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Abstract

One way in which to address the intriguing relations between science and reality is to work via the models (mathematical structures) of formal scientific theories which are interpretations under which these theories turn out to be true. The so-called 'statement approach' to scientific theories -- characteristic for instance of Nagel, Carnap, and Hempel --depicts theories in terms of 'symbolic languages' and some set of 'correspondence rules' or 'definition principles'. The defenders of the oppositionist non-statement approach advocate an analysis where the language in which the theory is formulated plays a much smaller role. They hold that foundational problems in the various sciences can in general be better addressed by focusing on the models these sciences employ than by reformulating the products of these sciences in some appropriate language. My model-theoretic realist account of science lies decidedly within the non-statement context, although I retain the notion of a theory as a deductively closed set of sentences (expressed in some appropriate language), in this paper I shall focus -- against the background of a model-theoretic account of science -- on the approach to the reality-science dichotomy offered by Nancy Cartwright and briefly comment on a few aspects of Roy Bhaskar's transcendental realism. I shall, in conclusion, show how a model-theoretic approach such as mine can combine the best of these two approaches.

1. Introduction

Realists traditionally believe that science is somehow 'about' reality (Nature). Interpretations of the relations between science and reality have varied from notions of a one-to-one correspondence relation, to relations of approximation, to relations of such an 'open' nature[1] that they have become quite meaningless.

One way in which to address these intriguing relations (or, in the case of the more anti-realist inclined, to decide whether any such relations do in fact exist) is to work via the models (mathematical structures) of formal scientific theories which are interpretations under which these theories turn out to be true. The so-called 'statement approach' to scientific theories -- characteristic for instance of Nagel, Carnap, and Hempel -- depicts theories in terms of 'symbolic languages' and some set of 'correspondence rules' or 'definiion principles'. This set of 'bridge principles' supposedly ascribe 'meaning' to the 'theoretical' terms of theories that are expressed in the appropriate symbolic language. Another more accessible and perhaps more realistic approach to scientific theories -- the so-called 'non-statement' approach --examines theories in terms of sets of mathematical structures.

In opposition to the advocates of the statement approach's claims that theories are formulated in some (first-order) symbolic language with direct links to reality, the defenders of the non-statement approach advocate an analysis where the language in which the theory is formulated plays a much smaller role. They hold that foundational problems in the various sciences can in general be better addressed by focusing on the models these sciences employ than by reformulating the products of these sciences in some appropriate language. Advocates of this approach vary from the defenders of the structuralist programme, the different views of Patrick Suppes, Bas van Fraassen, Frederick Suppe, and Ronald Giere, to various Polish logicians such as Wojcicki.[2]

My own model-theoretic approach lies decidedly within the non-statement context, although I retain the notion of a theory as a deductively closed set of sentences (expressed in some appropriate language). Theoretical entities' possible links with reality are an integral part of the various truth relations in which the theory in question (albeit only via its models) stands during the various stages of the scientific process. I claim that these truth relations can only be examined and talked about in a sensible way if they are viewed as (interpretative truth) relations between theories, models, certain empirical subsets of these models, and aspects of reality.

Nancy Cartwright is one of the most influential philosophers currently writing on the role of models in the process of science. In the first half of this article I shall give a brief exposition of the main features of her account of science. Although some of her work comes very close to the model-theoretic interpretation of science that I am offering, there are also serious differences between our approaches. In the second half of this article I shall comment on a few related aspects of Bhaskar's transcendental realism. Thereafter I shall show how a model-theoretic account of scientific knowledge [see Ruttkamp (1999,section 2, in this issue) for an exposition of this account] has more to offer current realism than either Cartwright's causal account of science or Roy Bhaskar's transcendental realist account of science.

2. Nancy Cartwright's account of science 2.1 Introduction

Cartwright's main claim is that scientific theories (or rather, the 'fundamental laws' which are part of the theories' content) have very little or nothing to say about reality. She argues for this with the aid of two arguments:

-- An instrumentalist, anti-fundamentalist, and, mostly, anti-realist, strategy arguing against a 'theory-driven' interpretation of the function of models in (philosophy of) science; and

-- A metaphysical argument offering a hierarchy of causalities, dealing at the highest level with the capacities of real things -- representing a 'patchwork of laws' -- based on a notion of reality as not necessarily being ordered and structured, even possibly being 'disunified'.

2.2 Phenomenological and fundamental laws

Nancy Cartwright claims in How the laws of physics lie (1983) that considering the truth of the (fundamental) laws of physics will force anyone to admit that almost all of these laws are strictly false, i.e. 'lie', because they are valid only under certain circumstances or given certain conditions that do not strictly hold in reality. However, it is interesting to note that the implication ['Conditions' arrow right 'Law'] is (logically) true even if the 'conditions' (the antecedent of the implication) are not satisfied. Therefore 'inapplicable' would be more appropriate than 'lying', which seems to imply 'false' in Cartwright's context.

Her distinction between phenomenological and fundamental laws is centred around the distinction between the particular (concrete) and the general. She claims this distinction to be in the Aristotelian tradition of emphasising the richness and vitality of the particular, although it is interesting that she chooses not to refer to the fact that Aristotle saw the richness of the particular only becoming fully realised in the 'universal' (or general). My approach is therefore perhaps more 'Aristotelian' than hers, because my model-theoretic interpretation of the process of science will be meaningless without the role it ascribes to general statements (i.e. scientific theories), while Cartwright is always trying to get away from the need to assign too meaningful a role to this kind of statement. She quotes (Cartwright 1983: 9) Boltzmann's equation and the general equation of continuity used by Maxwell in his explanation of the motion in a radiometer as examples of fundamental laws and describes these laws as 'general, abstract equations; ... not about any particular happenings in any particular circumstances.'[3] I do not think that this description of fundamental laws is debatable. What is debatable, however, is whether this necessarily leads to the conclusion that fundamental laws have no links with aspects of the real world.

Phenomenological laws are complex descriptions of actual situations in very specific terms -- 'what can be confirmed through tests and comparisons with observations are phenomenological laws -- comparatively detailed descriptions of concrete situations, which because of their richness in detail, do not have great generality (sometimes called 'low-level' generalisations)' (Cartwright 1983: 129). Cartwright (ibid.) claims that it is phenomenological laws that fulfil the 'traditional role' of laws in the sense that they describe empirical regularities --which fundamental laws -- because they are too general and much too simple -- cannot do, since they cannot account for the actually observed variety in the behaviour of objects in reality. Fundamental laws do not have anything to say about 'regularities' (constant conjunctions of events in Humean terms), because describing regular behaviour requires more and more complicated descriptions of the situation. The descriptive phenomenological laws thus have less and less generality and they can never be stated without exceptions, while fundamental laws 'by contrast, are simple, general, and without exception' (Cartwright 1983:157).

Cartwright (1983: 55ff.) quotes the universal law of gravitation, Schrodinger's equation, and Maxwell's equations as further examples of fundamental laws, and gives (ibid.: 2) Airy's law of Faraday's magneto-optical effect as an example of a phenomenological law, because Airy's law does not explain Faraday's law (in the way that the more theoretical treatment of it in terms of electron theory by Lorentz does), but rather describes the actual changes in Faraday's dense borosilicate glass as magnetic fields rotate the plane of polarisation of light (while Lorentz's formulation of the Faraday effect appeals to the electron theory, and so has an underlying explanatory content to it, that Airy's law does not have). Other examples of phenomenological laws she discusses (Cartwright 1983: 55ff.) are the performance characteristics of lasers as specified by their manufacturers, and the phenomenology of fundamental particle interaction, including things like scattering cross-sections. She choses these examples, because, as Alan Chalmers (1987: 83) points out, '[m]easurements of the rotation of the plane of polarisation are related to the strength of the magnetic field that causes it in the way specified by Airy's formula, lasers do perform [mostly!] in the way specified in the manufacturer's instruction manual, and, for example, scattering cross-sections of interacting proton beams at some specified energy are reproducible whenever such beams are made to interact'.[4] It seems then that Cartwright claims fundamental laws to be explanatory of the content of phenomenological laws, and phenomenological laws to be descriptive of aspects of reality. Explanation and description are thus done at different levels of the scientific process. This is a very important point, and is also accommodated in my model-theoretic account of science, but it is not a point that necessarily scores any marks for any kind of anti-realism. I shall show that, on the contrary, it is rather a supportive point in a model-theoretic realist account of science.

It is claims like the following about fundamental laws that do not seem to be entirely correct from a model-theoretic perspective -- '... fundamental laws ... do not hold for the most part, or even approximately for the most part, and conversely, those laws which are more or less true much of the time are not fundamental' (1989b: 174). The unease that such claims cause is not necessarily the result of what she says about the nature of these laws,[5] but rather that they seem to imply that she still believes in some absolute notion of truth. She stresses that fundamental laws can -- possibly and at most -- explain the content of phenomenological laws by organising or classifying them, and that fundamental laws therefore do not describe the behaviour of real objects in the world. However, as Rueger and Sharp (1996: 95) point out, fundamental laws are in this context still useful to her even though they are not 'true descriptions' of real objects or their behaviour, precisely because they 'organise and classify our knowledge in an elegant and efficient manner' (Cartwright 1983: 100). She creates the impression in How the laws of physics lie (1983) though, that she might view the fact that fundamental laws only serve to organise and summarise real phenomena as a particular weakness of these kinds of law, because she puts so much emphasis on the fact that 'the cost of explanatory power is descriptive adequacy' (Cartwright 1983: 3), which seems to imply that the final cost of explanatory power is the loss of the truth of fundamental laws.

In my version of the scientific process, however, that is not a problem, and, I might add, neither should it be in hers, because we both accept and acknowledge from the outset that truth is a very local and limited notion, albeit in a more complex way than is ordinarily thought. In other words, the fact that she (ibid.:5) denies that 'explanation is a guide to truth', surely is only problematic if one thinks of truth as a universal notion. She does, in a sense, make amends in Nature's capacities and their measurement (1989b), as well as specifically stressing pretty clearly in her article entitled Fables and models (1986), the fact that questions of truth are not necessarily questions of universality.[6]

Cartwright is arguing against the notion that fundamental laws give true descriptions of real phenomena. And thus, she is also arguing against my notion of science, because although we both acknowledge the use of models to mediate between the concrete and the abstract, she still thinks that accepting some kind of realism with regard to fundamental laws means accepting an absolute notion of truth, when, paradoxically enough -- as I have mentioned above -- she herself still seems to believe in this notion in any case. Why else does she say that fundamental laws lie? If she takes seriously the possibility of contextualising the 'truth' of these kinds of laws with the help of abstract models, why then does she still argue for the falsity of fundamental laws as if it is not possible for her to be satisfied with a 'localised' version of truth? She argues against assumptions of the absolute truth of fundamental laws by stressing the concrete character of phenomenological laws. However, she cannot acknowledge the semantical links between theories and models that I claim exist, because she apparently thinks that would somehow imply that she believes theories to be absolutely true, and as they are not, she would rather discard them completely as part of the meaningful (and descriptive) side of the scientific process, and simply acknowledge (a la Duhem) their organising role, than try to find (like I am) some kind of reason for them to be part of the chain of factors or concepts that in the end make science mean (and explain) something to people living in the real world. (More on these issues a little later on in this section.)

Returning to Cartwright's interpretation of 'phenomenological', she does not use it in terms of its usual interpretation as referring to the 'observable' (Cartwright, 1983; Cartwright, 1986), but rather points to the fact that this kind of law describes actual behaviour of real objects. She believes that, regardless of whether an object is observable or not, if we can manipulate it [intervene in its behaviour a la Hacking (see his Intervening and representing (1983)], we can formulate (true) low-level generalisations which accurately describe the (causal) relations into which it enters. Phenomenological laws describe particular events while whatever fundamental laws have to say is always about various situations in reality in one sweep. So, then -- because fundamental laws can supposedly do no more than explain the content of phenomenological laws (in accordance with the covering law model of explanation, about which Cartwright has quite a lot to say), and good explanations are supposed to be simple (abstract) and general -- it seems that fundamental laws can indeed never directly be about any particular aspect of reality. However, it is important to understand that Cartwright, by referring to phenomenological laws as low-level generalisations, means to say that they too, have an abstract nature in the sense of being idealised descriptions of objects in reality. Moreover, as Chalmers (1993: 199) also points out, quite in accordance with the fact that phenomenological laws too, are generalisations that involve conditions, and even the much discussed ceteris paribus conditions, these laws themselves sometimes fail to adequately describe the behaviour of real objects. (Think, in this regard, most simply of Cartwright's example of the instructor's manual of lasers -- surely lasers can malfunction?)

But how then does Cartwright conceive of relating fundamental with phenomenological laws and either (or both) of these sets with real objects? It seems that the 'content' of fundamental laws is filled in by various abstract models.[7] In Cartwright's scheme of things,[8] these models mediate between theories and fundamental laws on the one hand, and phenomenological laws and reality on the other. According to the model-theoretic interpretation of the process of science that I am offering, models mediate between theories (linguistic systems) and systems in reality. Phenomenological laws, in my terms, would simply be part of the content (or properties) of the models interpreting scientific theories, and they would be expressible as sentences true in the model(s) under consideration, as well as possibly true in some empirical substructure(s) of these models, and so, true of some real system.[9]

Cartwright explains in How the lairs ... (1983: 4), that the 'route from theory to reality is from theory to model, and then from model to phenomenological law', and goes on to claim that 'phenomenological laws are indeed true of objects in reality -- or might be; but the fundamental laws are true only of objects in the model'. In other words, she does not see the same kind of referential relation between models and systems in reality that I see. The reason, I think, is that she worries too much about the ideal character of the models and the role of the ceteris paribus clauses needed to interpret phenomenological laws. Also, it is difficult to see how a very specific link with reality can be given by a law, even if it is a phenomenological one. For example, if Newton's laws are fundamental, Kepler's are phenomenological (and deducible from Newton's), but the direct observations (done in both cases) are specific activities (looking in a particular precise direction) carried out at a specific time (specific to the second). Statements describing these kinds of activity surely are not laws, but can rather be expressed in terms of some empirical model which would be a subset of the model of the theory under consideration and which interprets experimental data and empirical activities leading to the formulation of these data.

