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Target Systems, Phenomena and the Problem of Relevance[1]

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Introduction

In his 'Target Directed Modeling', Michael Weisberg offers an account of scientific modeling directed at a specific system. The models under consideration are mathematical models and the system being modeled is real and particular[2].

With remarkable clarity Weisberg distinguishes and analyses three phases in this type of modeling process. The first phase is the construction and interpretation of the model, what Weisberg calls 'the modeler's construals', and the second phase is the analysis of the model's implications. The last one is the specification of the target system, which involves selecting features out of a phenomenal domain, its empirical representation, and the evaluation of the model against this empirical representation[3].

Constructing and analyzing the theoretical model, preparing the system that is the object of the model, and comparing the model and the system, are generally not separate activities. So, as Weisberg himself notes, the distinction between these three phases is not to be taken to reflect a real separation. Still, a better understanding of the different pieces that are involved, which they are, what they are, what role they play, and how they are related, is certainly part of what is needed to understand the process. In this respect, the map, the model of modeling, that Weisberg is laying out is not only insightful but has the additional virtue, typical of models in general, of being a guide for further investigation.

Because I largely agree with what Weisberg says concerning the first two phases and because of my own interest in the experimental component of modeling, it is the third phase of his schema that I wish specifically to discuss: the experimental side of modeling. This is where, if anywhere, modeling gets at the world, and where it gets decided how, if anyhow, the world is given to us. Compared to the effort that has been put into trying to clarify the notion of model, the attention received by the notions of target system and phenomena, how they are identified and empirically characterized or how they relate to one another, varies from scarce to inexistent. This is philosophically surprising and frustrating. Surprising because, after all, target system and phenomenon are the red thread of the modeling process: phenomena are what models are supposed to save or explain, target systems are what models are directed at, and target systems, or

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[2] Weisberg contrasts this type of model with models "used to study non-existent targets (e.g perpetual motion), or generalized targets (e.g. sexual reproduction in general)".

[3] For this account of modeling, see also (Weisberg 2007a).

phenomena, depending on the account, are what the implications of models are supposed to be about. They are what measurement outcomes forming the data-models are supposed to characterize, which data-models, in turn, provide the benchmark for the evaluation of theoretical models. Frustrating because it is not clear at all what unique conception of target system or phenomenon, if any, can play all these roles.

Weisberg's overall picture of scientific modeling has the merit of offering a way to articulate the notions of model, target system and phenomenon and of making these notions more precise. But I had some difficulty to put all the pieces together and found some elements crucially missing. I will explain these difficulties and point at these absences by examining in turn the notions of target system and of phenomenon of interest and then the relation between the two. In the process, I will suggest some clarifications and what I hope to be constructive amendments. As we will see, a major problem on the experimental side of modeling, absent from Weisberg's account, is that of the identification of the relevant empirical factors. Weisberg is not unaware of this problem and we can find in other work of his on modeling strategies (Weisberg 2006, 2007b) some suggestions for how to deal with it. I will discuss these suggestions and illustrate their limitation with a case study presented in the last section of this essay.

I. Target System

Let's suppose that, as Weisberg writes, we are considering a type of modeling activity where "the modeler has a specific target system in mind, and the goal of modeling is to understand and make predictions about that specific system" (p.1) We limit ourselves to cases where the model will be a mathematical structural object endowed with what Weisberg calls an 'assignment'. The assignment provides an interpretation of the elements of the model that coordinates "parts of the model with parts of the real-world phenomenon" (p.5) by specifying what quantity in the world is denoted by each element of the model: "for example, in the Lotka-Volterra model the dimension of the state-space labeled P was assigned to denote the abundance of the predator population". (p.5) The assignment also specifies which parts of the model are relevant and which are to be ignored by specifying the domain of values of the variables and parameters in the model: for instance, "Volterra only assigned rational values for the state variables (and probably only certain ranges of those numbers) to population densities in the Adriatic and other possible populations" (p.8). The assignment is part of what Weisberg labels 'the first phase' of the modeling process, whereas the second one consists in analyzing and deriving implications from the model. As noted above, however, the modeling process is driven by the goal of making predictions about a specific system, what Weisberg calls the 'target system'. For the implications derived from the model to be predictions about a specific system, the assignment must be governed by what this specific system is that the modeler has in mind. But what is it exactly that the modeler is supposed to have in mind? What is it to have a target system in mind? Is the idea of the target system an idea that one can form simply by looking around? Not in Weisberg's model of modeling.

