehicle) that predicted important properties of atomic nuclei (the target), like changing amounts of binding energy, special stability, etc. The presence in these theoretical structures of the three clearly established characteristics of theoretical models (the three components of the DDI account) is the good reason why they bear the name 'theoretical model' without problems.

Strange as it might seem, after this criticism of Weisberg, concerning the lack of a sound demarcational criterion, in what follows, *I'll try to defend his methodological flair for a similar demarcation*. My claim, to this effect, is that models could really be distinguished on the basis of the theoretical mode in which they represent their respective targets. The distinction I suggest, however, does not separate models from representations that are *not* models. I assume that every theoretical representation which is *abstract* (and Weisberg willingly uses this adjective), and is directed towards a concrete phenomenon within the domain of a (fundamental) theory, could count as a model. So, the suggested distinction spreads over models, which I classify into two kinds: descriptional and constitutive models.

Descriptive models could bear the name “abstract direct representations”, but the expression is loaded with the connotation imposed by Weisberg. For this reason I make use of the otherwise unsuitable expression “descriptive models”. These are theoretical representations, realized through idealizations and abstract images of observable components and properties of a real system (phenomenon), taken to be essential for its functioning (for the conservation of its status). Descriptive models are often visualized.

A good example for a model of this kind is the so called simple (or mathematical) pendulum. It is an *idealized model of a mechanical system*, consisting of a hanging and swaying rod, ending with a bob. The pendulum of a clock is a sample of such a system. Its elements, as well as its periodical motion, are *directly observed*. But to describe how this motion depends on the elements of the system and on Earth gravitation, one needs to construct a model.

> A simple pendulum is a mass swinging from a massless string attached to a frictionless pivot, subject to a uniform gravitational force, and in an environment with no resistance. This is clearly an ideal object. No real pendulum exactly satisfies any of these conditions (Giere 1999, 122).

The simple pendulum is an idealized model because the essential properties of the real pendulum are taken into account, while the others (like the mass of the string, the pivotal friction, and the air resistance) are abstracted away. An analysis of the visualized model allows us to reach the law for the period of the pendulum: it is directly proportional to the square root of the length of the pendulum and inversely proportional to the square root of the gravitational acceleration. This law is confirmed for real pendulums with small angular displacements or, to use the accepted methodological jargon, the surrogative inference from the model to the target has proved to be faithful.

M. Weisberg’s own example of Volterra’s model is one for the same kind of descriptive models. The Adriatic biological subsystem of fish populations is a real system from which one can choose two no less real shark and cod-fish populations. Concepts such as ‘population’, ‘predator’, and ‘prey’ have clear referents and contents. The essential features of the interactions among the components of such a simplified system can be incorporated in an idealized model, in a similar way to the construction of the idealized model of the pendulum.

Constitutive models differ from the descriptive ones in the mode of their theoretical realization. In contrast to a descriptive model, a constitutive model does not represent a target by abstractions and idealizations only. The model provides a theoretical *constitution* of the target. It is needed in every case in which the target is not an observable phenomenon and the model is not solely a vehicle for achieving valuable knowledge about the regularities followed by the phenomenon. A model of this kind intends to represent entities and interactions which are not observable in the course of scientific experiments, but the system of which explains the arrangement of all observable results in a domain of investigation. Denoting its target, the model at the same time creates it theoretically.

Thus, for instance, the quark model of the hadrons (the strongly interacting elementary particles) can be taken as an example of this kind of models. Neither the quarks as supposed entities, nor their attributed properties, such as fractional electric charges, quantum colors, and way of mutual interaction, are directly observable in quantum experiments. As constituent parts of the model, quarks are taken to be "pure theoretical constructs". Yet physicists say that members of the quark family constitute all hadrons as existing particles, that mesons are made out of two, while baryons are made out of three quarks and that the specific combinations of quarks determine the physical properties of each strongly interacting particle.

The idioplasmatic model by Negeli, now part of late 19th century history of biology, is another example of a constitutive model. Long before genetics came to be widely accepted as a successful theory of hereditary mutability, different explanations had been proposed for this purpose. Negeli's model was one of these attempts. The existence of a hereditary stuff, called 'idioplasma', is a central hypothesis around which the model is organized. It was thought that it is situated not only in the germ cells, but goes through all of the cells in a biological organism. The influence from the external environment could be transmitted, through the idioplasma, from the periphery of an organism to its germ cells, because of the existing links among the idioplasmatic material incorporated into the cells. The mechanism of the model thus explains how some influences and adaptive reactions, important for the life of an organism, could reach its reproductive system, change the idioplasma of the germ cells, and how the changes are consequently transferred to the posterity. Negeli succeeded in accounting for hereditary mutability, by suggesting a model to represent its mechanism.

Because of their high theoretical level, constitutive models are often presented additionally by appropriate visual images. Let us return to the quark model. The confinement hypothesis states that quarks can never be seen in isolation, because the interaction among them becomes stronger when distances among them become greater. A simple image visualizes this strange fact (from the standpoint of the opposite behaviour of classical interactions), *as if* the three quarks, say within a proton, are bound by minute springs, so that every attempt to set them apart causes proportional inverse resistance. Negeli's model is visualized by idioplasmatic rows going through the cells, and composed of small balls in which mycelia are situated. The idioplasmatic rows illustrate the dynamic medium of the model.

5 Conclusions

R.I.G. Hughes' DDI account of modeling was considered in comparison with the inferential conception of scientific representation, offered by M. Suárez, and elaborated by G. Contessa. My claim is that the DDI account could be amended, specifically by further elaboration of its first and third components – denotation and interpretation. The denotation ought to be supplied by a set of implicit assumptions that underlie the construction of the model and are thus transferred to its interpretation. The latter can be explicated as an interpretation in terms of the target system, on the basis of the analytical approach suggested by G. Contessa. Thus the DDI account could considered a plausible epistemological model of how scientific models inferentially represent.

These considerations are brought to bear on M. Weisberg's distinction between theoretical models and abstract direct representations. The resulting criticism considers only the clarity of his demarcational criterion. His methodological flair for such a distinction is maintained and transformed in an attempt at distinguishing between two kinds of theoretical models, descriptive and constitutive models, respectively, rather than between models and scientific representations that are *not* models. These two kinds of models differ from one another in the mode of their constructive realization.

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