Cartwright is not a complete anti-realist, as her interpretation of phenomenological laws clearly shows,[10] it simply seems to be the case that she cannot see how to escape the antirealist implications of the abstract nature of fundamental laws. This anti-realism has its origin in her interpretation of the 'explanatory' role fundamental laws play in the practice of science (physics). It seems as if Cartwright is implying -- in the sense of the validity of fundamental laws being dependent on abstract, idealised situations -- that fundamental laws must hold regardless of the individual arrangements of things possible in each separate situation in reality that they (these laws) are 'about'; while phenomenological laws potentially describe the actual situations to which they are applied. Here again it seems as if she still holds on to some belief in truth per se ('... fundamental laws hold regardless of ... individual arrangements ...'), although her entire crusade is supposedly focused on showing the local character of truth. Perhaps this should be taken as a warning of the danger involved in attempting to exclude the role of fundamental laws from the model-theoretic process, since it makes for certain invalid -- surely unintended? -- conclusions. The problem is that Cartwright does not acknowledge that the sense in which fundamental laws hold 'regardless of individual arrangements' is merely in terms of their abstract nature. This should not be linked to thinking that therefore they are universally true. Nor should it be thought that because they are too general to describe real systems, they are false. Questions of truth can only he addressed in terms of the conceptual models and empirical models of scientific theories.

In my account of the scientific process, as remarked above, I show how meaningless any talk about the truth (or validity) of fundamental laws per se is and I argue that these issues can be meaningfully addressed only in terms of the infrastructure of the models interpreting these laws. But if, then, in Cartwright's terms, the main distinction between fundamental and phenomenological laws is taken to be the fact that fundamental laws hold by themselves -- albeit only in certain 'unreal' situations -- while phenomenological laws can only hold on account of some (non-necessary) arrangement of circumstances, what does that imply for scientific explanation, prediction and the description of real objects?

Phenomenological laws describe actual events, because although they are usually mathematically formulated in physics, no fundamental explanation of the mathematical formulae nor of the mechanisms underlying these formulae are assumed in these laws.[11] The problems related to scientific explanation in the context of the 'leap' from fundamental and even phenomenological laws into more messy 'worldly' situations are emphasised differently in a model-theoretic approach. Within such a model of science -- as I have pointed out before -- it is usually taken that scientific theories explain the content of their models, and through these models, some aspects of reality and the behaviour of certain phenomena may be described and predicted. A theory does not always necessarily explain every detail of the system in reality it is focusing on. Newton's mechanics does not explain the phenomenon of gravity. It rather explains the influence of gravity on certain events and in that sense, it describes gravity rather than explains it. The old (deductive-homological) symmetry between explanation and description should be 'stretched' such that it covers all three strata of a model-theoretic model of science. If this is not done the fact that the descriptions of gravity in the above sense may enable someone applying Newton's mechanics to make certain predictions concerning the results of the exertion of the forces of gravity, without explaining gravity itself, cannot be grasped, and then it might seem that explanatory power indeed diminishes descriptive power, as Cartwright so often claims.

Thus, in model-theoretic terms scientific theories are said to explain in the basic sense of theories explaining the content of their models by establishing deductive links between the sentences expressing what is true in some model. Thus in a model-theoretic account of science a theory and its conceptual models 'explain' in the strict logical sense that a predicted phenomenon can be logically deducted from the theory and the model(s) in question. Newton's three laws of motion and his law of gravity plus the model of our solar system -- in terms of current scientific knowledge -- explain why we see Mars tonight at eight o'clock in a particular position. In these terms, a preceding theory (e.g. Newton's laws of motion and gravitation) may describe models which (under certain conditions, within a certain interpretation, approximately) are also models of a later 'higher order' theory (say the general theory of relativity), and then the latter may be said to explain the former.[12] The better explanatory power of later theories with respect to the content of their models is then the result of at least the higher level of accuracy of the theory. For instance, as Penrose (1997: 57) points out, Einstein's general theory of relativity can be said to be accurate to about one part in 10[14], which is about ten million times as accurate as Newton's mechanics, which may roughly be taken to be accurate to about one part in 10[7]. Improved accuracy is one embodiment of that continuity and progress in science with which some form of realism sits comfortably.

The referential relations between model terms and objects and relations in some real system are (indeed, as Cartwright claims) more descriptive than explanatory. However, this need not result in anything as negative as Cartwright's claims of high explanatory power of fundamental laws diminishing their 'truth making' power. If the whole interpretative chain -- i.e. from terms of some theory, to terms in some conceptual model(s) of the theory, to terms in some empirical substructure of the conceptual model in question, to some real system -- is taken into account, the fact that models seemingly describe and theories explain only the content of their conceptual models does not necessarily have any anti-realist consequences. Usually it is even the case that theories contain some basic notion that they merely describe, even if they do explain the rest of the content of their models -- for example, Newton's mechanics does not explain the notion of 'gravity' itself, but merely describes its behaviour. The distinguishability and interconnectedness of the three stages roughly outlined by this 'interpretative chain' --as set out in Ruttkamp (1999) -- show however that description (a feature mainly of models) and explanation (a feature mainly of theories or fundamental laws) are inseparable, perhaps even just as much as explanation and prediction have traditionally been taken to be. Just as nothing can really be said about a theory's truth or reference without linking the theory to a specific interpretation of the relevant language given by some model of the theory, explaining something means at some ('deep') level describing certain aspects of that thing. Definitions have to terminate at undefined terms, and the deduction of sentences of the theory has to start at unproven axioms.

Cartwright has a valid point in emphasising the role of phenomenological laws against the overwhelming philosophical attention that fundamental laws have been getting -- and in certain cases to a certain extent, still get -- but my account differs from hers, because I introduce the role of models from a different angle than she does. My approach, although making much of the role of fundamental laws in the scientific process, is enough in the semantic (non-statement) tradition to find her anti-realism towards fundamental laws too limiting. Unfortunately, she has few kind words to say to supporters of the se-mantical approach to scientific theories: 'On the semantic view, theories are just collections of models; this view offers then a modern Japanese-style automated version of the covering-law account that does away even with the midwife [of deduction]' (Cartwright Shomar & Suarez 1995: 139). I agree that the non-statement elimination of the theory as a linguistic expression is misguided, and that is why in my approach I stress the role of theory as much as I do the role of models. Theories (or fundamental laws) do indeed, in a certain sense, aim to 'state the facts in a more general way so as to make claims about a variety of different circumstances' (Cartwright 1983: 103). But, I see them as a crucial link in the chain of scientific progress, and I stress that it is mainly thanks to their general nature in the above sense, that they are a link in the first place.

The main reasons for Cartwright's antirealism about fundamental laws can be summarised as follows:

-- She believes that the fundamental laws of science (physics) do not describe the behaviour of the objects in their domain, rather they 'provide mathematical frame-works into which, by various devices some phenomena of the world can be fitted' (Chalmers 1987: 84). In other words, to give a fundamental theoretical account of an object is to 'fit' that object into the mathematical framework of the theory (see Chalmers 1987: 84, 85 for examples) -- which means that the laws explain idealised versions of the behaviour of real phenomena by some underlying (usually mathematical) mechanism.

-- She takes the underdetermination of theory by data as proof for the anti-realist nature of fundamental laws (she offers the example of radiative damping as an example, see Chalmers's discussion of this example in Chalmers 1987: 86). She claims to successfully address the underdetermination problem however by showing the real nature of capacities in her later works. (I 'solve' the 'problem' of underdetermination by simply incorporating it into the structure of the scientific process.)

-- She emphasises the logical gap between fundamental theoretical descriptions and adequate phenomenological descriptions of real situations by pointing to the imprecisions in mathematical expressions of theoretical descriptions (she offers the quantum mechanical treatment of the Lamb-shift as supporting evidence for this claim -- see a discussion of this example in Chalmers 1987: 86) -- instead of stressing the interpretable nature of these kinds of expressions and examining the possibility of them having 'links' with reality via models.

Chalmers (1987: 87) writes: 'Cartwright takes on an anti-realist stance with regard to fundamental laws, then, because the situations described by them are too simple and artificial to correspond to real world situations [no description of real objects], because adequate descriptions of the latter cannot in general be logically deduced from fundamental laws in conjunction with initial conditions [against the covering-law model], and because physicists frequently employ fundamental laws in diverse ways to offer different descriptions of the one real world situation [underdetermination of theory by data].'

2.3 The role of models in science and Cartwright's 'simulacrum' account of science

Traditionally, according to the statement approach, in philosophy of science a theory is taken as consisting of two parts:

-- internal principles (the 'core' or basic content of the theory expressed in some theoretical language), and

-- bridge principles (links or procedures 'giving meaning' to the theoretical terms in the theory's language by relating these terms to phenomena in the 'external world'). A problem that has been worrying Cartwright -- and which occupies any philosopher concerned with dealing with the intricacies of realism, maybe especially those working from a model-theoretic point of view -- is, very simply put, that the presence or necessity of these 'bridge principles' of an indirect and complex nature, however they are interpreted (in terms of models, mathematical functions, both, or something entirely different), implies somehow that the theory[13] itself has very little to say about the real phenomena the bridge principles are supposed to link it to.

First on her mind in How the laws of physics lie (1983,chapter 8), is to make clear whether having as few as possible bridge principles, should hold a promise of high explanatory power. I suppose the reasoning behind this claim is that the fewer bridge principles a theory needs the less 'fundamental' -- in the sense of falsity -- its fundamental laws. She (1983: 143) claims that, if members of some research community want to be able to work together, some way has to be found in which to limit the kinds of models that can be used to describe real phenomena, because, given the complex and rich nature of these objects (the phenomena that have to be described) a variety of models might describe the same phenomena. The anti-realist implication of this is, I agree with her (Cartwright 1983:145), not to be argued against by some belief in only a very small number of basic interactions in nature, but rather, I claim, by a belief in the very nature of the link between models of a theory and some aspect of reality.

I do not think though that these links are determined solely by traditional bridge principles, because the notion of bridge principles too has too much of a universal air about it. I am more comfortable with the more 'natural' limitation on models provided by the aims, background information, equipment, and training of the specific scientific community in question, as well as the satisfaction functions operating between theories and their models that set out the specific boundaries of the content of models in the first place. I am not entirely convinced though, that Cartwright would completely agree with that, given her anti-realist interpretation of the question of underdetermination, as well as her repeated efforts to show that terms in a theory have nothing to say about any aspect of reality, but can only explain the idealised behaviour of objects in models of the theory, thus effectively still severing the realist link.

She (1983:147-150) does however distinguish two senses of the notion of the 'realistic' nature of a model:

-- The first sense has to do with the relation between a model and reality (some aspect of reality, I would say). In this sense, a model is realistic if it gives an accurate description ('picture') of the aspect of reality ('situation') being modelled. In other words it will have to describe the structure and actual behaviour of the real system.

-- The second sense has to do with the relation between the model and 'the mathematics' [in her terms (1983: 150)], which is the relation between the model and the theory in my terms. According to her a fundamental theory determines criteria for what counts as explanations, and, in these terms -- relative to those criteria -- a model will be realistic if it explains the mathematical representation -- i.e. if it realises the theory.[14]

She (Cartwright 1983: 151ff., chapter 8) offers her 'simulacrum account of explanation' in the place of the covering law model of explanation. According to her (Cartwright 1983:151) the covering law model requires the way in which phenomena are modelled to be realistic in both senses because it views a phenomenon to be explained if it has been derived from some fundamental law. Cartwright, however, primarily wants to show that -and how -- fundamental laws logically summarise and classify (as mentioned before, in Duhem's tradition) groups of phenomenological (experimental) laws without aiming to explain them. She (ibid.: 152) writes:

I have been arguing ... that the vast majority of successful treatments [of phenomena] in physics are not realistic. They are not realistic in the first sense of picturing phenomena in an accurate way; and even in the second sense, too much realism may be a stop to explanatory power, since the use of 'phenomenological' [still abstract] terms rather than more detailed 'causal' constructions may allow us more readily to deploy known solutions with understood characteristics and thereby to extend the scope of our theory [although this will not necessarily lead to a better understanding of the actual aspect of reality the fundamental laws are 'about'].

Cartwright's problem with fundamental laws is that they are laws about distinct (separate) aspects of objects in reality and their behaviour --or, in her most recent terms, about distinct causes and their separate effects -- while, in the real world, these things actually occur only in combination with other aspects of these or even other objects. And, moreover, these combinations change quite often and occur very seldom according to some regular kind of pattern, because of the variety of factors involved. Cartwright's problem is that '[e]ven if these regularities did hold ceteris paribus -- or, other things being equal -that would have no bearing on the far more common case where other things are not equal' (Cartwright 1989b: 177). Again, this interpretation of the nature of fundamental laws is not really what is at issue here, the problem or challenge really is to find a kind of view that can accommodate these fundamental features of scientific theories and still offer a realist interpretation of the scientific process. A model-theoretic approach such as the one that I am offering holds this promise without even having to specify whether one sees objects and activities in reality in terms of causes and their separate effects (as Cartwright seems to be doing nowadays) or not, since both accounts can be accommodated.