The determination of the target system is, according to Weisberg, the result of an operation that he locates in what he calls 'the third phase' of modeling. Now, it is clear that the phase numbering cannot be any kind of ordering, be it empirical or logical: there cannot be a model assignment without first a determination of the target system, if only a tentative one. More interestingly, the operation that results in the determination of the target system is far from trivial. The determination of the target system is closely related to another operation that takes place in this third phase: the construction of the data-model that will serve as a basis to evaluate the theoretical model. This relation underscores the importance of getting clearer on how the target system is determined.

The data-model, a model of the data, is a representation of empirical data, outcome of measurements, that will be compared to the predictions from the theoretical model obtained in the 'second phase'[4]. The relation between data-model and target system is that the dimensions of the data-space are nothing else than the "properties making up the state of the target system" so that the data-model is "a state space for the target system" (p.14). So what the representation of the data will look like will depend on what these properties of the target system are. But how are they identified as such? On what basis? Take the example of modeling that Weisberg uses in his paper: the construction of a model that would account for some surprising aspects of the aquatic population in the Adriatic sea after World War I. What motivates the modeling process is the evolution of the fish population in the Adriatic sea. But the system formed by the fish population and its environment is not what Weisberg calls the target system. For many features of this real system will not be represented in the data-model, like the temperature or salinity of the water. Some features are selected as the ones that are empirically relevant, relevant for the construction of the data-model, as well as for the interpretation and evaluation of the theoretical model. So again on what basis are they selected as such?

There is no clue on how to answer this question in 'Target Directed Modeling'. But some suggestions elsewhere in Weisberg's papers will be discussed below. To understand both the relevance and limitation of these suggestions however, we need to get clearer on what the question is. For that we need to clarify a bit further the notion of target system and to introduce the notion of 'real-world phenomenon' of which Weisberg also says that it is what is being studied in the modeling process. And finally we will need to clarify the relation between target system and real-world phenomena, both presented as what is targeted by the model. Let's first go back, then, to the target system.

There are several things about target systems in Weisberg's model of modeling that we can take as a starting point. To begin, the target system is, as we saw, a particular system, located in a spatio-temporal region of the phenomenal world. Its relation with the data-model, with trajectories in the data space representing the evolution of the target system, implies that it is what is the object of measurement and is manipulated in the experimental phase of the modeling process. For a contrast, one can look at Eric Winsberg's conception of target system (Winsberg 2009). For Winsberg, the target system of the modeling process is not what is manipulated and object of measurement in the experimental phase of modeling. Rather, according to him, what is manipulated, at least 'in a large class of experiments', is generally regarded as being some object that 'stands in for the target.' (p.583) One consequence of this conception is that it precludes any epistemic privilege that experimentation could have over simulation due to its experimental

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[4] The notion of data-model is generally traced back to (Suppes 1962). For the limitation of the distinction between data acquisition and data manipulation, see (Harris 2004).

access to the target system, which access it does seem to have in Weisberg's conception of target system[5].

But I do not think that Winsberg and Weisberg necessarily have different views on what is object of measurement and manipulation in experimentation. I suspect that they simply do not have the same thing in mind when they speak of a target system. Winsberg would probably take the fish population and its environment to be the target system. By contrast, for Weisberg, the target system is the outcome of a process of abstraction, abstraction from 'a real world phenomenon': "[A]fter individuating a phenomenon, a theorist must make decisions about which parts of a phenomenon she will consider and which ones she will not. [...] The outcome of this process of abstraction is the generation of what I call *target systems*."(p.13)

That the target system is the product of a process of abstraction does not mean that the target system is an abstract object. There is one sense in which it is. Suppose the target system is a spring and the modelers are interested in some dynamical properties of the system. There is nothing wrong or surprising in saying that two series of experiments conducted in different laboratories were using the same target system. One does not mean that the same physical system has been moved from one place to the other. One refers to the type, instantiated in two different places. But the token is a particular, real and spatio-temporally located. It is not an abstract system.

Still, there does seem to be something somewhat abstract with target systems, even in this sense of token systems. It is because materiality cannot be sufficient for individuation. A chemist and a physicist may work on, investigate, and manipulate the same physical system. Would they be using, investigating, the same target system? Probably not. But the difference between the two target systems would not then be material; rather it would be, it seems, a function of how the state of the system that is manipulated is characterized, in terms of what properties, what features.

