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What is in a Model?
Combining Theoretical and Material Models to Develop Intelligible Theories

by

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1. ***Introduction: What's in One Model?***

Both biologists and philosophers have widely acknowledged models to be crucial epistemological components of scientific reasoning and experimentation. This volume represents yet another instance of such recognition, while at the same time illustrating an important characteristic of modelling practices: their diversity. Modelling strategies in biology are extremely varied in both the form that they take (e.g. diagrammatic, computational, robotic models) and the way in which they bridge between theories and data. This chapter contends that such variation is not only irreducible to an all-encompassing, unique account of what a model is or should be, but that it is actually necessary to the pursuit of the several epistemic goals of interest to practicing biologists. This argument is intended as a contribution to a pluralistic account of modelling strategies, the development of which complements what I call 'single model approaches' to the subject. As an instance of the fruitfulness of a multi-model approach, I consider the differences between what I call *theoretical* and *material* models and I argue that the complementary use of both types of models is crucial to achieving *intelligible* theories about biological phenomena.

As carefully spelled out by Shank and Koehnle (this volume), research in biology involves the manipulation of several types of models, each of which plays a different role towards the achievement of scientific understanding. How to assess the epistemological significance of such diversity? Perhaps unsurprisingly, many of the several philosophers hitherto struggling with this question have privileged a focus on generality over attention to diversity. Especially between the 1960s and the 1990s, a good part of the flourishing philosophical debate on scientific models concentrated on establishing which features, if any, are *common to* all models. This choice is understandable, given the need for at least a minimal definition of model that would allow to distinguish it from other elements involved in scientific experimentation and reasoning (and thus clarify its distinctive epistemological role). And indeed, it generated a vast body of literature fruitfully analysing the modalities by which models abstract, represent, illustrate and/or explain the aspects of reality and/or the scientific theories that they embody. However, this focus also took attention away from the glaring differences among models and among the types of theoretical knowledge produced and epistemic goals achieved by their use. In much the same way, the importance, to practicing scientists, of using several types of models in combination did not receive much attention. The attempt to analyse the diversity among models turned into an effort to explain it away - either by arguing that only certain kinds of models should be characterised as such[i] or by elaborating a unifying description of the processes of abstraction, representation and explanation characterising modelling activities.[ii]

I shall refer to the tendency to explain away, rather than value and analyse, the diversity among models, as the 'single model approach'. Within the first section of this paper, I spell out some reasons why this approach provides a necessary but insufficient basis for analysing the epistemic utility of modelling practices within any given research context. I then argue for the development of a pluralistic account of modelling, which focuses not so much on a general definition of what models are but rather on an analysis of their epistemological functions. Such analysis draws explicit connections between the different features of models and their role in securing one or more of the several epistemic goals of potential interest to practicing scientists. In order to investigate what's in *one* model, we need to recognise and explore how and why biologists choose specific *combinations of* them in order to pursue a desired research outcome.[iii] As an illustration of this, the bulk of the paper applies this pluralistic approach to the study of one prominent epistemic goal, i.e. the achievement of intelligible research outcomes. Specifically, I argue that an *intelligible theory* about a biological phenomenon can only be developed via the combined use of what I call 'theoretical' and 'material' models. After exploring the epistemological differences between these two broad classes of models, I claim that they are irreducible to a single account of modelling practice. While theoretical models are powerful tools for increasing the explanatory power (and predictive value, when relevant) of a given theory, material models guarantee that the terms and symbols used within the theory secure and retain empirical content. Which is all very well, I conclude, since the need to combine these epistemological features accounts, at least in part, for biologists' need to adopt multi-modelling approaches to the investigation of phenomena.

2. ***Toward a Pluralistic Approach: Refocusing on the Usefulness of Modelling***

Empirical evidence overwhelmingly indicates that modelling strategies within biology are hugely diverse and, at least in practice, irreducible to one another. The contributions to this very volume represent a good instance of this. We find here discussions of *mathematical models*, such as Rasskin-Gutman's genetic algorithms used to predict ciliary movements; *geometrical maps*, such as the one used by McGhee to study theoretical morphospace; two-, three- or four-dimensional *computer simulations*, such as Bajaj's virus maps and Ascoli's study of the morphology of dendrites; *robotic models* such as Dautenhahn's human-sized robots and Shanks' robotic rat pups; and *model organisms,* both simulated and real, such as Palsson's slime molds. Each of these types of model is constructed – or even ' discovered', as is arguably the case with model organisms – in different ways and for different purposes. Further, none of those types of models is ever used on its own: all contributions to this volume underscore the need for a combination of different types of models in order to answer specific theoretical questions.[iv] Consider Niklas' simulations of the adaptive walks characterising the morphological evolution of ancient plants. The aim of his study is to reconstruct the evolutionary history of plant biomechanics as a demonstration of their increasing efficiency in facilitating the survival, growth and reproduction of plants. To this purpose, he needs to integrate a variety of data-sets generated by reference to data from the fossil record, by empirical observation of the physiology and morphology of currently existing plants, and by mathematical computation of geometrically possible morphologies and physiological constraints (which provide further clues about the physiology of ancient plants). Each of those data-sets is organised, visualised, interpreted and eventually integrated via different modelling strategies, including geometrical maps, propositional and symbolic descriptions, diagrams, equations and three-dimensional simulations. A major philosophical question raised by this case as from so many others, therefore, is the following: why such need for multiple representations and, consequently, for integration?

As first step towards answering this question, I shall consider the views recently proposed by Morgan, Morrison and their associates in their 1999 edited volume 'Models as Mediators'. Their analysis is congenial to my present concerns insofar as it focuses closely on the scientific practice of modelling and it does take interest in the overwhelming evidence for the diverse modelling practices used within any one research context. Making sense of the multiplicity of models and their uses is actually a major goal of the mediators view, which is one of the reason for the broad definition given of models themselves:

anything used by practising scientists to mediate between theory and phenomena can be called a model. The notion of mediation is used to suggest that a model serves 'both as a means to and as a source of knowledge' (1999:35): the model functions as *representative of* one or more phenomena as well as *representative for* a given theory taken to apply to such phenomena (see also Morgan 2003: 230). In short, models constitute the meeting point between knowledge and reality, thus providing 'the kind of information that allows us to intervene in the world' (1999:23). By representation, Morgan and Morrison do not necessarily denote some kind of mirroring relation or structural (isomorphic) similarity between what is represented and the representation itself.[v] Rather, they imply 'a kind of rendering – a partial representation that either abstracts from, or translates into another form, the real nature of the system or theory, or one that is capable of embodying only a portion of a system' (1999:27).