Now, if giving a fundamental theoretical account of a certain object means fitting it into the mathematical framework of the theory under discussion, and, if this is what fundamental laws ultimately do, as Cartwright claims -- 'To explain a phenomenon is to find a model that fits it into the basic framework of the theory and that thus allows us to derive analogues for the messy and complicated phenomenological laws that are true of it' (Cartwright 1983: 152) -- the obvious question to me is why this should result in false fundamental laws? The answer lies in Cartwright's notion of models and their role in the scientific process.

She (ibid.) points out that models help us to 'see' the relevant phenomenon through the mathematical framework of the theory, but stresses that different problems will have different emphases on different aspects of that framework. This, to me, implies that different models can -- and should -- only be evaluated according to the different aims guiding their construction. And that is, in my view, why she calls her account of explanation a 'simulacrum' account.[15] She (ibid.) writes:

This is just what I have been urging that models in physics are like ... A model is a work of fiction. Some properties ascribed to objects in the model will be genuine properties of the objects modelled, but others will be merely properties of convenience ... Not all properties of convenience will be real ones. There are the obvious idealisations of physics -- infinite potentials, zero time correlations, perfectly rigid rods, and frictionless planes. But it would be a mistake to think entirely in terms of idealisations -- of properties which we conceive as limiting cases, to which we can approach closer and closer in reality. For some properties are not even approached in reality. They are pure fictions. I would want to argue that the probability distributions of classical statistical mechanics are an example ... It is better, I think, to see these distributions as fictions, fictions that have a powerful organising role in any case and that will not mislead us too much even should we take them to be real in the simple cases.[16]

However, '[b]eing explanatory in this sense, that is, being useful in many different contexts, requires the theory to neglect the special differences between the contexts ... Therefore, the theory cannot be true of any of these real situations; it can give a correct description only of the behaviour of objects in highly idealised contexts or models. The model contains the distortions and idealisations that are necessary to make a theory bear on a real situation. Real objects and their behaviour are too varied, too complex, too messy to be treated faithfully by theories of great generality; that's why we need models to mediate between theory and phenomenon' (Rueger & Sharp 1996: 95). The important thing that both Rueger and Sharp, and also Cartwright, seem to overlook is that scientists never examine any real system in all its messiness. That simply is not -- and has never been -- the aim of science. It is however part of the task of philosophy of science to show how such abstract and general theories may be said to be (or not to be) about aspects of this complex reality, and yes, that is where studies of the internal structure of models of theories and the various relations into which they enter come in. The mediation of models between theory and reality is however misunderstood if it is taken to offer the means by which science can indeed be said to say something about some real object in all its varied complexity.

And, it seems that (for Cartwright) it is because of the simulacrum nature of models that bridging relations can only hold ceteris paribus. This shows the structural error in her account as far as ceteris peribus conditions are concerned. The view that portrays these conditions as some kind of ingenious device cunningly designed by naive realists or staunch fundamentalists to 'save theories from point-to-point testing' (Rueger & Sharp, 1996: 103)[17] is completely misguided. First, however, as far as bridge principles are concerned -- they do not hold ceteris peribus. There is no absolute set of rules describing these kinds of correspondence relation. Rather these rules hold with respect to a certain model within whose boundaries the theory is true. The best that can be said about 'bridge principles' in my terms is that perhaps one can speak of a set of bridging 'procedures' or 'links' that extracts data from the relevant real system relative to a specific empirical context, and then injects these data, as an empirical model into the model under consideration.

The theory holds ceteris peribus yes, but not in Cartwright's sense of the word. In my terms, to say that a theory 'holds' means, per definition, that it holds (is 'true') in a particular one of its models. To say now that it holds 'ceteris paribus' adds nothing to simply saying that it is true. Moreover, there is nothing else about which it can be stipulated that it stays the same -- everything is given in the model. Ceteris paribus clauses seem in Cartwright's terms to play a more and more important role the further away one moves from fundamental laws. In model-theoretic terms, however, they are necessary only at the level of scientific theories or linguistic systems, and become less and less active the closer to reality one moves. I claim -- see also Ruttkamp (1999) -- that they are suspended in their generality as soon as the theory in question is interpreted in specific models, rather than activated. The idealised nature of conceptual -- and even empirical models -- is not the result of specific ceteris peribus clauses, but indeed simply true to the nature of science. No real system can ever be examined, represented, explained, or described in its full complexity. That is simply not science's function.

Following Duhem (Duhem 1914: 7), Cartwright (1983: 96) considers the notion of scientific explanation in terms of description in the sense that explaining a set of phenomena means giving a physical theory of them, 'a physical theory in Duhem's sense, one that summarises ... and logically classifies them' (Cartwright 1983: 96). Rueger and Sharp (1996: 95) refer to the problem she has with the covering law account of explanation as the 'unsoundness argument' and set it out as follows: 'If ... phenomenological laws could be soundly derived from more fundamental laws as the traditional [covering law] view would have it, then any successful comparison of the phenomenological consequences of the theory with the observations would count unproblematically as inductive support for the theory. Confirmation would flow upwards from the phenomenological level to the fundamental level. This flow, however, is staunched ... because phenomenological laws typically cannot be soundly deduced from more fundamental theories. To derive the former we usually need assumptions [ceteris paribus clauses] which are either false (distorted representations of the situation of application) or which contradict the fundamental laws themselves. Inductive support cannot, therefore, be transmitted.'

Claiming that phenomenological laws cannot 'typically' be deduced from fundamental ones, is perhaps jumping the gun a bit. Is it not the case that Kepler's laws can be deduced from Newton's in a very sound way? Moreover the ceteris paribus clauses and other additional assumptions needed to validate the fundamental laws are suspended when models are constructed of some theory -- as remarked above -- and thus these clauses become more and more concretely realised as they set the boundaries for the truth of the theory, i.e. the clauses themselves (e.g. 'no other forces act differentially on components of the system') become realised, i.e. true in the relevant models. Thus it is rather unclear how they can be understood to 'contradict' the fundamental laws themselves (which are also true in these models).[18] These ceteris paribus conditions or clauses will usually be incorporated into the formulation of the law explicitly (as when stating that Hooke's law holds as long as the elastic limit has not been exceeded), or else implicitly and tacitly by common understanding.

So, it seems that to Cartwright, in order to fit some phenomenon into the mathematical framework of some theory, a model of that phenomenon 'which re-describes it in terms which are amendable to mathematical theoretical treatment' (Chalmers 1993: 200) has to be constructed. 'Before we can apply the abstract concepts of basic theory -- assign a quantum field, a tensor, a Hamiltonian, ... or write down a force function -- we must first produce a model of the situation in terms the theory can handle' (Cartwright 1994b: 282). I do agree with this, but not with Cartwright's conclusion that models are thus (in her terms) merely devices used to fit phenomena into the theoretical frameworks of theories which in their turn classify and organise groups of phenomena -- '... the point of the kind of models I'm interested in is to bring the phenomenon under the equations of the theory' (Cartwright 1983: 157). This is not the only role models have. They do have this function, but it is by virtue of this very function that they offer ways to link the theory with aspects of reality.

Cartwright (1983: 160) wants to focus on what 'actually happens in concrete situations, whether these situations involve theoretical entities or not, and how these differ from what would happen if even the best of our fundamental laws played out their consequences rigorously'. Moreover, she stresses (1994b: 292) that Hacking's point in Representing and intervening (Hacking, 1983) is not merely that theoretical entities exist if we can use or manipulate them or intervene in their behaviour, but far more important, that '... it must be the case that we understand their behaviour very well if we are able to get them to do what we want.'

She (1994b: 292) concludes then from this that such an understanding should be taken as enough evidence for '... the truth of some very concrete, context-constrained claims, the claims we use to describe their [the theoretical entities' under discussion] behaviour and control them.' The important fact here remains that (ibid.) '... in all these cases of precise control, we build our circumstances to fit our models ... [in other words] that does not show that it must be possible to tailor our models to fit every circumstance. ... some circumstances resemble the models we have; other do not. And it is just the point of scientific activity to build models that get in, under cover of the laws in question, all and only those circumstances that the laws govern [try to describe]'. She already says as much in How the laws of physics lie (1983: 157), where she clearly states yet again that she is arguing, not that the generality and exceptionlessness of fundamental laws are proof of their being laws of nature, but precisely the opposite, namely that these features are not real, that is, fundamental theories simply appear to have these characteristics. This is the result of an over-focusing on what she calls the 'second stage of theory entry', in the sense that the fundamental laws may be true of the objects in the model (but not of objects in reality), 'but that is because the models are constructed that way ... when we present a model of a phenomenon, we prepare the description of the phenomenon in just the right way to make a law apply to it' (ibid.).

I suppose we do, in a sense, try to 'make' the circumstances resemble the models we have, at the stage when the intended model is being constructed, insofar as the aims of the scientists concerned, the available mathematics, and so on, will define the nature of the models at that level. Also, perhaps at the stage of the scientific process where empirical adequacy is tested these kinds of considerations will have some role to play. In a sense, one's view on this will depend on the role one takes background information to play in the process of science -- are scientists trying to find a 'match' between some model and an aspect of reality, or an aspect of reality-as-they-interpret-it according to their established body of knowledge, future research aims, and training? A last remark on saving the phenomena: the construction of models means precisely the preparation of the conceptual description of a certain object in reality to 'make' some law apply. However, no a priori rules exist -- as pointed out above in terms of bridge principles -- that govern the construction of models, and therefore, obviously scientists cannot guarantee that these deliberate actions will indeed end in descriptions of real phenomena, I am not sure though whether philosophy of science should demand such guarantees. I think that being able to describe in general the way in which theories are linked to aspects of reality, and so to set out the way in which (the process of) science should be interpreted, should be enough. The guarantees at issue here are at the level where relations between aspects of reality and models are evaluated, and that is solely an empirical issue, and thus falls in the domain of science itself.

This also has to do, as Cartwright (1983: 158) points out, with the anti-realist implications of her account, because different -- possibly incompatible -- models are used for different purposes, and so there is no one-to-one matching between models and the real situations being studied. And, obviously then, that is why she cannot see how the laws governing the models could be presumed to apply to real situations. According to Rueger and Sharp (1996: 107) fundamental theories cannot receive confirmation from successful predictions, based on models of theories, precisely because these models themselves are no mirror-images of reality but rather distort reality -- because of their ideal (or context-dependent or) aim-orientated emphasis-sensitive nature. Then one may well ask how, if there is no one-to-one mapping between a situation studied and a model, and a fundamental theory is true only in a model, can it be claimed that a scientific theory has something -- or anything for that matter -- to say about reality? Of course, this makes sense, but I am claiming that these issues can be viewed and set out a little differently:

-- It is true that there is no guarantee that some theory will be applicable to a specific aspect of reality before that theory has been interpreted and this interpretation (model) linked empirically to some system in reality.

-- But, what does it mean to 'place a phenomenon in the mathematical framework of a theory'? Does that not already imply that the model was constructed -- apart from giving a true interpretation of the theory --also with some kind of real phenomenon in mind? Yes, indeed.

In other words, the model-theoretic view of science implies that scientific theories can -- and do -- say something about reality (because of the way the reality-model-theory link is interpreted), but acknowledges that it is not possible beforehand to determine or claim that a certain theory will definitely be applicable to a certain aspect of reality and to no other. The reasons for this are very important since they explain the 'vagueness' of any kind of description of 'bridge procedures' between theories, models, and real systems. The first reason lies in the 'freedom' involved in the construction of models in the sense of model construction being influenced, amongst other things, by the scientific community's aims with the theory under discussion. Entangled with this reason, the second reason lies in the specific nature of the real system that the theory in the end -- via its conceptual and empirical models -- may be said to be 'about'.

Cartwright does see this, in a way, since she (Cartwright 1994c: 293) acknowledges that the link between (an aspect of) reality and some model is 'a matter for hard scientific investigation, not a priori metaphysics'. And goes on (ibid.) to explain that '[t]hat is the reason I am so concerned with the successes and failures of basic science in treating large varieties of situations differing as much as possible from our experimental arrangements'. This is a valid point, but only worrying if one believes that science is mirroring total Nature by discovering its ultimate laws. Otherwise, this simply is a feature of the way science is being done.

So, although models are the source of the 'distortions' and idealisations that prohibit theories (and their fundamental laws) to say anything directly about any real situation, model-theorists like me cannot see the scientific process continuing without them. But, that simply brings us back to Cartwright's claim that laws that explain are not necessarily true. Rueger and Sharp again (1996: 96): 'There is thus a trade-off between a theory's explanatory power and its (potential) truth: the more efficient a theory is in explaining or organising a large variety of different phenomena, the less can it be true or state the facts.' As Cartwright (1983: 72, 73) herself has been stressing since How the laws of physics lie (1983):

[i]f we state the fundamental laws as laws about what happens when only a single cause is at work, then we can suppose the law to provide a true description. The problem arises when we try to take the law and use it to explain the very different things which happen when several causes are at work. This is the point of 'The truth doesn't explain much'. There is no difficulty in writing down laws which we suppose to be true: 'If there are no charges, no nuclear forces, ... then the force between two masses of size m and m' separated by distance r is Gmm'/r2'. We count this law true -- what it says will happen, does happen -- or at least happens to within a good approximation. But this law does not explain much. it is irrelevant to cases where there are electric and nuclear forces at work.