So two systems physically distinct could be of the same target system type and two different target systems may be physically the same. The reason, in both cases, is that a target system, in Weisberg's sense, is only characterized in terms of a limited number of features.

Another important point about target system is that the data recorded as outcomes of measurement cannot really represent the state of the target system. For the measurement will be influenced by more properties than those represented in the data-space. So what represents the evolution of the target system should rather be what others have called 'a pattern in the data'[6], that is, a relation between the quantities that are represented in the data-space obtained by somehow subtracting the influence on the measurement of any other factors. Remarkably, this pattern is what others have identified with the instantiation of phenomena, real-world phenomena, but not Weisberg, and more will have to be said about that. Whatever we call what is represented in the data-space, the basis for the selection of the quantities to be represented in the data model is crucial. We use the data-model to evaluate the theoretical model and what we want, when we evaluate the theoretical model, is not simply that it fits any old data-model. The difficulty of making correct, proper measurements, measurements that really measure the quantity they are

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[5] According to Winsberg (2009) the difference between simulation and experimentation is a difference in 'how researchers justify their beliefs that the object can stand in for the target.' (p.586). I discussed this view in (Peschard, 2010)

[6] See for instance (Feest 2009)

supposed to measure is well-known. But supposing that all the measurements are done properly, there is still the question of what quantity should be measured.

In the case of hypothesis testing experimentation (Steinle 2002), where the aim is "to test a clearly formulated expected effect", what needs to be measured is clear at the outset: the influence of one specific quantity on another. In such cases, there is no need to generate what Weisberg calls the target system because there is no need to *select* some features as those that should figure in the data-model. This procedure is needed on Weisberg's account because "theorists construct and analyze models in order to study real-world phenomena" (p.11) which occur in the midst of a multitude of factors without clarity as to which of them are relevant to this occurrence. The process of abstraction that produces the target system is instrumental in the study of a real world phenomenon, and as such, then, must indeed be submitted to normative constraints. But what are these constraints?

II. Phenomena

Let's start with the concept of phenomenon, and more particularly, the idea of a 'phenomenon of interest'. In Weisberg's account, the modeler starts by drawing a spatio-temporal region of the world that isolates, circumscribes, some main object, property or process "along with anything exogenous to this object, property, or process that has a causal influence on it." (p.13). The main object is what Weisberg calls the phenomenon of interest and drawing the boundary has the result of "individuating phenomena from the buzzing, blooming confusion of the world *in toto*". It is from this causal spatio-temporal region that the features of the target system are supposed to be selected ("abstracted"). I will limit myself to the case where the object of investigation, and modeling, is a process[7].

In the case of the Lokta-Volterra (LV) example, the main process of interest seems to be the evolution of the fish populations in the Adriatic sea during and after WW I. But, if the phenomenon of interest is what the modelers are studying, how could its individuation depend on the identification of its causal dependencies? How could the modelers confronted with the mystery of the fishing shortage know *where* the causes are if they do not know *what* they are?

What scientists started with is, say, the fishing shortage, and the question: Why?, just as it could be, to take other examples, a change in the global temperature for climate scientists or some pattern in the electrical activity of visual neurons for the neuroscientists. More generally, the starting point is the evolution of some quantity or quantities remarkable enough to be worth an investigation, interesting enough to be worth an explanation, or important enough to be worth a prediction[8]. It is sometimes the effect that motivates the investigation that is referred to as

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[7] One reason is that this is the sort of case that Weisberg uses as main example when he discusses the Lotka-Volterra (LV) model. Another, more serious, is that I am not sure at all that there is no important difference between process modeling and object modeling. Object modeling seems a more complex task that involves creating and modeling processes (like the diffraction of X-rays by the DNA molecule) and some inferences from the results of processes to the structure of the object. If this is so, it seems reasonable to get clearer first on the case of process modeling.

[8] In fact, to express the intriguing effect in terms of a variation in one or several quantities is already a step in the process of modeling. How to measure pattern recognition, climate change, or memory, is not a given, and may even be controversial. The variation of interest needs to be operationally defined (Sullivan 2009).

phenomenon of interest. On this view, the phenomenon of interest is something that needs to be accounted for. The aim of 'accounting for' is reminiscent of that of 'saving', with the same ambiguity between explaining and merely predicting (Hacking 1983: 222). It is true that this evolution is spatio-temporally located. But the location is generally not even well-defined and more importantly, where and when it happens need not be taken *a priori* as being essential to what happens. In fact, to understand *why* it is happening will be precisely, it seems, to clarify, among other things, the role of its spatio-temporal location.