This account is very permissive, encompassing a potentially enormous diversity of types of representations (including all the ones listed above). Indeed, I see this breadth of scope as one of its main strengths.[vi] While useful in framing the analysis of modelling practices, however, the mediators approach does not venture into a systematic study of variability among models. This is arguably because it overlooks a crucial feature of the research context in which models are employed: that is the type of research outcome, or epistemic goal, envisaged by the investigators. Scientists make specific choices about what they wish to achieve within any given research project. Such choices inform the whole research process, including the selection and use of theoretical assumptions, descriptions of phenomena and modelling strategies. Further, extended philosophical reflection on this matter has made clear that (i) there are several possible outcomes to be obtained via scientific research and (ii) that some of those outcomes are incompatible with each other.

Point (i) is exemplified by the suggestion that both theoretical and non-theoretical types of knowledge constitute typical targets for biological research. Investigation often aims at improving control over phenomena by developing new ways of intervening in the world.[vii] The goal of this type of knowledge can be referred to as 'knowing how' (Polanyi, 1958:56). In this context, the relevant research outcomes are, for example, the development of procedures, protocols and instrumentation allow scientists to modify the entities and processes of interest (where the usefulness of specific modifications might largely depend on the agenda of funding agencies interested in the applications of such knowledge, as is often the case for example with contemporary research on functional genomics).[viii] On the other hand, a lot of biological knowledge aims at 'knowing what', that is, at acquiring knowledge of (what we regard as) facts, processes, explanations, concepts – in other words, knowledge about nature itself. This is what I would call theoretical knowledge, a broad characterisation of 'theory' that encompasses a wide variety of expressions. We find biologists pursuing theoretical knowledge in the form of, among others,

- propositionally or diagrammatically expressed *explanations* providing mechanistic, structural, functional, causal, phylogenetic and/or narrative information (as illustrated by Machamer et al, 2000 and Salmon, 1998);

- ensembles of propositions that are *intelligible* to a given audience (this idea shall be expanded upon in the second part of this paper);

- law-like generalisations (where 'law-like' includes accounts as varied as Dretske's 1977 universally valid generalisations and Cartwright's 1999 nomological machines);

- mathematical formulas;
- networks of concepts organised into some type of part-wholes relationships (as instantiated in gene ontologies[ix]);
- collections of models and their robust consequences, as coordinated by a theoretical perspective (as advocated by Levins, 1968 and Griesemer, 2000).

These different ways to conceive of and express theoretical knowledge are all alive within scientific research, some of them more popular in specific disciplines than others.[x] In fact, especially within the biological sciences, scientists are not necessarily divided as to which of these types of theoretical knowledge is the most informative or correct. More often than not, it is acknowledged that these types of knowledge, as well as knowledge aimed at intervention, can all have a *relative significance* towards the understanding of a given phenomenon (Beatty, 1997:S433). Most importantly for my purposes, each of these forms of theory constitutes a different type of research outcome and might thus require, in order to be achieved, a different set of modelling practices.[xi] Hence the necessity of a framework in which the study of the diversity and integration of modelling practices is combined with a focus on the research outcome that each model system is supposed to yield.

Now, what about pluralism? Nothing in my considerations so far implies that we should not strive for a universally applicable account of how biologists model phenomena. The crucial step away from single model approaches follows from an elaboration of observation (ii) above, according to which the knowledge contained within these various theoretical outcomes is largely non-overlapping. Each way of theorising implies a different perspective, a different way of carving nature at its joints. An obligatory passage point in dealing with this point remains Richard Levins' work on the multiplicity of epistemic virtues that any one model can possess. Levins characterises such virtues as features that a model is expected to maximise in order to accomplish its mediating function in the most successful way. These features range from the requirement of generality (denoting the range of phenomena to which the model can be applied) to the ideas of tractability (the ease with which it can be computationally or experimentally manipulated) and realism (the degree of empirical accuracy with which it represents relevant elements of the phenomenon under investigation). Levins' important observation is that no single model can posses all virtues at once. Emphasis on one of them is only possible at the expense of some other: for instance, all concessions to the realism of a model unavoidably detract from its tractability or generality. Trade-offs among the epistemological merits of models are therefore unavoidable: scientists need to employ a 'multi-modelling approach' involving the integration of several different models, each of which maximises at least some of those dimensions.[xii] This allows biologists to optimise their overall modelling strategy at any given moment by constantly shifting and calibrating priorities from one set of requirements to another, depending on the changing demands of their research context (in turn dictated, at least in part, by the pursuit of the above-mentioned research outcomes). As Levins concludes: 'it is desirable, of course, to work with manageable models which maximise generality, realism and precision toward the overlapping but not identical goals of understanding, predicting and modifying nature. But this cannot be done' (1984:19).

Coming back to the present discussion, this implies that the types of theoretical outcomes listed above differ, among other things, in the epistemic requirements they impose upon research practices. A cursory second-look through the list confirms this intuition. While the development of a mechanical explanation of a particular process might sacrifice generality to realism (to use Levins' own selection of criteria), the pursuit of a mathematical formalisation of the same process, or cluster of processes, will in most cases privilege generality over realism. Similarly, attempts to build models for interdisciplinary research will favour tractability over realism and precision. I thus conclude that the initial choice of an epistemic goal, as well as the heuristic role that such choice plays throughout the process of investigation, crucially influence both the way in which models are manipulated and the assumptions about what models are supposed to represent. Given the multiplicity of epistemic goals of potential interest to scientists, a clear analytic distinction between the notions of model, theory and phenomenon can only emerge from an assessment of the modalities and extent to which the use of models facilitates the achievement of a specific type of outcome.[xiii] Such assessment provides insights into the epistemological utility of models. I also think that a renewed focus on the utility of models can also help us make sense of their variability, as well as of the need manifested by practicing scientists to use more than one model in order to tackle a specific query.