Well, yes, of course, if one believes in an absolute notion of truth, and if one believes that this absolute truth in science is about specific individual situations in their uniqueness. That is the whole point! It is, I believe however, possible to speak only of theories being true-in-some-model and not of theories being true qua nothing else, i.e. absolutely or 'universally' true. Cartwright (1986) does point out the fact that 'truth' does not mean 'universal', but almost everywhere else she continues to contradict herself. I think the reason for these ambiguities may lie in her interpretation of ceteris paribus clauses or conditions. She views these clauses as playing an important role in the explanatory power of the fundamental laws, in the sense that they determine what kinds of explanation are permissible because they lay down or record, in a sense, the nature of the abstractions from real situations made by the theory and its fundamental laws. It seems then that in this sense the conditions laid down by these clauses also determine the nature of the models of the theory in question in their function as part of the concretising mechanisms of science, such that they 'adapt' contexts to 'fit' the laws explaining the behaviour of the objects found within that particular context.

In model-theoretic terms the role -- if any -- of ceteris paribus clauses is somewhat different -- as I have already pointed out. First, they are only of importance -- if at all -- as part of the linguistic expression of some theory. Cartwright's reconstruction of Newton's gravitational law (see above quote) -- i.e. 'If there are no ...' is simply wrong. The law states rather that 'The gravitational force between two masses ...' without exception. The law is still absolutely and totally relevant when there are (also) other forces present! What she implies with her reconstruction of the law is simply not the case. The gravitational force is still there in cases where electric and nuclear forces are at work. We take the (vector) sum of all the forces on a particular body to see how it will behave. I might also remark here that thus, a remark such as Rueger and Sharp's (1996: 96) conclusion concerning Cartwright's thoughts on this issue: 'Because the complexity of the behaviour of real objects is produced by the interaction of hopelessly many (causal) factors, varying from context to context, a simple, highly explanatory theory which inevitably ignores most of these factors has (almost a priori) no chance of ever providing a true description of a [real] situation', is equally wrong. We do have theories and models about how different causal factors combine when acting on the same system in reality. A simple example is given by the vector addition of speeds, accelerations, and forces. The only kind of reconstruction of Newton's gravitational law that mentions other forces and factors should then simply be something like this: 'Even if there are electric charges, nuclear forces, the sun shining on them, rain falling on them, ..., then still, everywhere, under all possible circumstances: the gravitational force between two masses ...'.

Ceteris paribus clauses, where necessary, form part of (a complete formulation of) the law and are not even really extraneous conditions. A scientific theory makes statements concerning the nature and behaviour of a certain phenomenon, or a group of phenomena, in some real system(s). These statements are 'sweeping' precisely because they have to cover all phenomena, in whatever context they may occur, that may exhibit the features of the ones described in the theory, and not only specific ones. In that sense, the formulation and application of any fundamental law is never ceteris paribus. Rather than saying 'if all possible influencing factors not explicitly mentioned are absent or neutralised, then ...', a fundamental law will typically say 'even if all possible other factors influence the system in all possible ways, then still ...'. Of course, the complete formulations of many (fundamental and phenomenological) laws are conditional. Remember again Hooke's law: 'If the elastic limit has not been exceeded, then ...'.

The specific clauses that Cartwright has in mind, are thus much rather part of the content of the (conditional) law expressed by the theory in question, than conditions for the law's applicability. Ironically enough, if these clauses are conditions (in Cartwright's or in the logical sense) it implies that they can be negated -- i.e. not be satisfied -- and the conditional formulation of the law can still apply. (It is a case of simple logical equivalence that cp arrow right law left and right arrow cp *[this character cannot be represented in ASCII text] law). One reason why Cartwright sees ceteris paribus conditions as separate from the law and will probably not accept my incorporation of them into a conditional formulation of the law, is the following. Her ceteris paribus conditions may involve influencing factors (for the physical system under consideration) for which there are not even terms in the language of the theory. Look again at her reconstruction of Newton's law of gravity: it drags in ceteris paribus conditions involving electricity and nuclear forces, about which the language of Newton's theory cannot even talk. So it is impossible to formulate her reconstruction of the law as a conditional sentence in the language of Newton's theory. What we have here is another manifestation of Cartwright's aim (which is again, not the aim of science) to scientifically and truthfully encompass a system in all its limitless complexity and interrelatedness. Maybe her 'patchwork' picture of the world is her loophole to investigate what would otherwise be inexorably entailed by her view, namely that there is only one possible object for science: the whole cosmos.

The conceptual models of theories determine the idealised context(s) --i.e. (truth) conditions -- within which the theory will be true, simply because that is what an interpretation of a linguistic expression does. The idealised nature of models is not 'ideal' by virtue of any ceteris paribus conditions added to the theory. As I have stated before, ceteris paribus clauses are suspended (in their generality) when the theory is interpreted in its model(s) -- in the same way in which the law's generality is suspended, as is now evident form the above. Perhaps Cartwright misinterprets the reason for the fact that truth and universality are different concepts. She (Cartwright 1997: 167) claims that 'To say the laws of physics are true ceteris paribus, is not to deny that they are true. They are just not entirely sovereign.' Well, model-theoretically, theories are indeed not simply true, whether conditional, ceteris paribus, or not, but they are sovereign. Theories can only be true in their models, regardless of how many -- if any --ceteris paribus clauses form part of their formulation. Moreover it is exactly because they are sovereign, in the sense of being formulated for all possible circumstances satisfying their terms, that they have any chance at all to be true (in their models).

Now, let us look again at Cartwright's distinction between universality and truth. She (Cartwright 1989: 162) claims that theories should not be taken as summaries of laws about observable entities, because theoretical entities are not needed to explain the behaviour of observable entities, but are rather necessary to systematise observable behaviour -- as pointed out before. Very much in the constructivist tradition, she then goes on to stress that theories are never universally applicable, but their domain (the limit of their applicability) is determined by making use of the theory and its concepts themselves. This is interesting in the sense that, as far as the constructivists are concerned, it makes it impossible to ever move to a meta-level for any reason -- like evaluating the scientific content of a theory. What Cartwright wants to show, I think, is also along these lines, although she is, more specifically, aiming to show that truth and universality do not necessarily imply each other. This, of course, is entirely in line with a model-theoretic interpretation of these notions. The notion of universality, in model-theoretic terms, is not applicable when it comes to science, and the notion of truth, though still important, is ultimately based on the notion of context-dependent empirical adequacy.

All of this illustrates the necessity of the interpretative role models play in science. In an article entitled The toolbox of nature (1995), that she co-authored with T. Shomar and M. Suarez, Cartwright again claims that '[r]epresentations of phenomena must be constructed and theory is one of the many tools we use ...' (Cartwright, Shomar, Suarez 1995: 139). She goes on: 'I want to urge that fundamental theory represents nothing and there is nothing for it to represent. There are only real things and the real ways they behave. And these are represented by models, models constructed with the aid of all the knowledge and technique and tricks and devices we have' (ibid.: 140). I have no quarrel with these remarks. That is exactly what I am trying to show, in the sense that I want to establish the fundamentally 'constructed' nature of science. But, although in a sense I urge the 'constructedness' of theories just as much as the 'constructedness' of models, I view the role of theories and their abstract and general nature as a central part of what science is, while Cartwright more often than not sounds as if she would much rather do without theories, although she of course does acknowledge their organising features. And the reason for that is, I think, that she focuses too much on the spurious theory-reality link at the cost of the construction of the theory-model-reality link.

I see the role of theories within the whole representation process as more meaningful and maybe more useful -- than she sometimes seems to do. To me models are constructs, that is (conceptual, i.e. mathematical) structures that do not have to be primarily linguistic, while theories are primarily linguistic entities (sets of sentences of some appropriate language, that -- among other things -- describe models in which those sentences are true). In my view therefore, theories are absolutely essential to science, because they formulate the conceptual content of the models, make this content amenable to deduction and computation, and communicate this content to other scientists. The main means of communication in science still is language (together with diagrams, physical models, demonstrations, films, and so on).

However, the arguments Cartwright sets out against the 'theory-driven' (Cartwright Shomar, Suarez, 1995) approach to models fit well into my model-theoretic account of science. It is true that models are not simply deductions from theories, nor is it possible for any kind of nomological (law-like, universal, always-the-same) link between theories, models, and reality to exist. Briefly the 'theory-driven' approach has to do mostly with that old realist favourite: approximation. However, this kind of account of science and its theories (in terms of approximations) seems somehow to imply a kind of apriori-ness about models, in the sense that each new (approximating) model will always definitely bring us closer to 'the truth' than the previous one. I claim that in general no such guarantees can be given, and I think that Cartwright would agree. The advocates of the theory-driven view --according to her (ibid.: 148) -- see the construction of a new approximating model as one of the following activities:

-- certain correction factors that are strongly motivated from the perspective of physical theory -- are introduced into the theory's equations;

-- or, some other model is found for the (revised) theory which implies the revision of some of the standing physical assumptions.[19]

Cartwright and company (ibid.) are advocating a much more free notion of models in the sense that (phenomenological) model construction should be viewed as far more independent -- in method and aim -- from the theory in question. In a sense, at least as far as the limited power of the formulators of the original theory over its resulting logically possible interpretative models is concerned, it might seem as if that is what I am advocating too. The semantic link between theory and model in my approach, however, has to remain. Establishing this link is simply not applicable or relevant in a non-statement framework, given their disregard of scientific theories as linguistic entities (although they of course do not deny that models are interpretations in which a given theory is true).

2.4 Nature's capacities causally explained

Cartwright amends her simulacrum account of explanation in her book entitled The capacities of nature and their measurement (1989b) by arguing that causal claims should play a central role in the explanations offered by science. This is an obvious continuation of her attack on the Humean characterisation of science (according to which explaining a phenomenon means showing it to be an instance of a general law or regularity), which she keeps focused on arguments against the Humean attempt to reduce causal concepts to law-like ones (and finally to reduce these to regularities, or statements of association). She offers, instead, a metaphysics of enduring causal capacities.[20] She

(1995c: 292) claims that '[l]aws in the conventional regularity sense ... must be constructed, and the knowledge that aids this construction is not itself again a report of some actual or possible regularities. It is rather knowledge about the capacities of [nature] and what these capacities can do if assembled and regulated in appropriate ways'.

Cartwright and John Dupre in their article entitled 'Probability and causality: Why Hume and indeterminism don't mix' (1988: 521) see events (constant conjunctions) in a completely un-Humean way: '... events and things have causal capacities: in virtue of the properties they possess, they have the power to bring about other events or states. ... The Humean tradition downplays capacities, and conceives of them as no more than misleading ways of referring to law-like regularities. We [Cartwright and Dupre] want to reverse this idea: it is better to think of law-like regularities as misleading ways of referring to the exercise of capacities.' Causality, in the Humean tradition, is depicted in terms of relations between events, which holds, as i have already pointed out, in virtue of the regular association of the empirically distinguishable properties of these events. Dupre and Cartwright (1988: 521), however, point out that, on the other hand, no 'right' sort of connections exist between capacities and properties of events. 'Capacities are carried by properties. That is, you cannot have the capacity without having one of the right properties. But the same property can carry mixed capacities, and so the true complexity of the situation cannot be revealed by the association of properties.' And, moreover, since '... at any stage in [an] inquiry, there are always alternative sets of capacity that could account for the statistical data [under consideration]' (Cartwright & Dupre 1988: 522), it is not possible -- contrary to Hume -- to find statistical data that can 'settle' the truth of probable cases of regularity. In line with her empiricist's sympathies, Cartwright wishes to show that capacities can, however, indeed, be measured.[21] She lengthily discusses the use of probabilities in this regard in quite a few of her more recent articles, as well as in The capacities of nature and their measurement (1989b).[22]

Cartwright gives (1989b: 226-227) three main arguments for the real existence of capacities: (In the scope of this article I am primarily interested in the second argument, given its relation to realist considerations.)

-- She bases the first argument on the nature of the composition of causes and the fact that scientific explanations and predictions involve causes as well as their behaviour.

-- The second argument is based on the problem of the exportability of information or knowledge, that is, the fact that information gathered in one situation can be applied in a completely different situation. This is essentially the issue that Poincare referred to as the problem of 'transduction'.

-- Thirdly, she offers an argument that is a counter-attack on the Humean tendency to 'modalise away' capacities, and here she concentrates on the problems of interaction between causes themselves and between causes and capacities, and also on the 'duality' of capacities, which refers to the problem of controlling multiple capacities associable with one and the same feature of nature.

One of the most important reasons why Cartwright insists on the reality of causes is, as Chalmers (1993: 199, 200) points out, to '... overcome the inability of the orthodox Humean view of laws as constant conjunctions to accommodate the asymmetries that exist between the phenomena constantly conjoined..."I agree with Hacking', writes Cartwright (1983: 3), 'that when we can manipulate our theoretical entities in fine and detailed ways to intervene in other processes, then we have the best evidence possible for our claims about what they can and cannot do'. So, she offers the fact that scientists can 'successfully intervene' in nature by 'manipulating causes' as an argument for the existence of those causes. This point is echoed by Ernan McMullin (Cartwright 1989: 185): 'The unordered world of nature is a tangle of causal lines; there is no hope of a "firm science" unless one can somehow simplify the tangle by eliminating or otherwise neutralising, the causal lines which impede, or complicate, the action of the factors one is trying to sort out ...'.

However, as she points out (Cartwright 1991a: 9), '[t]he punchline of course is that the fundamental laws of physics may not be so fundamental either. ... By choice and arrangement of materials and either by intensive shielding or very heavy over-determination, we create special environments which hold fixed the principal effective parts. We may in this way arrive at very precise and reliable regularities without in any way grasping the true form of what is going on.' Cartwright, of course, is here referring to her notion of a 'patchwork of laws' -- a world of '... tens of thousands of patches, cut up in no particularly logical way, exhibiting tens of thousands of different regularities of countless different forms ...' (Cartwright, 1994b), which leaves open the possibility of reality being completely unordered. Two brief related remarks on this last statement: I do not see how the acceptance of a notion as metaphysical as the notion of capacities, can solve any of Cartwright's anti-realist worries. On the other hand, the exact nature of the things in reality that scientific theories may be 'about' --whether referred to simply as the activities or behaviour of real phenomena, as the mechanisms of reality, the capacities of nature, or whatever else -- is irrelevant to the successful application of the model-theoretic model of science that I offer.