But Weisberg is right that it is not what is generally meant by phenomenon of interest. But he is wrong that modeling starts with the individuation of the phenomenon. What is generally meant by phenomenon of interest is what the model is used to represent. And that is not merely an effect but rather a relation between quantities that accounts for this effect, by having the original variation as one of the quantities represented in the relation[9].

Of course, one may object that if the 'effect' is a variation, it is itself a relation. But generally the variation is a temporal variation. By contrast, the sort of relation represented by the model is causal, at least in the interventionist sense. This is why the model can account for 'the effect' rather than merely describe it: it accounts for the effect as the result of the causal influence of the other quantities represented in the model. Suppose scientists investigate the same effect produced by different factors, say, the effect on memory of two different molecules[10]. Even if the effect is exactly the same, same variation of the same quantity, they will generally not be regarded as studying the same phenomenon. Paul Teller (2010) nicely illustrates the point with the example of diabetes: we make the distinction between two types of diabetes insofar as we make a distinction in the cause of the effect[11]. But when one is engaged in scientific modeling, one does want to make this distinction. And that may be why Weisberg also speaks of the region that includes both the primary object (in a general sense of object) *and* its causes as individuating the phenomenon. How the effect is produced makes a difference with respect to the individuation of the phenomenon.

It is not always clear in the literature whether the phenomenon is conceived of as an effect *as* resulting from certain causal factors, or as a causal relation between quantities. Even though these are different conceptions, as far as modeling practice is concerned, they are equivalent. The reason is that the empirical representation of an effect individuated by its causes will have to be that of the relation between the quantities representing the effect and its causes. The representation will consist of points in a data-space whose dimensions represent the quantities in question. Such relations are the empirical regularities that experimenters try to get evidence for in exploratory experimentation (Steinle 2002). By contrast with theory testing experimentation, the aim is not here to measure the effect of one quantity on another. It is to find out, through the systematic variation of different quantities under controlled conditions, what

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[9] There are two ways in which one could speak of 'what the model represents'. In one sense, it is its representational content. This is something we know about the model if we understand it. In another sense, it is what the model is used to represent, something that happens to a real system. We need more than simply to understand the model to know what the model is used to represent.

[10] This case study is discussed in (Sullivan 2009) from a different but related perspective.

[11] "Diabetes is a phenomenon caused by insufficient insulin uptake. One cause, one phenomenon. But this cause itself has two different causes: Insufficient production of insulin, type 1 diabetes, or resistance to its effects, type 2 diabetes. The one phenomenon, diabetes, can be analyzed into two more specific subphenomena." (p.818)

quantities are responsible for a certain effect in a quantity of interest.

The data in the data-space are the basis for the evaluation of the model but they are not themselves the representation of the phenomenon. That much is actually uncontroversial. The reasons why it is so, however, diverge according to two very different conceptions of phenomena. According to both conceptions, phenomena are causal patterns or effects as individuated by their causes. But on one conception, phenomena are simpler than the relations generally instantiated in the data-space; on the other conception, they are much more complex. On the former conception, phenomena involve only a limited number of factors and may occur in very different conditions. The phenomenon only includes a set of factors common to these different occurrences[12]. It is simpler than the relations that are instantiated in the data-space precisely for this reason: this instantiation include factors that are specific to the conditions of occurrence of the effects and the conditions of measurement. One goes from the data to the phenomenon by subtracting the influence of these extraneous factors.

This is not how Weisberg thinks of phenomena. According to Weisberg, what is mathematically represented in the data space for the sake of comparison with the model is the *state* of the target system and "*the evolution* of the target system as transition in the state space" (italics added). But the phenomenon is something much more complex involving many more factors than those represented in the data space. In fact, it is 'too complex to be described, let alone compared to a model'. In the data-space, only some features of the phenomenon are represented. According to the previous view, what Weisberg calls the evolution of the target system would be nothing other than the instantiation of the phenomenon.

These two conceptions of the relation between target system and phenomenon of interest lead to different conceptions of what models can tell us about the world, which will be discussed in the next section. As we will see, both conceptions suffer from a mis-representation of some critical aspects of the practice of modeling which leads to an over-simplification of the task of determining the relevant dimensions of the data-space.