In her analysis of the multiplicity of modelling strategies characterising the history of experimental biology, Evelyn Fox Keller points to similar conclusions:

> 'To be sure, a model is expected to bear some resemblance to that which is being modeled, but in science as in art, the degree of resemblance is generally understood to be a matter of perspective. The more critical question is whether it is a 'good' model, and in both science and art the measure of how good a model is varies notoriously. If it is possible at all to make any generalisation, one might say that it is here, in the criteria brought to the measure of 'goodness', that models in science and art most clearly depart. For the value of a scientific model is judged, first and foremost, by its *utility*. [..] There is a lot of hedging here, but - once one recognises the enormous variability in the meaning of scientifically productive - *necessarily so*' (2001:47; my emphasis)

My intention here is to pursue this suggestion in a way that Keller does not explicitly propose, that is by articulating a pluralistic framework for reflecting upon the epistemological role of models in science. Such a framework could complements single model approaches in two ways:

(a) by taking explicitly into account the relation between modelling practice and its goals within its research context; and

(b) by building a taxonomy of types of models based on their differing ability to facilitate the achievement of one or more epistemic goals.

Recommendation (b) is particularly relevant, given that philosophers have hitherto paid very little heed to it. The development of utility-based taxonomies of models would provide us with a general analytic framework for the study of models, without encountering the problems afflicting single model approaches. Moreover, it would throw further light not only on the diversity among models, but also on the reasons for scientists to use more than one types of model within any one research setting. Once we acknowledge that we can build a typology of models based on their differing epistemic function (rather than on their structural properties and/or the properties of the theory/phenomenon that they represent), it becomes easier to see how the choice of a specific combination of models reinforces the investigators' commitment to a specific research outcome (as emphasised by Griesemer[xiv]) and allows them to pursue more than one epistemic goal within the same research context (as exemplified by Levins).

An in-depth study of how various combinations of modelling dimensions might facilitate or prevent the development of different theoretical outcomes is far beyond the scope of this chapter. What I tried to point out within this section is the feasibility – indeed, the necessity - of such study as part of a pluralistic account of scientific modelling. In the next section, I provide a sample of such approach that focuses on a specific (and arguably popular) epistemic goal, that is, the strife to develop *intelligible* theories about biological phenomena. I shall examine a way to distinguish among types of models that is indeed based on their relevance towards securing this goal.

3. ***Modelling to Develop Intelligible Theories***

3.i Intelligible Theories as Epistemic Goals

Let me start this second part of my discussion by qualifying what I mean by intelligible theories and how I think that they can be developed. I rely on the general framework provided by Henk de Regt[xv], which broadly defines intelligibility as the epistemic feature of theories that guarantees their relevance towards a scientific understanding of the world. De Regt emphasises the strong heuristic role played by understanding in science: the vast majority of scientists strives to increase our scientific understanding of natural phenomena. Next, de Regt stipulates that, in order to obtain such understanding, scientists need to develop intelligible theories. This position is summarised by his Criterion for the Understanding of Phenomena [CUP]: 'A *phenomenon P is understood iff a theory T of P exists that is* *<u>intelligible</u>* *(and meets the usual logical and empirical requirements)* (de Regt and Dieks, 2005). Within this framework, de Regt elaborates what I regard as his most significant claim. He argues that intelligibility is a context-dependent feature: it depends on the content and applicability of the theory to which it applies (that is, on its epistemic virtues) *as much as* it depends on the skills, background and commitments of the scientist employing that theory. Hence, not all scientific theories are intelligible to everyone at every given time: researchers need to acquire specific skills and background knowledge in order to use a theory towards gaining understanding of a given phenomenon. From a philosophical perspective, this implies that an analysis of scientific understanding needs to focus on the context in which scientific theories are produced and applied: information about *who* understands, *what* is understood and *how* becomes crucial to the assessment of the epistemic value of a theory.[xvi]

My present discussion upholds the idea that developing an understanding of phenomena is of major interest to practicing scientists, as well as the intuition that both the epistemic virtues of theories and the specific skills of the scientists are relevant to its investigation. Indeed, within this section I abide by the CUP by focusing strictly on what I called theoretical knowledge, or knowledge aimed at 'knowing what'. Before further elaboration, however, I should note two important differences between my intuitions and de Regt's account of the role of theory. The first is a matter of definition: the scope of my notion of theoretical knowledge is potentially much broader than what the definition by de Regt and Dieks allows for, or so it may seem since they do not provide a clear specification of what they mean by 'usual logical and empirical requirements'. The second difference concerns the status of theory as a necessary requirement for understanding. My remarks on the importance of 'knowing how' in biology make this position very difficult to maintain. Theories are certainly an important component in the acquisition of understanding, but they need not be a necessary one. Knowledge about how to modify and control specific aspects of scientific phenomena also adds to the scientific understanding of those phenomena. My present concern with intelligible theories as an important epistemic goal in biology therefore represents just a subset of the many ways in which scientists understand the world.

Given these qualifications, let us now zoom in on 'knowing what' claims and examine more closely what it means for a biological theory to be intelligible. I want to propose that, in order to provide understanding about a phenomenon, a theory needs to satisfy two main requirements.[xvii] First of all, it must possess empirical content. This means that it should be possible to trace an explicit relation between at least some terms within the theory and some aspects of the phenomenon to which it applies. The second requirement for an intelligible theory is that it should have explanatory power. The theory should trace a narrative that clarifies why focus on its terms accounts for a specific feature of the phenomenon – or, as it is often the case in biology, for its very existence. This narrative can take the form of any available type of explanation, conceptualisation or perspective: what matters for it to be explanatory is that it specifies how the relationships among its components account for the behaviour, structure or existence of the entities or processes to which the theory applies.

3.ii Conceptual and Material Manipulation

So how can a theory that has both empirical content and explanatory power be obtained? An obvious answer in the light of Morgan and Morrison's account is that, like all theories, such a theory is developed by using models. The focus of this section will therefore be on the kinds of modelling practices that can help securing a potentially intelligible theory. For a start, let us turn again briefly to Morgan and Morrison in order to examine another relevant component of their view. This is the recognition that modelling is, first and foremost, a human activity. This activity is directed at the construction and constant modification of objects that can mediate between theories and phenomena. These objects can be actual entities, as in the case of mechanical or scale models, or mental representations, as in the case of mathematical formulas, diagrams or even thought experiments (where diagrams and equations can also, and often do, take the form of actual objects whenever they are reproduced on paper, computer screens or blackboards). In both of these cases, using models to learn something about what they represent always implies their manipulation: 'models are not passive instruments, they must be put to work, used or manipulated' (Morgan and Morrison, 1999:32; see case studies in the same volume for an illustration of this point). In my view, the notion of manipulation includes all conceivable ways of 'putting [the model] to work' (ibid, 33). I interpret the claim that models are not 'passive instruments' as indicating that both the building of a model and its use within a research context involve constant attempts to modify some of its features. In short, all manipulation involves modification – and such endless bickering and transforming of the model exemplifies the scientists' active engagement with it.