The primary problem that she wants to solve with her notion of capacities is indeed whether -- and to what extent -- the laws of physics that are true in certain situations -- that is in the 'highly contrived environments of a laboratory or inside the housing of a modern technological device' (Cartwright 1994b: 281, 282) -- carry across to 'systems, even systems of very much the same kind, in different and less regulated settings' (ibid.). She says 'It]he overall programme I want to urge is a careful and detailed philosophical story of the evidence about the boundaries of relevance ... for any of our ... fundamental laws. We have to allow for the possibility that they are true but not universal; exact but limited in range' (Cartwright 1994c: 293).

She sets out a hierarchy of laws in Nature's capacities and their measurement (1989b) and also in her Precis of 'Nature's capacities and their measurement' (1995a), in terms of generality -- or modality, because the claims at the highest two levels are universal in time and space and they support counterfactuals, license inferences, and so on. Her classification looks roughly as follows:

-- At the highest level there are 'capacity claims' (Cartwright 1989: 228), which are to be taken as being general causal claims. These are statements which associate capacities with properties. These claims summarise the range of outcomes that some system can cause. She says about these most general kinds of causal statement that 'I maintain that the most general causal claims -- like "aspirins relieve headaches" or "electromagnetic forces cause motions perpendicular to the line of action" -- are best rendered as ascriptions of capacity' (Cartwright 1989: 141). So, for instance, aspirins, because of being just that, can cure headaches. The phrase 'because of being aspirins' indicates that this claim expresses a fact about a special property, namely that the property of being an aspirin carries the capacity of relieving headaches.

-- At the middle level there are 'causal claims' that give phenomenological (actual) content to the capacity claims -- they are laws about 'what singular causings occur in what circumstances what percentage of the time' (Cartwright 1989: 228). In other words, they are more specific than capacity claims and they describe -- usually probabilistically -- what causal claims obtain in some given specific situation. And, thus they can -- other than capacity claims -- be used in a mechanical way to make predictions. Also, they can be established inductively, as long as the situation they are prescribing for, or predicting of, is the situation they are describing in the first place. 'They describe what would happen were the situation like that. But by their very nature they do not describe what would happen were the situation different' -(Cartwright 1995a: 154) -- their weakness lies in their strength.

-- At the lowest level of abstraction, at the level of the real, that is, there are singular causal claims. 'Nature's capacities argues that it is not possible to characterise correctly the relation between probabilities and causal laws without referring to singular causal facts' (Cartwright 1995a: 153). That is actually one of the main reasons why this book is still very important from a model-theoretic point of view. Because, if it is true that, as she claims now, fundamental laws are about the capacities of nature, as Chalmers (Chalmers 1993: 201) points out, then they cannot describe sequences of events as well, and therefore they cannot anymore be taken to lie in the sense of How the laws of physics lie (1983).

The fact that capacities enable us to carry over information gathered in one set of circumstances to another, is the reason why capacity claims are not simply 'higher levels of modality, but instead must be taken as ascriptions of something real' (Cartwright 1989: 158). I will briefly discuss what she has to say on this point, as it points to her realist tendencies with regard to capacities, while she remains anti-realist with regard to fundamental (abstract) laws. She claims that independent evidence that some interaction between certain variables and no others -or at a certain level and no other -- is occurring, should be found. She illustrates the value of this with the example of chemistry (Cartwright 1989: 165): 'One does not say the acid and the base interact because they behave differently together from the way they behave separately; rather we [should see that we] understand already a good deal about how the separate capacities work and why they should interfere with each other in just the way they do.' Thus ceteris paribus conditions describe the model in which the fundamental ascriptions of capacity will be true. In a model-theoretic account no ceteris paribus clause has the power to describe a model of some theory. Rather models are determined -- amongst other things -- by the whole linguistic expression of the theory (including factors such as the nature of the language in which the set of sentences comprising the theory are formulated) as well as by some of the applicationary aims scientists have in mind for the theory. Cartwright's aim is to show that the regularities that are described by the (phenomenological) laws in the model are the consequences and not the sources of these models.

She sees (Cartwright 1995c: 290) 'natures [or capacities] as primary and behaviours, even very regular behaviours, as derivative. Regular behaviour derives from the repeated triggering of determinate systems whose nature stays fixed long enough to manifest itself in the resulting regularity'. Well, as pointed out before, in model-theoretic terms the (relevant) empirical model that 'drives' the final references of some theory [via its conceptual model(s)] to some real system is very much dependent both on the nature of the conceptual model of which it is a substructure, and on the nature of the system in reality it shows the theory to be 'about'.

The connection between causes and probabilities that causal claims make possible can be described in the following terms: a cause increases the probability of its effect (while obviously a preventative should lessen it) if all other causes are held fixed somehow. However, Cartwright argues (1989b, throughout chapter 3) that more important than holding the other causes and their forces of possible interference fixed, is that the operation or activity of all other capacities that may be at work, should be held fixed as well, whether they are attached to the cause being scrutinised or to the other separate causal factors. She (Cartwright 1989: 167) claims that only in this way, can facts about capacities be connected to facts about probabilities.

However, since ascriptions of capacities are found in fundamental laws, Cartwright has to say something more about the role of fundamental laws as ascriptions of capacity in the causal account of scientific explanation that she is working out. She (ibid.: 175) writes: '[m]ore important for my thesis, however, is not the fact that laws which are nearly true, albeit for particular situations and finite periods, are not fundamental [which is what Mill also points out], but rather that fundamental laws are not true, nor nearly true, nor true for the most part. That is because fundamental laws are laws about distinct "atomic" causes and their separate effects; but when causes occur in nature they occur, not separately, but in combination.' As pointed out before, these 'complex combinations' that we find in Nature are of no particular consequence to a model-theoretic realist at all. We have other laws that tell us how to combine effects -- for example, the addition of speeds (which by the way is different for Einstein than for Newton).

Now, Cartwright (1989b: 178) has been arguing all the while in Nature's capacities and their measurement (1989b) that scientific methodology and its application presupposes that these capacities (or tendencies, as Mill calls them, or propensities as Popper calls them) are real. In other words, the only way in which fundamental laws can be taken to say something about reality, is if they are viewed as ascriptions of capacity. Although this doesn't change their nature, they still lie (according to her) however, because they are still Aristotelian abstractions. But, now, at least, they can be interpreted in some kind of realist terms. I fail to see the need for this. Whether the function of the fundamental laws of nature is to assign stable capacities to specific causes (Cartwright 1989: 179) or not, does not really decide whether these laws may have something to say about reality or not. Rather, it is the ways in which specific models may be shown to be linked to certain aspects of reality that hold that promise.

But, if capacities are real (more than high-level Humean modalities), what becomes of the lower level modal laws, in particular, what becomes of Hume's laws of association? Cartwright (1989b: 181) concludes that Nature in no way presents us with them as 'given', but rather Nature selects the capacities of different entities and determines their interaction.

It is not the laws that are fundamental, but rather the capacities ... Whatever associations occur in nature arise as a consequence of the actions of these more fundamental capacities ... Nature, as it usually occurs, is a changing mix of different causes, coming and going, a stable pattern of associations can emerge only when the mix is pinned down over some period or in some place. Indeed, where is it that we really do see associations that have the kind of permanence that could entitle them to be called law-like? [Only in ancient astronomy or in science laboratories.] ... laws of association are in fact quite uncommon in nature, and should not be seen as fundamental to how it operates. They are only fundamental to us, for they are one of the principal tools that we can use to learn about nature's capacities.

In these terms then, the basic laws of physics are not laws about sequents of events (a la Hume) but laws about the capacities of nature. I have no quarrel with remarking that 'a stable pattern of association can emerge only when the mix is pinned down over some period or in some place', because that fits my description of the functions of models and the ways in which they interact with theories on the one hand, and with aspects of reality on the other hand. Keep in mind here too that the conditions in which capacities 'reveal themselves in their canonical behaviour are usually in no sense normal at all; [but it] ... requires the highly artificial and contrived environment of a laboratory to manifest them' (Cartwright 1989: 200).

Alan Chalmers (1993: 201) agrees with Cartwright that, if laws are supposed to describe capacities, then they cannot be taken to describe Humean sequences of events as well. But, then, fundamental laws do not really lie in the sense of How the laws of physics lie, because only laws taken in the sense of regularities could be said to lie in this sense (Chalmers 1993: 204). This is close to what is worrying me, although I do not necessarily see scientific theories in terms of discoveries of regularities, because it is simply not necessary to think in that way, given the descriptions of the nature of theories and models and their interaction in my model-theoretic system. Moreover, we all know that science has never aimed at describing reality in all its complexity and fullness. The main problem in realist philosophy of science has obviously always been, in a sense, to link the simplified versions of reality modelled by scientists to fit their theories to the reality of reality as it were. Cartwright (and Roy Bhaskar) offer us ways in which to do this with their various descriptions of the 'exportation of information' or bridging the 'unsoundness argument'. Cartwright actually does say something along these lines when she claims (1989b: 184) that '... abstractions can be taken as claims about capacities; ... and where abstraction reigns.... laws -- in the conventional empiricist sense -- have no fundamental role to play in scientific theory'.

Fundamental laws or abstractions, considered as capacity claims, have the following distinct features:

-- Cartwright calls the abstractions made in science material abstractions (which refers to the fact that there is no a priori recipe for getting from these abstract fundamental laws to the concrete situations (and capacities) they are supposed to be about). This brings in the role of ceteris paribus clauses again, since the suspension of these clauses in my terms 'puts back' what abstraction has 'left out'.

-- In Cartwright's terms the laws that constitute the phenomenological context of capacity claims are causal, and neither laws of association nor equations. In a model-theoretic system the nature of these 'laws' may be causal or not. They should merely be specific enough to function within the idealised contextual framework of the conceptual models of the theory in question.

What does it mean to 'link' an abstract fundamental law making some kind of claim about the capacities of nature, to reality? How can a general ascription of capacity like that be linked with different situations in reality? Cartwright (1989b: 203) discusses these matters in terms of what she calls 'idealisation' and 'abstraction' on the one hand, and 'concretisation' on the other. She formulates the most pressing worries about these processes in terms of the problem of material abstraction.

Concretisation has to do with the route 'downwards' (from abstract law to concrete real situation) and involves adding corrections to allow for the effects of interfering or disturbing causes that may be at work in a given real situation, because, as pointed out many times before, causes are rarely single and never act separately in nature. These corrections are a necessity, also in my terms. They are however not ceteris paribus but rather their function is similar to that of boundary and initial conditions. It is a necessary feature of the process of science, model-theoretically interpreted, that these 'corrections' are needed, given the abstract and simple nature of fundamental laws (or scientific theories) and the rich changing nature of facts in reality (whether this richness is the result of multiple interacting causes or of something else). So, Cartwright (1989b: 184), is not wrong in pointing out that '... the converse processes of abstraction and concretisation have no content unless a rich ontology of competing capacities and disturbances are presupposed', but my point is that that is true, however we fill in the ontology of reality.

The process of idealisation starts in reality, or rather, with some aspect of reality (Cartwright 1989: 187), and rearranges (conceptually) some of its (inconvenient) irrelevant features. However, note that the model still says something -- albeit something more simplified than is the case in reality -- about all relevant factors present in the real system focused on, and leaves out only the irrelevant factors. The ideal model thus still has a link with reality, because it sets the aspect being studied in a concrete situation. It is more 'real' because all relevant factors are present (even if they are, as pointed out, idealised and simplified versions of the real ones). However, these models also have an 'unreal' side to them -- they are after all representations, conceptual models about some aspect of reality.

The laws active in models are phenomenological ones (as pointed out above). These kinds of laws are however problematical in Cartwright's terms, in the sense that they are apparently subject to a kind of ceteris paribus condition -- they tell us what happens if the relevant factors are arranged in a specific (ideal) way. The (fundamental) laws describing the content of abstractions, however, do not have these ceteris paribus-like conditions. I disagree as I have often pointed out. The fundamental laws are not about everything. Newton's laws of motion and his law of gravitation were not about everything in the universe, but were rather concerned with everything to do with gravitation. For this reason it is (some) fundamental laws that need ceteris paribus clauses and not the phenomenological ones. The ideal character of the models is ideal because the theory has to be true in it, and not because of special ceteris paribus conditions. The only sense in which 'all other things are equal' in a model is in the sense of satisfying the truth relations between the model and its theory. This means that only entities and relations to which the (language of the) theory refers (under the relevant interpretation) occur in the model, and there they behave as the theory stipulates.

Now, for Cartwright it is phenomenological laws that offer the descriptions of real objects. She (Cartwright 1989b: 225) writes: '... if we could write [the phenomenological laws] down, [they] would literally describe the features of this concrete phenomenon and the nomological links within it. [Such a law] would be a highly complex law, and would include a specific description of the physical structure and surroundings of the concrete device. Possibly it could not be written down; perhaps the features that are relevant to its operation make an open-ended list that cannot be completed. But because we are talking about a concrete object, it is at least conceivable that some law is true of its operation.' Actually, there exist no such 'open-ended lists' of features relevant to the operation of 'highly complex' laws. Such lists are only necessary if one's aim is to describe a system in all its complexity, and although -- as pointed out before -- that seems to be Cartwright's aim, it is neither my own nor the aim of science in general. Science never tries to describe the features of any concrete phenomenon in all its complexity. No empirical model of a specific real system is ever complete -- or need ever be complete.