III. How models relate to the world

As we saw, there is a conception of phenomena on which they will just be what in Weisberg's terms corresponds to the evolution of the target system. But there are good reasons to maintain, as Weisberg does, the distinction between the phenomenon of interest and the evolution of the target system.

In particular, conflating the instantiation of the phenomenon under investigation with the evolution of the target system makes the individuation of phenomena dependent on the selection of features of the target system. The world 'in toto' does not isolate these features. The conditions in which the evolution of the target system is instantiated are constructed and even then, this evolution can only be *inferred* from the data. To identify phenomena with the pattern instantiated by the evolution of the target system inevitably leads to the idea that phenomena themselves are

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[12] This seems to be the sort of conception of phenomena held, for instance, by Bogen & Woodward (1989), Feest (2009), Mc Allister (1997), Glymour (2000). For instance, according to Bogen & Woodward (1989: 317): Instances of phenomena … "can occur in a wide variety of different situations or contexts. This, in turn, is closely connected with the fact that the occurrence of these instances is (or is plausibly thought to be) the result of the interaction of some manageably small number of causal factors, instances of which can themselves occur in a variety of different kinds of situations."

constructed, in fact doubly constructed: first, because the conditions of their instantiations need to be constructed (Hacking 1983) and second, because they can only be inferred from the data and this inference may be underdetermined (McAllister 1997). If we separate clearly phenomenon and target system, the fact that target systems are thus constructed raise an epistemological issue: on what ground can the evolution of the target system be the basis for the evaluation of a model of the phenomena? But by conflating instantiation of phenomenon and evolution of target system, we deprive ourselves, one may argue, of the possibility to refer to the world itself, independent of any construction. We mistake an idealization of the world for the world itself.

The issue of idealization can be clarified by starting with what Weisberg elsewhere calls 'the representational ideal of completeness' (Weisberg 2006). This ideal is to achieve a representation that would include all the factors that causally contribute to the evolution of the quantity of interest and only such a representation would be a representation of the phenomenon. To represent the evolution of the target system in terms of a limited number of factors must be seen as a failure to achieve this ideal.

Weisberg points out that it may not be possible to carry out the experimental measurements that completeness of the representation would require. But this is only a practical difficulty. That doesn't necessarily compromise the ideal. Completeness however comes with a price: the explanatory virtue of the model that one should aim at according to the ideal of completeness. One reason is that analytical solutions will most likely be out of reach. If as Weisberg notes, stability analysis would remain an alternative, it will typically involve approximation techniques that seem to defeat the idea of completeness. In addition, and this is probably the most compelling objection to this ideal, it carries with it the promise of a loss of sense, of meaning of the models. Weisberg (2006) offers several interpretations of this objection originally formulated by Richard Levins (1966). One could concern our cognitive limitations and our inability to grasp the model in its entirety: "Humans are incapable of grasping such complex dynamics and the several hundred dimensional state-space. These models cannot really be thought as wholes, they can only be manipulated on computers and, perhaps, only a little bit at the time" (Weisberg, 2006: 632). To recommend the production of models that do not make sense to us makes the ideal of completeness certainly unattractive. But the problem with this ideal is even worse than that. It is the lack of generality of models that the ideal of completeness recommends. Such models, Weisberg writes, "are tailored to very specific phenomena. They will often not generalize beyond the particular phenomenon" (p. 632). The real problem is so not much that the model achieving the ideal of completeness would be of a particular phenomenon; it is that this phenomenon is supposed to be so complex that it is and can only rarely be instantiated. The problem is that the ideal of completeness, as it is formulated, presupposes that the phenomenon comprehends all the factors that have an effect on the evolution of the variable of interest, and that is something that must be unique. And if phenomena are such that they are uniquely instantiated, it is not just models, it seems, that will not make sense, it is the world itself. The distinction between phenomena and target systems was supposed to save the idea of a world independent of any selection. Paradoxically, this world then turns out to be one that cannot make sense. If that is so, it is difficult to see how the ideal of completeness could be a scientific ideal.