Remarkably, this engagement requires expertise, encompassing adequate training as well as the experience acquired via iteratively handling the appropriate instruments, procedures and material. In other words, the successful manipulation of models requires both background knowledge and skills (as implied by de Regt's analysis). Taking this into account, I shall now distinguish between two ways of manipulating models, each of which plays a different, yet indispensable role in fulfilling of the above-mentioned conditions for the intelligibility of theories. A hint to this view was already presented in the above remarks on manipulation, when I suggested a difference between manipulating a model via physical interaction and manipulating it conceptually. Indeed, I see the degree to which a model can be manipulated materially – that is, by constant appeal to information provided by sense-perception (including touch, smell, sight, hearing and sometimes even taste), rather than reasoning – as a prominent source for variation among the very features of models, as well as their epistemological utility. In particular, I want to argue that subjecting models to extensive material manipulation is usually done at the expense of conceptual manipulation. As I explain below, this is not because the two types of manipulation are incompatible. Rather, it is because models that are manipulated materially seem to have different epistemological functions with respect to models manipulated mostly conceptually – so that a shift, for instance, from material to conceptual manipulation implies a shift of epistemological focus in the use of a model. Elaboration of this claim will bring me to assert that models subjected to extensive material manipulation are especially conducive to the fulfilment of the first condition for the intelligibility of theories, i.e. to securing the empirical content of the theory developed via the model. By contrast, models developed via largely conceptual manipulation help to satisfy the second condition, i.e. to enhance the explanatory power of the theory.

Before turning to a comparative analysis of these two types of manipulation and their effects on the epistemological usefulness of models, I should clarify that I take the distinction between material and conceptual to be descriptive of actual modelling practices in a very broad and limited sense. It seems intuitively correct to attempt an analytic distinction between material and conceptual manipulation. For instance, we can easily point to cases in which the material manipulation of a model is almost absent, yet that model conveys a lot of knowledge and can serve a tool for discovery. Consider the process of thinking about a mathematical equation. Most often, this type of model is manipulated conceptually as a mental representation, in which case no physical movement on the side of the investigators is necessary in order to use and modify it as required. This case is very different from the one of a scale model, such as models of ships used in hydraulic engineering or models of riverbeds used by physical geographers, ecologists and geologists, whose adequate use requires skilful physical interaction with three-dimensional objects. Yet, it would be wrong to think that equations are not manipulated materially, too, or that use of scale models does not involve conceptual manipulation alongside the material. More often than not, manipulating an equation involves scribbling it down and deleting or adding terms to it, and in some cases these physical actions might yield precious epistemological insight (for instance, when playing around with symbols on a piece of paper leads unexpectedly to an illuminating new formula). Equally often, the material manipulation of a scale model is guided by the conceptual manipulation of some of its features. Given that the mixing of conceptual and material manipulation unavoidably characterises the handling of a model, my distinction between the two types of manipulation should be read more as an analytic tool than a descriptive statement. What I want to stress are two basic intuitions: that there are differences in the degree to which a model is materially manipulated; and that there are cases in which such material manipulation is largely predominant over conceptual manipulation, as there are cases to which the inverse applies. The distinction is not meant to be rigorous, but rather to provide ground for a comparison between the epistemological role played by 'theoretical models' (largely manipulated conceptually) and 'material models' (largely manipulated materially) toward the development of intelligible theories.

3.iii Theoretical models

Let us examine more closely the cases in which model manipulation happens largely conceptually. In those cases, the main goals for the manipulation of the model are the testing, elaboration or illustration of a given theory about the phenomenon that is modelled. The goal of manipulation is, in other words, to uncover ways in which a model can be *representative for* a given theory. The choice of the parameters used within the model is thus characteristically informed by a well-defined hypothesis about the theoretical outcome that the model is supposed to illustrate, test and/or elaborate. This is because we start from a theoretically informed 'prepared description' of the phenomena under scrutiny. The term 'prepared description' was introduced by Cartwright, who defines it as 'presenting the phenomenon in a way that will bring it to the theory' (1983:133). Importantly, she also argues that 'the check on correctness at this stage is not how well the facts known outside the theory are represented in the theory, but only how successful the ultimate mathematical treatment will be' (ibid.). Thus, the properties of the phenomena that are abstracted in order to be represented within the model, are properties that are either more causally relevant or more general (less context-dependent) than others: both generality and causal relevance are assessed in the light of a given theory.

I refer to models that are thus conceptually constructed as 'theoretical models', in order to indicate how strongly their use is related to the theory that they represent. The use of theoretical models is increasingly widespread among biologists. Take mathematical models, whose crucial role in the establishment of the 'Modern Synthesis' in the 1920s and 30s (Mayr and Provine, 1980), resulting in the birth of a whole discipline relying on statistical methods of analysis (i.e. population genetics), was only a prelude for their growing application across almost all biological disciplines. Even more evident is the pervasive use of simulations and algorithms to visualise empirical data, not to mention the push toward formalisation and away from the laboratory brought about by the increasing use of bioinformatics to store, organise and integrate data. These models are especially useful for elaborating explanations or confirming predictions stemming from given hypotheses (they are what Cartwright calls *interpretative* models in her 1999:181). They are also fundamental to the integration of biological knowledge concerning specific phenomena (as, for instance, bringing together insights from physiology, molecular biology, functional genomics and cell biology in order to understand root development in plants). However, precisely because of their strict reliance on theory, theoretical models are not the best of epistemic tools when the goal of their manipulation is to improve the empirical content of a theory (i.e., to fulfil condition (i) for the intelligibility of theories). Theoretical models give little indications as to which feature of the phenomenon under scrutiny should be considered as relevant to the development of explanatory knowledge about that phenomenon. Further, a theoretical model does not help with testing the empirical (descriptive) accuracy of the relation it stipulates between theoretical terms and aspects of the phenomenon.[xviii]