The problem of material abstraction is however -- as pointed out before -- that there is no universal a priori rule that sets out the movement from the abstract law to the phenomenological content, because the additions and corrections necessarily made by the phenomenological laws depend in each instance on how the abstraction is realised in that particular case. Cartwright wants to know why or how scientific realists may believe that what happens in an ideal case is commensurable with real cases? Her (Cartwright 1989b: 190, 191) answer is as follows: '... the logic that uses what happens in ideal circumstances to explain what happens in real ones is the logic of tendencies or capacities. What is an ideal situation for studying a particular factor? It is a situation in which all other 'disturbing' factors are missing (or controlled at least). And what is special about that? When all other disturbances are absent, the factor manifests its power explicitly in its behaviour. ... This tells you something about what will happen in different, mixed circumstances -- but only if you assume that the factor has a fixed capacity that it carries from situation to situation.'

The naive realist description of the scientific process in terms of approximations, however, conflates two separate actions, namely abstraction and idealisation, which both Cartwright and I (although maybe for different reasons) want to keep apart in our interpretations of this process. I assume, on the other hand, that scientific descriptions and explanations given by theories and models can be 'carried over' to real situations. This is, rather than being the result of the reality of capacities, because of the way the model-theoretic system interprets the links between aspects of reality, models and scientific theories, and also, because of the fact that we see these theories being applied -- and working -all around us every day.

In Cartwright's case, the focus is simply on kinds of abstraction aiming at formulating laws of capacities, where it is causal factors that are isolated and their capacities that are studied. But what exactly, is it that ascriptions of capacities (or fundamental laws or abstract theories) say about real things? In other words, what facts in reality make abstract capacity claims true? Given the hierarchy of laws that Cartwright works with, the answer to this question lies in the nature of the bearing of abstract theories on the more concrete and descriptive laws, that is, the phenomenological laws, that fall under them. It is, however, very difficult to answer these questions satisfactorily, because of the problem of material abstraction, already briefly, referred to in the above. At the centre of the process of material abstraction lie activities of correction and addition, which are not ruled (governed, or determined) by any set of universal (a priori) rules. (Which is my point exactly.) These activities are, also, not of the ad hoc nature they are sometimes thought to be by naive scientific realists. Rather, according to Cartwright (1989b: 202) they are sufficiently motivated in the sense that they genuinely describe other causes, interferences, and so on, that are active in the particular concrete situation being considered. These correcting activities are necessary, because of the subtractions abstract laws make (when addressing reality for the first time in my terms).

However, as Cartwright points out (1989b: 106, 107), to get back to the actual concrete laws that constitute the phenomenological content of these abstractions, the initially omitted factors must be added in again. But, where do these factors come from? Cartwright (1989b: 204-206) explains that the theory itself provides them, and gives an example (ibid.) to illustrate her point, although she also stresses that they are never entirely given by the theory since there will never really be an end to further factors relevant to a particular case. She says (1989b: 207): 'I have put "real" in quotes to signal my worry. For I think that, no matter how open-ended the list is, this kind of process will never result in an even approximately correct description of any concrete thing. For the end-point of theory-licensed concretisation is always a law true just in the model.'

It seems that Cartwright somehow links the so-called 'open-endedness' of the corrective ceteris paribus -- in her terms -- clauses to the fact that theories can only be true in their models. Again, that is not the model-theoretic conclusion, because of the semantic links in terms of satisfaction functions between linguistic systems and their mathematical structures (conceptual models) and substructures (empirical models). Alan Chalmers (1993: 196) offers the following example in support of Cartwright's views: 'The explanation and prediction of the return of Halley's comet was certainly a triumph for Newtonian theory. Nevertheless, the first "sighting" of the comet on its most recent return enabled the predicted orbit and time of return to be corrected and subsequent attempts to track it were able to benefit from that correction.' The empirical truth of this illustration is of course not disputed, however, I do not see why it necessarily has to be interpreted as implying that theories can only ever explain what happens in models. These corrections were made as a result of what 'really' happened --real observations -- and consequently led to more sophisticated models of Newton's theory, which at the same time are empirically more adequate than previous models.

There is a very fine difference in emphasis between Cartwright's account (of the process of science) and mine, but it is a very important one, because it relates to our attitudes towards the realism of our models. I would prefer to say, rather than simply claiming that theories cannot say anything about the aspect of reality that their models may be linked with, that theories can only explain in that little piece of reality that each of their models 'refers' to (or rather might refer to). In other words, taking a previous example: the solar system model of Newtonian mechanics consisting of only seven planets, (used before Neptune was discovered) did indeed refer to the real situation. Although, in reality there were nine planets all along, the fact remains that it is quite possible to concentrate only on some of them and not on all at once. Whether and to what degree Newton's laws were empirically adequate when using this 'restricted' model might seem to be a more difficult issue to deal with. But it is not really, since I claim -- in agreement with Cartwright -- that empirical adequacy (or truth) is a notion that can only be used meaningfully if linked with the model offering the relevant interpretation of the theory being considered at the time, and the relevant empirical model available at the time.

In conclusion, if one believes in capacities, yes, then, a realist interpretation of these capacities would be the only way in which to still make sense of empirical science and transduction. However a belief in capacities is not necessary for a valid model-theoretic account of science. Rather, a model-theoretic account of science is necessary if one wants to give a realist interpretation to the notion of capacities.

3. Roy Bhaskar's transcendental realism 3.1 Introduction

I shall very briefly in what follows discuss Roy Bhaskar's transcendental realist account of science. Bhaskar (1978: 24) summarises his notion of science and the role he envisages for realism in philosophy of science as follows: 'For science, I will argue, is a social activity whose aim is the production of the knowledge of the kinds and ways of acting of independently existing and acting things.' Transcendental realism makes the following claims about nature and the body of scientific knowledge:

-- The objects studied by science are real structures which exist and act independently of our experience, our knowledge, or the conditions enabling us to study these objects.

-- Thus the objects of scientific knowledge are the structures and mechanisms (Cartwright's capacities) that generate phenomena in the natural world.

-- These objects of knowledge are intransitive structures -- neither events (classical empiricism) nor artificial constructs of human making (transcendental idealism). They exist totally independent of any human activities concerning knowledge production. These objects are by no means unknowable (remember that they are after all the objects of science): they are simply independent of our knowledge or perception of them.

Bhaskar (1978: 26) claims that only transcendental realism can sustain the idea of a law-governed world which exists and acts independently of human activities; and, moreover (or so he claims), only through grasping this notion can science be practised at all. His realist philosophy of science thus distinguishes between intransitive (ontological sphere) and transitive (epistemological sphere) objects of scientific knowledge. His ontology basically has three levels:

-- (1) 'real' generative mechanisms (at the intransitive level);

-- (2) 'actual''events' which are natural phenomena (also at this level) that the mechanisms tend to produce; and

-- (3) 'empirical' experiences of these events (at the transitive level). Transitive objects are the Aristotelian material causes, the (conceptual) raw material of science, out of which the products of scientific knowledge have to be constructed or developed. Instead of a direct relation between knower and known, knowledge is produced via these transitive objects. They include the already established arsenal of theories, facts, paradigms, models, and methodology available to a particular scientist working in a particular scientific community at some particular time, in other words the socially evolved and applied 'tools' of scientific inquiry and study.

Generative mechanisms are irreducible to events as well as independent of experience of events, though these mechanisms have the ability to produce events via their modes of behaviour or their 'tendencies' (which are expressed by the fundamental laws of nature, in their turn -- but more about this later).

It is thus possible to imagine a world of intransitive objects without science, but it is not possible to have any thought of science without transitive objects. In a way, Bhaskar is reformulating the problem Bachelard had already referred to in the 1930s: this was simply the question of how scientific knowledge can be about a human-independent reality, if this reality is so thoroughly dependent on human productive action. In Bhaskar's terms this question becomes 'How can scientific knowledge be about a human-independent reality if the methodological foundations of science are so thoroughly human-dependent?'.[23] An adequate philosophy of science would in these terms be one which can make sense of both the inherently social character of science (transitive dimension) and the independence of(the enterprise of) science (or of scientific knowledge) of the objects being studied by science (intransitive dimension). Note that the distinction between the intransitive and transitive dimensions implies a distinction between ontology and epistemology, that is between the real, knowledge-independent objects of science and the historical, social production processes of knowledge of these objects.[24]

3.2 Ontology and epistemology: fundamental laws and experimental activities

Bhaskar sees transcendental realism as directly opposed to what he calls 'empirical realism'. To illustrate the most important differences between these two forms of realism, I shall (in this article) concentrate specifically on the different notions of causal laws and constant conjunctions of events (Hume's notion) offered by them. Bhaskar's account of the laws of nature is based on

-- the nature of experimentation in the natural sciences, and on

-- the fact that laws supported in or confirmed by experimental activity are presumed to be applicable outside experimental situations (ie the transfactual identity of laws or simply what Bhaskar refers to as transduction).

Empirical realists view causal laws as dependent upon, or simply as constant conjunctions of events, while Bhaskar insists on an ontological distinction between 'patterns of events' and causal laws. He sees the structures and mechanisms at work in the intransitive dimension as forming the real foundations of causal laws and constant conjunctions as patterns of events which exist in the transitive dimension and are generated by these structures and mechanisms. His stratification of reality (intransitive and transitive dimensions) is mirrored in his distinction between open systems in which constant conjunctions cannot occur (because of their spatio-temporally restrictedness, for instance) and closed (experimental) systems, in which they can (and do) occur. These distinctions between structures and sequents of events, and open and closed systems presuppose the comprehensibility of experimental processes or activities. An experiment is simply a way in which to study (observe) or test some hypothesis about the modus operandi of a single mechanism or structure, activated and isolated in a particular closed (designed, produced, and controlled) system.

Note that Bhaskar by no means denies the significance of experience for science. It is simply that he sees the 'intransitive origin' of the objects being experienced (which in its turn presupposes the structured and stratified nature of the world) as a condition for the intelligibility in science of experience.[25]

Laws are then, in this way, explicated as tendencies (modes of behaviour) of generative structures and mechanisms operating in the intransitive dimension and thus in open systems -- and not (Humean) authorisations for deductions of (patterns of) events given in effectively closed systems. The laws of nature express the tendencies of generative mechanisms to behave in their own particular ways. Laws are, in other words, recordings or expressions of the tendencies of generative mechanisms to produce events in certain situations.[26] '[Laws], when their initial conditions are satisfied, make a claim about the activity of a tendency, i.e. about the operation of the generative mechanisms that would, if undisturbed, result in the tendency's manifestation; but not about the conditions in which the tendency is exercised and hence not about whether it will be realised or prevented' (Bhaskar 1978: 98). As is the case with Cartwright's capacity laws, the causal effectiveness of (causal) laws is thus in no way dependent on the satisfaction of (Humean or classical empiricist) ceteris paribus ('all things being equal') clauses. These clauses' role is limited to the empirical identification and use of 'law-like' knowledge in closed systems.

Scientists carrying out experiments are the causal agents of the resulting sequences of events achieved under the conditions established for a particular experiment; however, they do not -- cannot -- cause the law underlying this pattern of events, precisely because it (the sequence) has been produced under closed conditions in the transitive dimension, while the causal law operates in the intransitive dimension (describing the operations of the generative mechanisms constituting the world). Note that tendencies of generative mechanisms do not necessarily lead to regularities at the transitive level, because they will typically -- as Cartwright also claims -- be at odds with other mechanisms in complex ways. Think of the petal of a rose in a vase falling to the table. The petal is subject to inertial, gravitational, hydrodynamic, and other mechanisms. It is the gravitational tendency of the mechanism generating it that makes the petal fall to the floor or the table; but to identify that tendency and thus the law governing the tendency will need practical intervention in the sense of creating the possibility (experimental situation) to study the event (falling petal) without the interference of other mechanisms and their specific tendencies. Constant conjunctions are thus (against the empiricist tradition that causal laws are constant conjunctions of events) neither a necessary nor a sufficient condition for causal laws.

Henri Poincare's problem of 'transduction' is solved (Bhaskar 1986:30) if this difference in the natures of causal laws and constant conjunctions is taken into account. As pointed out above, Poincare's problem centres around the absence of any rational justification for the (empiricist) supposition that causal laws hold outside the specific closed system (laboratory) where they have been 'formulated'. Bhaskar stresses the fact that scientists produce the empirical grounds' for the laws of nature in their laboratories, but not the laws themselves. He (Bhaskar 1986: 30) claims that distinguishing between 'real and universal ... but non-empirical laws and their real and empirical but contextually localised grounds' dissolves the problem of transduction. The justification for each individual law can thus be found in the stratification of nature, and not by tying laws to closed systems and ceteris paribus conditions (classical empiricists).[27] Cartwright (1995a: 155) echoes this when she claims that scientists should '... figure out how to combine laws together and how to cash out ceteris paribus conditions ...'. This has to be done, given her problem of material abstraction, against the material conditions of the situation in question, and the only way in which this is possible, she claims, is to assume the existence of capacities.

The aim of an experiment is thus simply to activate and isolate a specific mechanism. These actions then cause a particular sequence of events which enables the scientist to identify the causal law which describes the operations (in the intransitive dimension) of the mechanism isolated by the conditions of the experiment. Note that the fundamental reason or logical basis of an experiment is precisely the supposition that the causal laws identified in experiments exist outside the context of the experiment (in other words supposing their transfactual identity). Thus 'empirical invariances, i.e. the realisation of the consequents of law-like statements, are not a necessary condition for the assumption of the efficacy of causal laws' (Bhaskar 1975:103). In this sense then laws are statements about the activities and operations of the mechanisms in the world; in no way can causal laws be viewed as empirical statements, nor can their natural necessity be viewed as being connected to any kind of human rule[28] --which is also very much in line with Cartwright's later claims.