The ideal of completeness is not, however, the only way to make sense of modeling even with the conception of phenomena adopted by Weisberg. Modeling can be seen, instead, as governed by a strategy of idealization (Weisberg 2006, 2007b). On this view, modeling aims at an idealized representation of the world, in terms of a limited number of factors. What is aimed at, then, is the representation of the evolution of a system characterized by only a limited number of features: "We no longer even aim at producing complete models. When we build a model of some phenomenon, we will try to include the most important or the most *relevant* aspects, which depends on the interests of the theorist." (Weisberg 2006:633, ital. added). Could someone who conceived of phenomena as something rather simple agree with this formulation of the aim of models? Probably. But, here is an important difference: she would not have to see modeling as a procedure of idealization since phenomena no less than models only involve a small number of factors.

According to the strategy of idealization phenomena can, in principle, be represented with different degrees of detail depending on how many of the causes are included. It is a virtue of this account that it can make good sense of this possibility. In practice, however, as Weisberg explains, there will be an appropriate level of detail of the representation that will be the result of a trade off between different virtues expected from the model, among which is the generality of the model. Of course, on the previous view, where phenomena are simple, this virtue of models comes for free since phenomena are, so to speak 'naturally', instantiated in very different conditions. Models aim at an accurate representation of phenomena and should be general since phenomena themselves are prolifically instantiated. On the other hand, when models only are and are meant to be idealizations, what phenomena are really like is not what is at stake in modeling. What is at stake is what the best idealization can be depending on the specific purposes of modeling: "In the context of modeling … how we break up a phenomenon into parts, and what features we choose to include and not to include in our model, must be a function of our modeling goals. There will not be a single norm that tells us how we should idealize in every case" (Weisberg 2006: 634)[13].

Following Levins, Weisberg distinguishes three desiderata of generality, precision, and prediction that govern the practice of modeling and whose maximal satisfaction regulates the procedure of idealization. Supposing that one aims to construct a model that accounts for a certain effect, there will be different models satisfying maximally the different desiderata. Different models provide us with different representations but in so far as the effect is accounted for, what is at issue is only *how* to represent phenomena, not what phenomena there are. What is at issue in the selection of the features of the target system, the dimensions of the data model, is only what idealization is deemed most appropriate to the purpose. This conception of a phenomenon as effect individuated by *all* its causes, whose models only provide an idealization, seems to keep the reality of phenomena safe and independent from the epistemic voluntarism that pervades the practice of modeling. That might look like an attractive virtue of this conception.

As we will see in the next section, however, the problem is that modeling is not just a matter of finding the causes of a given effect, because it does not generally start with a well-defined, uncontroversial effect. What effect there is and what causes are needed to account for it depend, at least in part, on one another and both are at issue. This is why there is more at stake in the

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[13] This is a view that Weisberg attributes to Levins without its being completely clear that he endorses it himself.

selection of the features of the target system than how to represent phenomena[14].

IV. Identifying the relevant factors

Suppose that one conceives of modeling as a matter of accounting for a certain effect. Then, to the question of what factors need to be included in the representation, Michael Strevens (2004) offers a compelling answer in terms of the selection of factors that make a difference in the occurrence of the effect in question. These are, Strevens argues, the ones that are relevant to the representation of the phenomenon. Take as an effect the death of Rasputin. Whatever factors could have been different while Rasputin would still have died are, on this account, not relevant to the representation of this phenomenon. Giving that Rasputin died drowning, after "he was tied up and thrown into a freezing river", the fact that he was served some poisoned teacakes beforehand, "which failed to kill him", turns out to be non-relevant to his death. But, slightly change the effect to be accounted for and what becomes relevant may change as well. Had Rasputin not eaten the teacakes, would he have died shaking? Probably not.

The problem with applying Strevens' solution to real cases of modeling is that the investigation of phenomena does not generally start with a well-defined effect, an effect that is uncontroversially what needs to be accounted for. As emphasized by Knuuttila & Loettgers (2011), we need to distinguish between the problem of identification of the causal core of an effect and the problem of the identification of the effect[15]. That they can be distinguished does not mean, however, that they can be dealt with independently of one another, and this is generally not the case. For example, there are different models in cognitive psychology of the phenomenon of attention and they are not different simply in how they account for a given effect. Rather, they are based on different conceptions of what constitutes the manifestation of the phenomenon (Watzl *forthcoming*). Reciprocally, what factors are responsible for some aspects of the effect may influence the judgment about the significance of this aspect. If the signal of the neuronal activity associated with a specific cognitive task is influenced by the emotional background of the subject, should this influence be part of the characterization of the *cognitive* activity or should it eliminated through averaging techniques? (Lutz et al. 2002)

Suppose that there is evidence for a factor that has an effect on the variable of interest, an *active* factor. What the evidence does not say in and by itself is what should be done with this factor and its effect, whether it is a relevant quantity that should be represented in the data-model or one whose effect should be neutralized.