3.iv Material models

When thinking about the type of description needed in order to model a phenomenon, Cartwright proposes to focus also on 'unprepared descriptions'. These are descriptions that (i) 'contain any information we think relevant, in whatever form we have available' and (ii) 'are chosen solely on the grounds of being empirically adequate' (1983:133). I consider the notion of unprepared description as a fruitful recognition, contra more traditional accounts of modelling that insist on referring to theoretical physics as a 'role model' for all other sciences, that within many experimental sciences the testing of theories need not be the starting point of investigation. What we witness in experimental biology is the use of several types of models that are not built by relying on a given theory. Their use is unavoidably guided by background knowledge and a commitment to the investigation of specific conceptual issues. Yet, the background knowledge needed to formulate a question should not be confused with the knowledge produced by trying to answer it. In preparing a description, scientists rely on an already formed hypothesis about how to answer a given theoretical question. The manipulation of unprepared descriptions, by contrast, does not require the choice of a specific interpretation to start with. That is to say that the manipulation of models sometimes requires no more than a general interest in exploring one or more aspects of the phenomena that they are taken to represent.[xix] In fact, and here I part company from Cartwright's 1983 account and I move into her revised 1999 framework, there are cases in which the unprepared description of a model is constituted by diagrams, objects or even samples of the phenomenon itself. These are cases where the model is a two- or three-dimensional object that is taken to be *representative of* a set of phenomena, in the sense of being used to explore which properties of the phenomena could turn out to be relevant to a theoretical account of it. Thus the model provides the epistemic access to phenomena that is necessary in the first place, in order to infer the kind of unprepared description that Cartwright is taking about.[xx] Epistemic access is granted first and foremost by material manipulation, since the amount of conceptual manipulation necessary to handle these models is minimal. The theoretical framework that they are representative for does not need to be specified in order for scientists to use them, since it will eventually be developed via their very physical manipulation. In other words, contrary to my characterisation of theoretical models, these 'material models' belong to a 'proto-explanatory context' (Ankeny, 2001) where scientists not only have not agreed on a theoretical explanation of the phenomena under investigation, but have not even settled, yet, on which properties of those phenomena could be relevant to the explanation (a decision that is crucial to the building first of an unprepared, then of a prepared description, thus enabling the shift from largely material to largely conceptual manipulation, and back).

Material models are tangible objects like scale models[xxi], samples, robots or, most emblematically, model organisms[xxii], which the scientists interact with through their sense perception. As put by Hacking in a different context, they are tools 'for doing, rather than thinking' (1983), where the action implied by 'doing' here is the material modification of characteristics of the models. A scale model or a model organism might seem far too complex an object to play a representational role: yet interacting with it - feeling, choosing, discarding and comparing its characteristics (whether implicitly or explicitly) – often leads scientists to consider alternative explanatory frameworks, precisely because of the largely underdetermined and dynamic nature of their features (on this point, see also Magnani, 2001 and Polanyi, 1958). This is possible at least in part because material models are both a *construct* and a *sample* of the integrated whole that they are representative of. As Griesemer makes clear, 'material models are able to serve certain sorts of theoretical functions more easily than abstract formal ones in virtue of their material link to the phenomena under scientific investigation. [..] They are robust to some changes of theoretical perspective because they are literally embodiments of phenomena' (1991:80). It is thus the degree to which material models embody phenomena that make them, in my view, take a substantially different epistemological role with respect to theoretical models as characterised above. Acknowledgements of the tight connection between learning and the material manipulation of objects have a long history and are increasingly accepted within current cognitive science.[xxiii] The philosophy of modelling is also, albeit more slowly, turning to the role of physical action and sensory perception in scientific reasoning.[xxiv] This move arguably results from a double recognition already hinted at within this paper: that of scientific epistemology as often geared towards intervention and control, rather than 'pure knowledge'; and that of experimental reasoning as yielding not only new ways of seeing, thinking of and talking about the world, but also new ways of interacting with it.

An important implication of my characterisation of material models is that they do not require the same type of abstraction processes that characterise theoretical models. Here the selection of parameters to be used in the model results from trial-and-error processes of standardisation and experimentation, rather than from the exclusion of features deemed irrelevant to the research context on the basis of a given theoretical hypothesis. As John Bonner famously put it, when discussing his discovery of chemotaxis as obtained by experiments on slime molds: ‘I had not carefully designed an experiment that would prove diffusion; I had managed it by accident. That and all the other observations I had made told me that the slime molds were in charge, not I. They would let me know their secrets on their terms, not mine' (2002:77-78). The trial-and-error selection of features to be standardised, and thus incorporated within a model, is due both to what is possible to maintain in practice and to what is ‘felt’ by scientists, via epistemic action, to be a promising representation of a particular class of phenomena (where what is ‘promising’ is not necessarily determined on the basis of an explicit theoretical framework, but typically depends on some vague intuitions on what properties of a phenomenon might turn out to be theoretically relevant).

4. ***Conclusion***

On the basis of the above characterisations of the differences in epistemological functions characterising theoretical and material models, I infer the following conclusions. Material models, by virtue of their large degree of independence from a theoretical account and their strong link to the phenomena that they are representative of, are especially useful in assessing the empirical value of parameters to be used in a theory. In other words, materially manipulated models are instrumental to the fulfilment of the first condition for the achievement of intelligible theories – that is, clarity about the empirical content of the theory eventually derived from their manipulation. In order to secure our chosen epistemic goal, however, another type of models is needed to provide explanatory power to the theory – it is not sufficient to know the properties of the phenomenon that it should refer to, we also want to know what the establishment of a relation between properties of the phenomenon and theoretical terms adds to our knowledge of the phenomenon. This is the function of theoretically manipulated models. No matter how blurred the distinction between these two types of models is, I believe that at least one of each type is needed in order to conduct research successfully leading to the development of an intelligible theory about a biological phenomenon.

As an example of how this works in practice, consider the current attempt, within large communities of biologists working on the same model organism, to gather all available data about the best-known model organisms into large, highly standardised digital databases. The goal of this type of projects is to gather together the vast amount of data on a specific organism as collected within several biological fields, so as to integrate those data and thus enhance our overall understanding of the biology of that organism. Examples of such enterprises are resources such as the Generic Model Organism Database [GMOD], gathering together data coming from different organisms but thought to have relevance for many life-forms; The Arabidopsis Information Resource [TAIR], storing data from research on the plant Arabidopsis thaliana; FlyBase, the database for Drosophila; and BioCyc, assembling information over metabolic cycles in the main model organisms.[xxv] These resources pursue their goal by constructing digital models of various aspects of the biology of these organisms, including their molecular structure, genomics, metabolism and developmental processes. The visualisations of data proposed by these databases are based on a variety of abstract concepts and theories about the phenomena to which data refer. Only through reference to theories and concepts can the vast amount of data available on specific organisms be assembled and visualised; further, the concepts used also enhance the integrative power of those models, which gather together data collected within different fields, thus enhancing interdisciplinary dialogue and information exchange within model organism communities. The resulting visualisations of data can thus be viewed as theoretical models: their construction is based on a well-prepared description of the phenomena to which they apply, whose explanatory power comes from its reliance on specific theories concerning those phenomena.