4. Conclusion 4.1 General remarks on Cartwright and Bhaskar

Cartwright (1989b: 181) concludes that Nature in no way presents us with Hume's laws of association as 'given', but rather Nature selects the capacities of different entities and determines their interaction. 'It is not the laws that are fundamental, but rather the capacities ... Whatever associations occur in nature arise as a consequence of the actions of these more fundamental capacities ... Nature, as it usually occurs, is a changing mix of different causes, coming and going, a stable pattern of associations can emerge only when the mix is pinned down over some period or in some place. Indeed, where is it that we really do see associations that have the kind of permanence that could entitle them to be called lawlike? [Only in ancient astronomy or in science laboratories.] ... laws of association are in fact quite uncommon in nature, and should not be seen as fundamental to how it operates. They are only fundamental to us, for they are one of the principal tools that we can use to learn about nature's capacities' (Cartwright 1989b: 181, 182). In these terms then, the basic laws of physics are not laws about sequents of events (a la Hume) but laws about the capacities of nature.

Bhaskar also claims that causal laws are 'ontologically uncoupled' (Bhaskar 1986:44) from patterns or sequents of events. Empirical regularities only occur as a result of active interference in nature: therefore this ontological distinction -- between the empirical regularity that scientists produce (in the transitive dimension) and the causal law (in the intransitive dimension) that it enables us to identify -- has to be presupposed and acknowledged if experimental (and thus scientific) activities are to be comprehensible. Causal laws are in no way dependent on the practice of science, but constant conjunctions viewed as experimentally controlled, induced productions are necessarily totally practice-dependent. These laws cannot be judged to be 'empirical' in any sense, while constant conjunctions can and are.

I have no quarrel with remarking that 'a stable pattern of association can emerge only when the mix is pinned down over some period or in some place', because that fits also my description of the functions of models and the ways in which they interact with theories on the one hand, and with aspects of reality on the other hand.[29]

Bhaskar states clearly in A realist theory of science (1978) that the relationship between science and reality seems problematic only if one either accepts the social character of science, but denies that its object of study is independent of all social activity (the epistemic fallacy), or if one accepts the independence of reality, but denies the social nature of science (ontic fallacy). He sidesteps these fallacies by starting off assuming that the process of science (especially the experimental side of it) does make sense and is indeed rational, and then transcending these practices by asking what reality has to be like for these assumptions to be philosophically intelligible.

If Bhaskar's transcendental realist assumptions about the relation between reality and science are not made, any scientist could generate any pattern of events at will, rendering all scientific activity totally uninteresting. In other words, a realist philosophy of science should be able to sustain an epistemological relativity, but fight against surrendering to ontological relativism. Bhaskar refers to epistemological relativism as the '... correct thesis of epistemic relativity, which asserts that all beliefs are socially produced, so that all knowledge is transient, and neither truth values nor criteria of rationality exist outside historical time ...' (Bhaskar 1989: 57), while he calls ontological relativism '... the incorrect thesis of judgemental relativism, which asserts that all beliefs are equally valid, in the sense that there can be no (rational) grounds for preferring one to another ...' (ibid.).[30]

A model-theoretic realism can -- as it should be able to do --accommodate such a Bhaskarian epistemological relativism. The varied nature of the conceptual models of a scientific theory represents precisely such a relativism. A model-theoretic framework however offers -- besides the fact that, as in the transcendental realist case, no ontological relativism need follow -- two additional implications of a realist nature:

-- retaining the notion of a scientific theory as the only means by which all the possibilities offered by the variation of models possible in a particular instance protects the epistemological choices at the conceptual (model) level from dissolving into a meaningless multiplicity, and

-- the internal structure of the relations between the conceptual models of a specific theory and systems in reality prohibits the stark anti-realist implications usually associated with the problem of the underdetermination of theories by data (and models in this framework).

We as philosophers cannot tell which model (of a certain theory) provides the most adequate description of reality, because the ontology a realist philosophy of science can offer is limited to descriptive claims about the structure of reality, while the epistemology it offers centres around the conceptual structure and development of scientific knowledge. Thus, although something akin to Bhaskar's distinction in terms of transitivity is also assumed in a model-theoretic realism, such a realism need however not dwell on the kind of metaphysical musings about the actual structure of reality that both Bhaskar's notion of law-like mechanisms and Cartwright's capacities leading to some patchwork of laws seem to imply. Only science itself can offer us an ontology which can specify the contents of the structures reality contains and the particular ways in which it behaves.

Both the 'truth' of our scientific conceptions and the establishment of the 'reality' of the system described are products of epistemically relative interpretations and subject to change. Of course I (and Bhaskar too, I think) would agree that for instance theories explaining and describing light as photons (quantum-mechanical entities that exhibit both characteristics of waves and of particles) are scientifically 'more advanced' or 'better' than theories based on Fresnel and Young's theories that claimed light to be transverse wave motion, but this does not in any way imply that I somehow view Planck and Einstein's theories about light to be out of reach of further scientific activities or criticism. Science does progress and scientific knowledge is cumulative. But, this progress is made at different speeds at different levels of the development of science. For instance, the model of Newton's theory of solar systems which works with seven planets can be said to refer to an aspect of reality, because the question of the truth of the theory is model-specific in the sense that it depends on the satisfaction of truth criteria which may differ from model to model and are satisfied differently in different models. Thus, in my example, Newton's theory may indeed be true in each independent model thereof, by specific valuations under specific interpretations. But the theory, taken in its general uninterpreted form -- could not be said to be true because the model with seven planets referred to reality, and then be said to be false, because of the construction of the new, more encompassing model dealing with eight planets. Theories change very slowly, conceptual models more quickly, and empirical models and the empirical data bases (the accumulation of empirical data via observations and experiments) they depict, the quickest. Theory changes occur only when the possibility of changing and modifying the models of the theory concerned has been exhausted, which confirms the continuity of scientific knowledge.

Scientific method should thus ideally provide a model-dependent model-modifiable strategy, because such a strategy offers within a realist context the possibility of modifying or amending our existing theories in the light of further research. The methodological principles of a strategy like this will themselves depend on the theoretical picture provided by currently accepted theories. Both our new theories and the methodology by which we develop and apply them depend upon previously acquired theoretical knowledge. And this fact about the continuous nature of science, as well as science's various relations to reality can best be supported and explained by a model-theoretic realist conception of scientific knowledge.

4.2 Model-theoretic realism

Model-theoretically speaking science studies systems in reality. This refers to the abstracting simplifying nature of science. As already often pointed out in the above, no-one, not even scientists, can study reality in all its fullness at once. Not only do scientists focus on some particular system of phenomena in reality, but also they aim to 'adjust' that system in such a way that they can focus only on certain of its features. The kind of abstraction that Cartwright talks about and that I have discussed in a model-theoretic context in Ruttkamp (1999,this issue) is a necessary and sufficient condition for scientific knowledge. If certain abstractions are made from the richness of experiences that reality has to offer, scientific knowledge of the real system in question becomes possible. And, vice versa, if some knowledge claim is offered as part of science, the nature of that claim will (relative to the complexity of the universe) be simple and it will be about a sufficiently abstract version of some real system (even if that system is the cosmos!).

Reality is not unknowable, but rather only knowable in a certain way. I am aware that this sounds particularly Kantian, but that is entirely my purpose. The basis of a model-theoretic methodology is Kantian in the sense that it implies that we can only know reality through science, but that scientific knowledge can only be achieved through certain abstractive actions. However, the Kantian Dingan-sich is the reality we study via science. There is no other 'underlying mechanism' or anything else that is somehow so fundamental to the ontology of reality that we cannot know it. Knowing through abstraction is knowing. There is no other kind of knowing, scientifically speaking. And, moreover, this kind of knowing is adequate in the sense that it does indeed allow us to study, discover, and utilise knowledge about reality.

Any kind of realism that decides how reality has to be in order for science to be possible -- such as that of Bhaskar (1978) -- is too metaphysical for my taste. I have been arguing all along that a model-theoretic realism is one that focuses more on science than it does on reality. Connecting an ontology of science to an ontology of reality however often seems to be strangely lurking behind many philosophic accounts of science. It seems as if Cartwright (1989b), for one, has been seduced. Her hierarchy of 'capacities' somehow seems to be offered as a kind of mirror image of her hierarchy of scientific laws --fundamental laws are capacity claims, phenomenological laws depict causal relations in reality, and the experimental stage of science focuses on 'singular causings'. Roy Bhaskar (1979,1981) also seems to have been caught, albeit perhaps in a different, but not necessarily lesser, way. In Bhaskar's terms the mechanisms of reality form part of the lowest -- ontologically deepest -- level of reality and their scientific counterparts are causal laws. The actual events produced by these mechanisms still form part of the 'intransitive' (real) dimension of science but are conducive to the identification of constant conjunctions or patterns of events at the 'transitive' dimension of science.

I plead for a distinction between ontology and epistemology with regard to science. I am aware that philosophers such as Joseph Margolis (1995) deny that this is possible. The reasoning behind disclaiming that such a distinction is possible seems to me as mainly based on the notion of science as a social construction. This is another of many contemporary echo's of Bachelard's (1934) point that it is difficult to see how science can be said to be about a human-independent reality if our notion of reality seems to be so very dependent on human action. One of the problems here, I think, is a certain vagueness of terminology.

In trying to clarify this confusion, let us first make it clear that in model-theoretic terms science is indeed also an individual and social construction. Science is 'transitive' in Bhaskar's sense as against the 'intransitivity' of reality. Scientific knowledge can change and is contingent on the actions of scientists formulating it. However the notion of 'reality' is sometimes used as a group noun to refer to the immediate 'stuff' or results of scientific knowledge. That is, the idealised pictures of the 'external' world that science offers us are somehow seen as constructing a reality about which science is. Since these 'images' are also still changeable because they are part of science, it is then perhaps concluded that reality too is changeable and therefore that a scientific epistemology is necessarily linked to an ontology of this (scientific) reality. The 'immediate' pictures that models of scientific theories offer of some system in reality are however just that -- i.e. representations of reality. The reality that is independent of science, in the sense that it exists regardless of whether its processes have already been 'discovered', 'explained', or 'described' by science, is perhaps better denoted by the -- somewhat outdated -- term 'Nature'. Reality in this sense is not dependent on human actions at all (except in so far as it encompasses humans and their actions or technology based on science). It is a complex system of 'mechanisms' the processes of which continue now as they have done through the ages. And, it is the ontology of reality in this sense that is separate from scientific epistemological factors. The 'reality' of science is an idealised version of this 'Nature' and consists of already established theories and the various actions of scientists as well as the results of these actions. This 'reality' is a social construction and this is the reality the more moderate constructivists claim to be constructing, in the sense that science cannot be studied without taking these matters -- i.e. the human activities and their context-dependent motivation driving the scientific process -- into account.

The ontology of this 'scientific reality' and the epistemology of its generator -- i.e. science -- can indeed not be separated. In a model-theoretic account of science this becomes even more clear. The models that offer us these pictures are an integral part of the process of science. The set of conceptual models referred to as the 'intended' models of scientific theories in my account of science,[31] are shaped by the already established pictures of this kind. And, moreover, at the interpretative stage of science the justification for, and evaluation of, scientific theories are offered and carried out within the context of this 'scientific reality'. The point is however, that in the final instance, although science is 'social' and 'constructivist' it is not a reflexive enterprise in the usual social constructivist meaning of the word. Science is not about something that it has constructed itself and that is inherently of the same nature as science itself. Science is about 'Nature' and about discovering the intricacies of the mechanisms according to which 'Nature' operates. In this sense, as pointed out above, ontology -- in the sense of the ontology of that about which science is -and epistemology have to be separated.

The 'game' of post-Kuhnian philosophy of science has one trick that has to be learnt if the realist quest is to be salvaged. This is the trick of keeping constant one of the changing factors at issue i.e. Nature, while acknowledging the variability of all the others involved.

Making the choice -- as a model-theorist would also do -- for Bhaskar's epistemological relativism, and thus acknowledging that truth criteria as well as criteria for (scientific) rationalism are part of the philosophy of science, does not mean that any criterium goes, but rather the opposite. The construction of these kinds of scientific criteria is, model-theoretically speaking, undeniably a function of the conceptual and empirical models of scientific theories. Validating them is the function of the various semantic relations that exist between reality as 'Nature' and these models.

The main question that one tends to want to answer in a realist context is, of course, how exactly scientific theories 'get to' reality. The problem is that there is no simple answer to that question. The conclusion drawn from claims like these -- e.g. 'there is no final description of the links between scientific theories and reality such that theories invariably refer to "something" in reality' -- should however not be that therefore realism is untenable. A model-theoretic approach to realism shows us exactly that, and more importantly, such a realism offers us the tools to examine -- and make sense of -- the various and complex empirical links between scientific theories and real systems. It may seem that -- perhaps as a result of the many-to-many relations between theories and their conceptual and empirical models, as well as between these models and systems in reality -- model-theoretically one merely ends up in the empirical substructures of some conceptual model of a given theory, rather than 'in reality'. Well, what does it mean to be 'in touch' with reality? I cannot see it meaning anything more than being in touch with the empirical practices of science. And, that is precisely what the empirical models embedded into a given theory's conceptual model(s) allow us to do. Of course, they allow this in a conceptual way, but then, the content of science -- i.e. the set of its knowledge claims -- is conceptual too is it not? A model-theoretic realism is thus a realism about objects in reality and the relations between them, although conceptual models (and the empirical models isomorphically embedded into them) are used to describe real systems by describing the systems' objects and the relations between these objects.