There are clear-cut cases, when there are some theoretical premises or specific interests that determine what is relevant or irrelevant. But there are also cases that are not so clear-cut and where judgments of relevance can become controversial. What is at issue in these controversies is both what features should figure in the representation of the phenomenon and what effect needs to be accounted for. I will give two examples of modeling that illustrate this type of issue, the issue of relevance.

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[14] I am gratefully indebted to Joseph Rouse (2002) for the distinction between what is at issue and what is at stake, with no certainty, however, that I am doing full justice to his own intended meaning.

[15] The authors formulate the distinction in term of causal isolation robustness analysis vs. independent determination robustness analysis.

In the context of the investigation of animal exploratory behavior, some recent experimental studies (Chemero & Heyser 2009) criticize previous studies for making no mention in their experimental report of why or how the particular objects were selected[16]. The description of the experimental procedure, they complain, is typically limited to 'they are made of…', 'they cannot be displaced', whereas, these critics argue, what these objects afford to the animal in terms of particular behavior, for instance whether it can or cannot be climbed onto, has an effect on the behavior that should be taken into account. I read these critics as saying that there is an active factor that has been overlooked, the affordance factor, and that the lack of information about the values taken by this factor makes the experimental results non-reproducible and non-reliable. To ensure reproducibility, all that would be needed is to neutralize the differential effect of the factor in question, say, by fixing or randomizing its value through an appropriate selection of the objects used for the experiments. This strategy would not be sufficient, however, to ensure the reliability of the data. For the critics claim not only that the affordance-factor is active but that its effect is part of the phenomenon under study; it is not only an active but also a relevant factor. In other words, they claim that the data model should include this factor as one of its dimensions, and that it is one of the characteristic features of the target system. Are they right? [17]

Many factors that may have an effect, like the age of the animal, were neutralized because not regarded as relevant. On what basis should a given factor be regarded as relevant? In certain cases, the variation of interest itself, if it is well defined, or the theoretical background, if it is sufficiently mature, will dictate judgments of relevance. Neither of these two conditions are met here. The effect to be accounted for is not well defined and it is studied from different theoretical perspectives without a unified theoretical background. To understand what motivates and supports the judgment of relevance, we need to step back and enlarge the perspective on the phenomenon. What is at stake in taking affordances as relevant factor is, at least in part, I suspect, an offshoot of what is at stake in developing an ecological rather than representational cognitive psychology (Chemero & Silberstein 2008), and an embodied rather than symbolico-computationalist cognitive science.

I will now offer an example that illustrates a different way in which the issue of relevance can arise. This is a situation where the effect of factors that came to be deemed relevant was not even recorded by most empirical studies. The example is provided by the study of the receptive field of visual cells. Here also the determination of the effect of interest has been transformed by the on-going systematic exploration of the effects of these 'new' factors. Experimental procedures studying the response of the visual cells used to be conducted in an artificial environment with isolated luminous stimuli (Hubel & Wiesel 1962). The behavior of neurons was then regarded as the expression of something like a property, an invariant characteristic of the neurons. Research over the past decades has shown that instead

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[16] The typical procedure consists in recording the behavior of animals presented with a set of objects in a bounded open field (see, for instance, Wilkinson et al. 2006). From one trial to the next, the position of some object is modified or some objects are replaced with new ones to investigate the effect of these changes on the behavior of the animal, in terms of time spent near the object.

it depends on the environmental context (Albright & Stoner 2002, Bonds 1989), it has a temporal, non-linear dynamics (Albrecht 2002), and it is influenced by top-down and lateral neural connections (Albright et al. 2003, Hirsch et al. 1998). As a result of the new experimental findings, what started as the phenomenon of the activation of individual neurons in response to the presence of a specific stimulus is now rather seen as only one manifestation, in restricted conditions, of something more complex, involving a myriad of factors. The conception of what had to be modeled has evolved from an intrinsic filtering property of neuronal cells characterized by their response to a specific region of the visual field, to something more like a dynamical, non-linear, integrated process involving networks of cells (Albrecht et al. 2003, Ringach 2004).