Scientists leading the resources who produce such theoretical models insist that the increasing sophistication of these images could, at some point in the future, render much of the current experiments on real organisms obsolete. Namely, they argue that data visualisations in TAIR or GMOD could be used as 'discovery tools', thus fulfilling the exploratory function that I assigned to laboratory organisms in section 3.iii: theoretical models could substitute material models as tools for the elaboration of intelligible theories about organismal biology. In light of my considerations above, I find this expectation entirely misguided. There is no way in which the conceptual manipulation of a virtual model displayed by one of those databases could substitute the material manipulation of an actual organism in the laboratory, because it is thanks to the latter research activity that the virtual model acquires empirical content. Scientists working on databases seem to think that reference to empirical data would suffice to confer empirical content to a model: however, by the time that the data have been organised and displayed according to the theoretical frameworks adopted within the databases, it becomes very difficult for a biologist to establish the relation between the resulting digital model and the phenomenon (process, component, function of the biology of the organism) to which it refers. Virtual displays of data are thus certainly priceless in their contribution to our understanding of organismal biology: they constitute theoretical models of great explanatory power, insofar as they use abstract concepts in order to classify, organise and relate the empirical data gathered from a given organism to the theoretical knowledge available on that organism. However, they cannot be used to formulate intelligible theories about the phenomena that they depict, unless the biologists referring to them do not attribute empirical content to that knowledge by manipulating actual organisms. Research on actual model organisms is indeed about assessing what organisms are made of, how organismal components interact with each other and which process gives rise to which other. This type of knowledge is needed to enrich the theoretical models provided within databases: the images therein are so abstract that they cannot be brought to bear on the phenomena that they depict, unless biologists can complement them with experimental research on actual organisms.

My analysis of the complementary epistemic utility of theoretical and material models constitutes, in my view, a convincing argument for maintaining an analytic distinction between conceptual and material manipulation of models, despite its above-mentioned fragility when considering how tightly these two dimensions are intertwined in scientific practice. Another important motivation is the need to counter a growing tendency among biologists of all trades: that is the tendency to value conceptual manipulation of models over and above their material manipulation. As I hope to have shown, the usefulness of splendid research tools such as computer simulations and mathematical models towards an improved understanding of the natural world is due as much to their own characteristics as to their complementation by material models. The largely happy marriage of empirical accuracy with theoretical tractability is, after all, one of the characteristic strengths of the biological sciences. Favouring predictive accuracy over empirical accuracy would amount to threatening the intelligibility of the knowledge produced. The combined use of what I called theoretical and material models – and thus the constant recourse to both conceptual and material manipulation in modelling – is the only possible guarantee for the acquisition of intelligible theories allowing to explain and predict the behaviour of the system under scrutiny, while at the same time maintaining and constantly re-defining the descriptive value of the parameters that are used.

As stated in my second section, this argument is also meant as a step towards the development of a pluralistic account of modelling strategies. Such a philosophical account would focus on the epistemological significance of differences among models, rather than emphasising what unifies them (again quoting Levins: 'understanding is not achieved by generality alone, but by a relation between the general and the particular'; 1984:26). My discussion of the distinct features of theoretical and material models hopefully illustrates that a renewed attention to the pluralism of models may lead to the formulation of research questions that differ considerably from the ones cherished within 'single model approaches', and yet are in dire need of philosophical attention. Among those, for instance: what makes the combination of different models so valuable? How are relevant types of models chosen and integrated within any specific research context? How do such choices relate to the expected outcomes of research, and what impact do they actually have on such outcomes when implemented? As testified by the contributions collected in this volume, these questions are very close to the ones posed by biologists themselves when confronting and discussing their modelling strategies. The development of a pluralistic account of modelling practices might thus provide fertile ground for an increase in collaboration between philosophers and scientists, which might in turn go a long way toward unravelling the multi-faceted contributions of models to scientific theorising.

Bibliography

Anderson ML (2003) Embodied Cognition: A field guide. *Artificial Intelligence* 149: 91-130.

Ankeny RA (2001) Model Organisms as Models: Understanding the 'Lingua Franca' of the Human Genome Project. *Philosophy of Science* 68(3): S251-S261

Bailer-Jones D (2000) Modelling Extended Extragalactic Radio Sources. *Studies in the History and Philosophy of Modern Physics* 31(1): 49-74.

Bailer-Jones D (2002) Scientists' Thoughts on Scientific Models. *Perspectives on Science* 10: 275-301

Bailer-Jones D (2003) When Scientific Models Represent. *International Studies in the Philosophy of Science* 17: 59-74

Beatty J (1997) Why do biologists argue like they do? *Philosophy of Science* 64: S432-443

Bonner JT (2002) *Lives of a Biologist*. Cambridge, MA: Harvard University Press.

Boumans M (2001) 'Ceteris paribus' conditions: materiality and the application of economic theories. *Journal of Economic Methodology* 8(1):11-26.

Cartwright N (1983) *How the Laws of Physics Lie*. Cambridge: Cambridge University Press.

Cartwright N (1999) *The Dappled World*. Cambridge: Cambridge University Press.

Chang H and Leonelli S (2005) Infrared Metaphysics: Theory-Choice and the Ontology of Radiation (Part 2).

Studies in the History and Philosophy of Science: Part A 36(4): 687—706

Cooper G (1996) Theoretical Modeling and Biological Laws. *Philosophy of Science* 63: S28-S35

Dretske FI (1977) Laws of Nature. *Philosophy of Science* 44: 248-68.

Galison P (1997) *Image and Logic: A Material Culture of Microphysics*. Chicago: University of Chicago Press.

Giere R (1999) Using Models to Represent Reality. In: *Model-Based Reasoning in Scientific Discovery* (Magnani L, Nersessian NJ, Thagard P, eds), pp 41-57. New York: Kluwer Academic/Plenum Publishers.