Take Newton's laws of motion and his law of gravity applied to our solar system again. Such a model (i.e. a model of our solar system) will be described by an uncountable set of sentences which, i.a., describes every position at any time of every planet in question on an elliptical curve. I claim there exists a transitive connection between experimental data and some model of the theory, since the data offer 'pieces' of the model by means of some 'experimental theory' in the sense that the theory of the experiment 'translates' observations into data (or models of data), which can be then possibly linked with (i.e. embedded into) some model of the theory. If a scientist is looking at a planet through a telescope, the theory of the telescope translates those observations into data, and these data give the position of say Mercury at a given point in time. But this is exactly what the empirical models of Newton's theory offer (in this context), since a conceptual model of the solar system offers here the positions of all the planets at specific times. The data thus do depict certain relations valid in models of the theory.

A model-theoretic approach to science supersedes and encompasses the best aspects of both the statement and non-statement accounts of scientific knowledge. Although in both the latter accounts

-- albeit in different ways -- it seems that the notion of a scientific 'theory' --however this notion is interpreted -- may be given some realist interpretation at least, the unnatural (and simply wrong) rigidity of the statement approach's correspondence rules as well as the non-statement disconnectedness of scientific theories and their models do not allow for reference to reality in a satisfactory way. In a model-theoretic approach a scientific theory is a certain (deductively closed) set of sentences linguistically expressed.[32] The conceptual embodiments of the contents of these theories are done via their models and the referential relations in question in a realist context are determined both by the empirical substructure, isomorphically embedded in some conceptual model of the relevant theory, and by the nature of the real system in question. By maintaining such an encompassing interpretational link from the theories themselves all the way through their models to some real system(s) the complicated and changing character of science may be described and accounted for in a more adequate way than is perhaps the case with some statement and non-statement approaches to science.

The problem haunting philosophers dabbling in more metaphysical aspects of scientific theories -- like Bhaskar and the later Cartwright -- is exactly to show that -- and how -through the complicated contingencies of science a given scientist is still dealing with the same physical phenomenon as her predecessors and her peers. These considerations are related to the underdetermination issue, since the problem is to show that the scientist in question can 'get to' the same phenomenon whichever theory (from the class of theories underdetermined by the phenomenon in question) she chooses to use. And, in this sense, Cartwright's capacities -- in as far as they somehow have some stabilising influence on the 'complex uncontrollability' of nature --may indeed seem to have a lot going for them. She (Cartwright in Boyd et al., 1991: 386) writes: 'Competing theoretical treatments -- treatments that write down different laws for the same phenomena -- are encouraged in physics, but only a single causal story is allowed. Although philosophers generally believe in laws and deny causes, explanatory practice in physics is just the reverse.' First, we know that science is not about stabilising in the sense of somehow changing the complexity of Nature into a controlled system. Rather science is about offering a glimpse as it were of some specific aspect of Nature. Such a representation of Nature is perhaps 'stable' in so far that it focuses only on relevant features of the aspect of nature it concentrates on at a given time. However a model-theoretic realism shows that underdetermination -- in a sense the converse of allowing only a 'single causal story' -- is a necessary characteristic of science, since the abstracting nature of the methodology of science specifically implies that other routes to the same conclusions are possible under a different abstraction from the same aspect of nature.

The necessity of looking to Nature -- and, in philosophy of science terms, thus perhaps turning to metaphysics -- for solutions to the supposed puzzles concerning underdetermination dissolves within the framework of a model-theoretic realism. As pointed out before, the main assumption of such a realism concerning Nature is simply that it (i.e. Nature) exists independently of science. This basic condition is emphasised and worked out by the model-theoretic insistence on the roles that both science -- in the guise of a specific conceptual model of a given theory having isomorphically embedded into it a certain empirical model -- and Nature -- in the sense of the characteristics of some real system satisfying the empirical results embodied by the specific empirical model in question play in the processes of science. No metaphysical characteristic of Nature somehow worked into the mechanics of science is necessary to make sense of a scientifically realist picture of Nature. Rather the definition of the methodology and strategies -- and aim of science, model-theoretically interpreted, already takes care of all of that. And, a scientist can 'know' that she is working with the 'same phenomenon', even if using 'different' theories, simply because of the possibility of analyses that a model-theoretic realism offers of the different empirical links between different empirical models of different conceptual models in (perhaps) different theories. Detailed analyses of these empirical links will reveal common factors on the reality side of the link (e.g. light blobs observed through different telescopes by different people at different times indicating -- by careful analyses -- a common factor called 'Neptune') which entails the 'same phenomenon'. There is, however, no universal prescription for these analyses.

Notes

1. *Think of Arthur Fine's 'natural ontological attitude' (so-called) solution to the realist problem.*

2. *See Ruttkamp (1999) in this issue for more on these accounts of science.*

3. *And so they should be, given that they form part of human scientific knowledge which, from the beginning, simply is based on activities of abstraction, because that simply is how we humans know anything.*

4. *My italics.*

5. *We have already agreed that fundamental laws are indeed too simple and abstract to be directly about any aspect of reality.*

6. *She also remarks in Cushing, Delaney, and Gutting (1984: 135) that '... abstractness and scientific realism are two different issues, and not all varieties of abstractness bear equally on questions of descriptive completeness, accuracy, and truth. This is so with our notion [of abstraction], where notions become more and more abstract as less and less explanatory information about them is given.'*

7. *Both Cartwright and I view these models as idealisations, although we differ about the implications of the ideal nature of these models for the process of science, as will be discussed below.*

8. *Cartwright, 1983, chapter 8.*

9. *Cartwright's 'phenomenological laws' remind one very much of Suppes's (1989) models of data.*

10. *See also Chalmers (1987: 82) for confirmation of this interpretation.*

11. *Well, this is true of fundamental laws too -- Newton says openly he offers no hypotheses concerning the reasons why his laws of gravitation are true.*

12. *See also my notes concerning Newton's mechanics offering an explanation of Kepler's laws in Ruttkamp (1999).*

13. *Remember that Cartwright takes scientific theories to be a set of fundamental laws -- like Maxwell's equations -- from which explanations in physics are supposed to start.*

14. *Cartwright claims (1983: 150) a model realistic in this second sense to be in need of more bridge principles than one realistic in the first sense. The best explanation for this is, I think, the fact that she sees the mathematical representation as being closer to -- or perhaps mainly identical to -- the theory.*

15. *As she explains (Cartwright 1983: 152-154): 'The second definition of "simulacrum" in the Oxford English Dictionary says that a simulacrum is "something having merely the form or appearance of a certain thing, without possessing its substance or proper qualities"'.*

16. *She offers Maxwell's treatment of the radiometer as a further example -- Cartwright 1983: 154--155.*

17. *Cartwright illustrates this accusation with the following remarks: 'Not all radiometers that meet Maxwell's two descriptions have the distribution function Maxwell writes down; most have many other relevant features besides. This will probably continue to be true no matter how many further corrections we add. In general ... the bridge law between the medium of a radiometer and a proposed distribution can hold only ceteris paribus' (Cartwright 1983: 155).*

18. *There are cases in which we believe phenomenological laws to be soundly deducible from a certain set of fundamental laws, but find that the actual deduction is extremely difficult. These cases, however, do not prove in any way either that phenomenological laws 'typically' cannot be deduced from fundamental ones, or that the 'all things being equal' and additional assumptions needed in such deductions may be found to 'contradict' the original (set of) fundamental law(s).*

19. Cartwright offers the problem of superconductivity as illustration -- see Cartwright, Shomar and Suarez 1995: 142-149.

20. This notion of capacities is close to Popper's propensities. See Popper (1990).

21. In addressing the testability of causal claims, Cartwright uses probabilities, while the Humean tradition reduced causal laws to probabilities. She says: 'I defend a very different understanding of the concept of Natural Law in modern science from the "Laws = universal regularities" account ... We aim in science, I urge, to discover the natures of things; we try to find out what powers or capacities they have and in what circumstances and in what ways these capacities can be harnessed to produce predictable behaviours. I call this the study of natures because I want to recall the Aristotelian idea that science aims to understand what things are, and a large part of understanding what they are is to understand what they can do, regularly and as a matter of course. Regularities are secondary. Fixed patterns of association among measurable quantities are a consequence of the repeated operation of factors that have stable capacities (factors of this kind are sometimes called "mechanisms") arranged in the "right" way in the "right kind" of stable environment' (Cartwright 1995c: 277). Ceterisparibus clauses can, however, it seems, not be escaped -- 'In order to generate a prediction [or, give an explanation] we must figure out how to combine the laws together and how to cash-out their ceteris paribus conditions -and we must do so in a way that takes into account the specific material circumstances of the situation under consideration' (Cartwright 1995a: 155). The way to do this then, is to assume the existence of capacities (as has already been pointed out) -- 'The point is that the fundamental facts about nature that ensure that regularities can obtain are not again themselves regularities. They are facts about what things can do' (Cartwright 1995a: 156).

22. For instance, she claims that standard philosophical accounts of probabilistic causality actually employ a concept of causation that is much stronger than the concept of a mere causal law and gives (Cartwright 1989: 142) the following formula, which she calls 'Principle CC', to illustrate this: 'C causes E' if and only if the probability ore is greater with C than without C in every causally homogeneous context. According to her (1989b: 143) the point with regard to this formula is that it is universally quantified, and thus represents a concept of causality powerful enough to be taken as a concept of capacity as well.

23. Note that Bhaskar views his distinction between intransitive and transitive objects, however, not so much as posing a problem for philosophy of science as offering the only possibility for understanding the concept of scientific knowledge.

24. Bhaskar claims that any account offered of science in fact offers an answer to the following ontological question: What must the worm be like for science to be possible? Thus, in Bhaskarian terms, the sense in which an account of science presupposes an ontology is relational or conditional. In these terms, it is the task of philosophical argument to establish the fact that the world is indeed structured and differentiated in this way; but it is the task of scientific ontology to explore and study the specific structures contained in the world and the particular ways in which this world is differentiated.

25. The comprehensibility of scientific experience depends on two factors. These are sense perception and experimental activity:

(a) Sense perception: Only because objects being perceived have an independent (intransitive) existence can the concept of 'perception' be meaningful and of epistemic significance. This is the case, Bhaskar argues, simply because these objects must have a spatio-temporally independent, distinct being if changing perceptions (experiences) of objects are to be intelligible. 'For Kepler to see the rim of the earth drop away, while Tycho Brahe watches the sun rise, we must suppose that there is something that they both see (in different ways)' (Bhaskar 1978:31). The objects being perceived are thus ontologically independent of the objects of perception. Thus, events (being the intransitive results of generative mechanisms' tendencies for certain behavioural modes) are absolutely independent of experiences; and a world of events without experiences ('unexperienced') is totally intelligible within a transcendental realistic framework. We only have to look at the history of science to see that it is entirely possible at any time for there to exist, in the intransitive dimension, types of events never even imagined at that time, of which scientific (theoretical and in some cases, empirical) knowledge is at a later time indeed achieved. The limits of scientific knowledge are thus continuously being extended, but then in the transitive dimension.

(b) Experimental activity: Experimental activities can only he intelligible if we presuppose the intransitive and structured character of the objects under experimental investigation.

- 26. Bhaskar probably uses the term 'tendency' rather than 'power', because of the reference to the transfactuality of laws and open systems in this context. Tendencies are potentialities which may be exercised in open systems without being realised, actualised, or manifest in any particular result or effect. In closed systems however, tendencies that are 'triggered' or 'set in motion' have to be realised, actualised or become manifest -- unless they are prevented in some external way.
- 27. Bhaskar uses the terms 'transduction' or the 'transfactual nature of laws' to refer to the applicability of scientific laws specifically outside the domain of actual experience. 'In the

full analysis of law-like statements we are thus concerned with a new kind of conditional ... [These conditionals[take us to a level at which things are really going on irrespective of [their] actual outcome' (Bhaskar 1978:51). These 'normic' conditionals are transfactual rather than counterfactual because they don't describe what would happen but rather what is happening, albeit in an unmanifested way. Bhaskar (1986:100) claims a scientist is always certain that, given some effect, something is producing the effect in question -- her only doubt is about what exactly that 'something' may be. In this sense, as claimed above, the function of scientists is primarily to produce a theory which correctly describes -- or adequately explains -- the mechanism by means of which the effect in question is produced. In asserting a 'normic' statement, one is asserting the operation of a mechanism, irrespective of its results (here is referred to the fact that tendencies of mechanisms do not have to become manifest or be realised or actualised in open systems). Hence, the fact that our knowledge can be both universally applicable and rarely instantiated can only become intelligible if we presuppose the (transitive and intransitive) stratification of the world.

- 28. In other words, the main 'condition of the intelligibility of the experimental establishment and the practical application of our knowledge [is] that its objects are real structures which exist and act independently of the patterns of events they generate. It follows from this that causal laws must be analysed as tendencies, which are only necessarily manifest in empirical invariances under relatively special closed conditions. Thus ... deducibility from empirical invariances, depending upon the availability of constant conjunctions of events, can be neither necessary nor sufficient for a natural scientific explanation. There is an ontological gap between causal laws and their empirical grounds' (Bhaskar 1989:68).
- 29. 'The harder we question nature, [and] the more fundamental the observations we make, the more dependent are the results of technique and theory' (Cook 1994:141).
- 30. See Spurrett (1998) for a thorough discussion of this aspect of Bhaskar's transcendental realism.
- 31. See Ruttkamp (1999) in this issue.
- 32. Note that, in principle, the language in question need not be a formal language at all. Almost any kind of scientific linguistic expression may be formalised in some first-order language and its interpretations re-constructed in a model-theoretic way.

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