But what makes this on-going research especially interesting here is not that the phenomenon of interest was redefined in the course of the investigation, it is rather that it has actually not yet been redefined. What has become clear is that the 'classical' conception of the receptive field does not accommodate more recent findings; what is not clear is what the new conception should be, "how revisionary or conservative neuroscientists could now be about the concept RF [receptive field] in the light of these findings" (Chirimuuta and Gold 2009). Like in the previous case, the answer will not be based strictly on what happens in the investigation of the receptive field. What is at stake in taking such or such an active factor as relevant to the conception of this phenomenon is a much broader enterprise. Chirimuuta & Gold (2009) consider, for instance, all of the following: the possibility of electing natural stimuli as canonical type, and weigh it against the cost of having to dismiss the huge amount of work made in artificial conditions; the possibility to accommodate the influence of neuronal connections by modeling the visual system at the level of the circuitry, and the cost of a "dramatic rethinking of neural computation"; the possibility to eliminate the concept of receptive field altogether with the risk of disrupting the coupling between neurophysiology and computational modeling; or finally to interpret these findings in the light of results about the role, in perception, of the interaction between organism and environment, which are themselves not unanimously regarded as compelling. What seems quite clear, in any case, is that there is more involved in establishing the relevance of a factor, and the subsequent need to re-conceive the phenomenon, than the technical difficulties of establishing that this factor is active.

It has been recently emphasized that not only are models not derived from theories, but the construction of models involves a variety of tools and considerations that are of diverse nature and diverse origin (e.g Boumans 1999). By contrast, data-models are still construed as obtained directly from the world so long as measurements and data analyses are properly and carefully conducted. This is the result of overlooking the difference between active and relevant factors. If judgments of relevance are involved in determining which data-models should serve as touchstone in modeling, these data-models can no more be the direct result of measurement than theoretical models can be the direct result of derivation.

V. Conclusion

Scientific modeling is one way we have to make the world intelligible, by identifying regularities that underlie the diversity and complexity of what happens. These regularities are described by mathematical models in terms of relations between quantities. Modeling amounts, as Weisberg's puts it, to a "partitioning of the universe into phenomena".

I have distinguished two conceptions of phenomena. On one conception, phenomena only involve a limited number of factors. Their instantiations take the form of patterns that can be abstracted from the data resulting from measurement. The abstraction aims to retrieve the simplicity of the phenomenon by eliminating the effect of extra factors. On the other conception, phenomena are much more complicated than what is happening in the data. The data are outcomes of measurement of only a small number of factors. Phenomena involve many more than are measured. The measurements are only in the service of an idealization of the phenomenon.

What is common to these two conceptions is that the investigation of phenomena is an investigation of the causes of a given effect in a quantity of interest. Modeling, the story goes, aims at models that will account for this effect, as a result of the combined influence of some other quantities. Data-models, touchstone of the evaluation of models, form a bridge between models and phenomena. But data-models can themselves go wrong. Notoriously, they may misrepresent the effect of a given factor on the quantity of interest. Modelers and experimenters have methods, interventionist and statistical, that they use to lower the risk of such mistakes. But these are not the only ways in which data-models can be undermined. Much less notorious is the problem of identification of the relevant causal factors, or briefly, the problem of relevance. Being a relevant factor is not simply having an influence on the quantity of interest. That is only being what I have called ‘an active factor’. Being a relevant factor is having an influence that is relevant to the representation of the phenomenon under study.

The problem of relevance translates into a problem of selection of the dimensions of the data-space in which the phenomenon is empirically represented. In Weisberg’s model of modeling, it amounts to selecting the features of the target system that mediate between the real world and the theoretical model. How are these features selected? As we saw, it has been suggested that the solution to the problem of relevance was to identify the factors that make a difference in the occurrence of a given effect. That might work if modeling phenomena were indeed an investigation of the causes of a given effect. But there are cases where the point of modeling phenomena is not simply to account for a given, well-defined effect because there is no such thing to start with. It is to clarify and specify the effect as much as it is to identify its causes and determine their form of influence. And the determination of the effect and the identification of some causal factors as relevant are dependent on one another. The problem with the problem of relevance, it was suggested, is that it seems to involve much more than the study of a phenomenon of interest. There may be no recipe to solve it. But, after all, it was already said of modeling that it is like baking a cake without recipe. It does not mean that it is not in need of clarification.

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