Griesemer JR (1990) Material Models in Biology. *PSA* 2: 79-93

Griesemer JR (2003) Genetics from an Evolutionary Process Perspective. In: *Genes in Development* (Neumann-Held EM, Rehmann-Sutter C, eds), pp 343-375. Duhram: Duke University Press.

Griesemer JR (2000) Development, Culture and the Units of Inheritance. *Philosophy of Science* 67: S348-368.

Hacking I (1983) *Representing and Intervening.* Oxford: Oxford University Press.

Hempel K (1965) *Aspects of Scientific Explanation.* New York: Free Press.

Hesse M (1966) *Models and Analogies in Science.* London and New York: Sheed and Ward.

Keller EF (2002) *Making Sense of Life.* Cambridge, MA: Harvard University Press.

Kohler RE (1994) *Lords of the Fly: Drosophila Geneticists and the Experimental Life*. Chicago: University of Chicago Press.

Leonelli S (2006a) Cultivando Hierba, Produciendo Conocimiento. Una Historia Epistemologica de Arabidopsis thaliana. In: *Variedad Sin Limites. Las Rapresentationes en la Ciencia* (Suarez E, ed). Universidad Autonoma de Mexico y Editor Limusa.

Leonelli S (2006b) *Weed for Thought. Using Arabidopsis thaliana to Understand Plant Biology*. Vrije Universiteit Amsterdam. Unpublished PhD Dissertation.

Levins R (1984). The strategy of model building in population biology. In: *Conceptual issues in evolutionary biology* (Sober E, ed), pp 18–27. Cambridge, MA: MIT Press.

Longino H (2002) *The Fate of Knowledge.* Princeton and Oxford: Princeton University Press.

Machamer P, Darden L, Craver C (2000) Thinking About Mechanisms. *Philosophy of Science*. 67: 1-25.

Magnani L (2001) *Abduction, Reason and Science. Processes of Discovery and Explanation.* New York: Kluwer/Plenum Publishers.

Morgan MS and Morrison M (1999) *Models as Mediators.* Cambridge: Cambridge University Press.

Morgan M (2003) Experiments Without Material Intervention. In: *The Philosophy of Scientific Experimentation* (Radder H, ed), pp 216-235. Pittsburgh: University of Pittsburgh Press.

Morrison M (2002) Models and Statistics: Pearson and Fisher on Mendelian Populations. *British Journal for the Philosophy of Science* 53: 39-68.

Mayr E, Provine W (1980) *The Evolutionary Synthesis*. Cambridge, MA: Harvard University Press.

Polanyi M (1958) *Personal Knowledge. Towards a Post-Critical Philosophy*. London: Routledge.

Puccia CJ, Levins R (1985) Qualitative Models. In: *Qualitative Modeling of Complex Systems. An Introduction to Loop Analysis and Time Averaging* (Puccia CJ, Levins R, eds), pp 1-11. Cambridge, Massachusetts: Harvard University Press.

Radder H (1996) *In and About the World. Philosophical Studies of Science and Technology*. New York, Albany: State University of New York Press.

Radder H (2002) How Concepts Both Structure the World and Abstract from It. *Review of Metaphysics* 55: 581-613.

Radder H (2003) Technology and Theory in Experimental Science. In: *The Philosophy of Scientific Experimentation*

(Radder H, ed), pp 152-173. Pittsburgh: University of Pittsburgh Press.

de Regt H (2004) Discussion Note: Making Sense of Understanding. *Philosophy of Science* 71: 98-109.

de Regt H, Dieks D (2005) A Contextual Approach to Scientific Understanding. *Synthese* 144: 137-160.

Rheinberger HJ (1997) *Toward a History of Epistemic Things*. Stanford: Stanford University Press.

Rose S (1997) *Lifelines.* Oxford: Oxford University Press

Salmon WC (1998) *Causality and Explanation*. Oxford: Oxford University Press.

Smith B (2004) Beyond Concepts: Ontology as a Reality Representation. In: *Formal Ontology and Information Systems. Proceedings of the Third International Conference (FOIS 2004)* (Varzi A, Vieu L, eds), pp 73-84. Amsterdam: IOS Press.

Solomon M (2000) *Social Empiricism*. MIT Press.

Star SL, Griesemer JR (1989) Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39. *Social Studies of Science* 19: 387-420

van der Steen WJ (1995) *Facts, Values and Methodology. A New Approach to Ethics.* Amsterdam and Atlanta: Rodopi Press.

Suarez E (2001) Satellite-DNA: A Case-Study for the Evolution of Experimental Techniques. *Studies in the History and Philosophy of the Biological and Biomedical Sciences* 32(1): 31-57.

Suppes P (1960) A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences. In: *Studies in the Methodology and Foundations of Science. Selected Papers from 1951 to 1969* (Suppes P, ed), pp 10-23. Dordrecht: Reidel.

Suppes P (2002) *Representation and Invariance of Scientific Structures*. Stanford: Stanford University Press.

Trout JD (2002) Scientific Understanding and the Sense of Understanding. *Philosophy of Science* 69(2): 212-233.

Wimsatt WC (1987) False Models as Means to Truer Theories. In: *Neutral Models in Biology* (Niteki M, Hoffman A, eds), pp 23-55. London: Oxford University Press.

Wimsatt WC (1981) Robustness, Reliability and Overdetermination. In: *Scientific Enquiry and the Social Sciences* (Brewer M, Collins B, eds), pp 124-163. San Francisco: Jossey-Bass.

Winther R (2003) *Formal and Compositional Biology as Two Kinds of Biological Theorising*. University of Indiana, Bloomington. Unpublished PhD dissertation.

Winther R (2006) An Empirical Analysis of Theoretical Perspectives in Compositional Biology. *Biology and Philosophy*.

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[i] For instance, see Patrick Suppes' restriction of the definition of model to set-theoretical constructs (2002) and Daniela Bailer-Jones' view of models as mental representations (2003).

[ii] Again, two illustrations among many possible examples: Hesse's early work on model-based analogical reasoning as heuristic to the development of theories (Hesse, 1963); and Cartwright's (1983) account of how features of representationally powerful models are abstracted from phenomena and idealised from theories (which she modified precisely by focusing on the distinctions among models in her later work).

[iii] Note that most philosophers hitherto busy with 'single model approaches' are trying to develop more pluralistic approach. Their work addresses directly the piecemeal nature of modeling practices. For instance, Cartwright's (1999) framework is unique in its attempt to deal with different types of models across the natural and the

social sciences, while Suarez (1999) carefully considers the pluralism among notions of theory idealised within models. Bailer-Jones published both on the different ideas that practicing scientists hold on what models are (2002) and on the use of 'sub-models' in order to obtain an overall model of a phenomenon (2000). Arguably, however, these philosophical accounts focus on how data from different models can be made to overlap into a coherent story, rather than looking at why and how do models provide different types of information.

[iv] Historians of science contributed extensive evidence for the importance of combining different models for the development of scientific research. For instance, some controversial cases of theory-choice characterising the history of physics and biology are being usefully reconstructed and understood as depending on changes in the range and interpretation of empirical evidence as much as on creative transformations in the number and sophistication of the modelling practices favoured within each of the rival theories (see for instance de Regt (2001), Keller (1999, 2000), Boumans (2001), Chang and Leonelli (2005)). Equally indicative are the analyses of the evolution of research systems presented by Kohler (1991), Rheinberger (1997), Griesemer (2001), Galison (1997) among others.

[v] This view, characteristic of the semantic account of models, found a strong proponent in Suppe, among others (1960, 2002). For forceful criticisms of what Giere refers to as the 'instantial view' on models, see Giere (1999) and Suarez (1999).

[vi] This very feature also makes this notion of representation rather slippery, for how are we to draw clear distinctions between a representation and 'the real nature of the system or theory' that it represents? Shank's and Koehnle's analysis of modelling within behavioural biology (this volume) can be used as a forceful illustration of this problem. In their view, most elements used by practicing biologists as tools for investigation – *including* theories, data and material samples - should be thought of as models, that is as representations conveying information about the real nature of what they represent in a limited and provisional way. This argument comes from the recognition that, within any research context, the stipulation of what the phenomenon and theory in question are depends on the tools used to investigate them just as much as the choice and handling of those tools is based on what scientists endorse as theoretical hypotheses and descriptions of reality. How should we then draw boundaries between the notion of 'mediators' and the

notions of theory and/or phenomena that they are supposed to mediate? The risk of a slippery slope, leading to the denial of the very possibility to distinguish between the model and what it models, represents a very serious problem for Morgan and Morrison: it runs counter their basic intuition, which I share and value, that an analysis of the representational value of models necessarily includes making explicit what it is that models represent at any given moment of the research process. Morgan and Morrison try to avoid the problem by a sophisticated discussion of what they mean by 'theory' and 'phenomenon', a detailed assessment of which eludes the present purposes of this paper (see Morgan and Morrison (1999) and Cartwright (1983, 1999)). I thank James Griesemer for discussions on this point.

[vii] As Claude Bernard famously put it, in new ways of acting, rather than seeing, talking or thinking; see also Longino 2002:124 on this point.

[viii] Another important set of considerations underscoring the importance of experimental protocols for achieving scientific understanding is provided by Hans Radder (2002), arguing that the achievement of methods to replicate experimental results is a necessary step towards the development of theories.

[ix] See Smith (2003) and Leonelli (2006b) about the increasing use of gene ontologies as a framework for data organisation and storage.

[x] See Winther (2003, 2006) for a discussion of the differences between styles of theorisation adopted within what he calls 'formal' and 'compositional' biology.

[xi] I exclude here the idea that the biological sciences do not produce any kind of theory (e.g. Cooper, 1996). This claim is based on restricting the notion of theory to indicate subsumption under a universal law, as in the 'received view' on scientific explanation (e.g. Hempel, 1965). Case-based analyses within both the life and the physical sciences indicate that this latter notion of theory is far too restrictive to account for all the types of generalisable knowledge acquired via scientific practice. Rather than interpreting this finding as a denial of the relevance of the notion of theory to biological knowledge, I see it as calling for its reformulation.

[xii] For details on this view see Levins (1984), Puccia and Levins (1985), as well as the elaborations of Levins' argument presented by Wimsatt (1981, 1987), Griesemer (2000, 2003), Shank and Koehnle (this volume) and Wim van der Steen (1995: 26ff).

[xiii] A similar argument is offered, in nuce, by Bailer-Jones (2000).

[xiv] On the notion of commitment and its variety, see also Polanyi (1958:320).

[xv] See de Regt and Dieks (2005) and de Regt (2004, 2001).

[xvi] For the purposes of this paper, I will only scrutinise the role of models in the acquisition of understanding, thus leaving aside considerations about the individual and/or community to whom theories are intelligible. For some insight into this difficult topic, see Longino (2002), Solomon (2003) and Leonelli (2006b).

[xvii] I do not believe that these two criteria represent sufficient conditions for a theory to be deemed intelligible. I do, however, take them to specify two necessary conditions for this. Several scientific theories do not conform to one or the other of these two requirements. Think of string theory as a glaring case of a highly explanatory theory whose empirical content cannot be specified; and of early versions of classification systems (such as the Linnaean taxonomy) as highly accurate in their empirical content, but arguably containing little or no narrative to make sense of how the categories that they use to individuate relevant properties of specimen (i.e., the theoretical terms in this case) may account for the structure, function and/or

existence of such properties. In both cases, intelligibility does not figure among the many virtues of these theories: the former contributes an explanation without specifying the phenomena to which it applies, while the latter provides a description without clarifying its explanatory relevance.

[xviii] I am upholding the empiricist view that it is not sufficient to rely on convention or whim in stipulating and maintaining such relation in order to secure the empirical content of a theory.

[xix] For further reflections on models as exploratory tools within an experimental context, see Radder (2003) and references therein.

[xx] Cartwright herself recognised this in her most recent work and thus distinguished these 'representative models' from the interpretive ones (1999:180).

[xxi] On scale models, see Giere (1999).

[xxii] For arguments on model organisms as models, see Ankeny (2000) and Leonelli (2006a).

[xxiii] Indicative of this trend is the recent focus of fields such as artificial intelligence on the idea of 'embodied cognition' – see for instance Andersen (2003) and references therein.

[xxiv] See for instance Morgan (2003), Magnani (2001)

[xxv] For further information about these projects, see the relevant webpages. GMOD: http://www.gmod.org/home; TAIR: www.arabidopsis.org; FlyBase: http://flybase.bio.indiana.edu/; BioCyc: http://biocyc.org/metacyc/.