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VAN FRAASSEN'S CONSTRUCTIVE EMPIRICISM, SYMMETRY REQUIREMENTS AND SCIENTIFIC REALISM

MICHEL GHINS

Abstract. Within the framework of a semantic view of science, such as the constructive empiricism advocated by van Fraassen, it is shown that the empiricist position naturally leads to symmetry principles in the construction of models in physics. As a consequence of those symmetry constraints, it is argued that a selective form of realism for theoretical objects is more plausible than antirealism. Several examples drawn from spacetime physics, static and mechanics are discussed, particularly the transition from Cartesian to Newtonian mechanics.

The semantic or model-theoretic approach of theories has long become the dominant view in philosophy of science. It opposes the logical positivist or syntactic conception of scientific theories. For the latter, a theory is essentially a set of theoretical postulates or axioms whose theoretical, non-observational, terms are interpreted by means of analytic correspondence rules which connect each theoretical term to a set of observational terms, i.e. terms which directly refer to observable entities, properties or processes. The objections to this neo-positivist view have been abundantly put forward in the literature (see for example Suppe (1989), van Fraassen (1980)) and it would be pointless to rehearse them. After a brief reminder of the central features of the model-theoretic approach, I will give some precisions on the notion of model which will be used, mainly inspired from van Fraassen's constructive empiricism which, in accordance with actual practice in physics, gives a prominent role to symmetries in model construction. I will then show that this particular brand of the model-theoretic construal of science naturally leads to restrictions, in terms of symmetry requirements, on the construction of models. As a consequence of these constraints, it is argued that scientific realism is more plausible than antirealism, in particular in the domain of classical spacetime physics.

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1. *The notion of model*

The fundamental sense, that played a role of paramount importance in the hands of the heirs of the logical positivists (Putnam and Quine, among others) is the well-known logical sense: a model is what satisfies or makes true a set of propositions. This sense, although obviously still accepted and used by the proponents of the semantic view, slid to the background among those who advocate a model-theoretic approach of science because it is closely linked with the neo-positivist conception which, among other drawbacks, laid too much emphasis on language and axiomatization. According to the defenders of the model-theoretic approach, a theory is not primarily a set of propositions or laws, but only derivatively, to the extent that such propositions describe the structure of a model, or, rather, a class of models: models are prior. Models moved to the forefront, they "occupy center stage" as van Fraassen puts it. A model then should not be introduced by starting from a set of axioms but should be presented "directly", independently of the laws and axioms which they may make true and which could even be unknown. Thus, the model-theoretic or semantic conception of theories is not only adverse to the positivistic *credo* that issues in the philosophy of science are in fact issues in the philosophy of the *language* of science but also to the contention that philosophy of science is first of all concerned with the logical, syntactic and even semantic, properties of axiomatized theories.

Of course, a model can only be conveyed in a language (this is, as van Fraassen remarks, a "trivial point"). What is a model then? Let me quote here a quite revealing footnote from van Fraassen's *Laws and Symmetry*:

> "In my terminology here the models are mathematical structures called models of a given theory only by virtue of belonging to the class defined to be the models of that theory". (1989, p. 366)

2. *The relation of models to phenomena*

A physical theory cannot be only a set of purely mathematical structures but must entertain an —at least possible— relationship to phenomena. This relationship is characterized by van Fraassen as "embedding": observable phenomena are embedded —or embeddable— in models. Phenomena which are isomorphic to parts or portions of the models are said to be embedded in them. In other words, phenomena are shown, by abstracting some of their aspects or contents, to possess a structure which is identical to the structure of some parts of the models. The parts of a model $\mathcal{M}$ which are isomorphic to possible phenomena are called "empirical substructures" $\mathcal{E}$. These substructures, being subsets of mathematical models, must obviously also be mathematical. Given that isomorphism means complete identity of structure, it follows that, for an empirically adequate theory, observable phenomena have the same mathematical structure as the empirical substructure: only their contents or "matter" may be different.

"Empirical adequacy is truth with respect to the observable phenomena" (van Fraassen 1989, 192–193). Truth here means isomorphism of an empirical substructure with the actual, real, structure of phenomena (which are *not* sense data). van Fraassen supplements his model-theoretic view with a correspondentist, and even representational, conception of truth. Models are not only copies of possible worlds, but possible worlds themselves in the sense that they possess and exhibit a formal structure which can in principle be shared, if the model is correct, by the real world. Empirical substructures of theories which save the phenomena correspond (and here correspondence means complete identity of mathematical structure) to real, observable structures of the phenomena.

> "(...) the empirical structures in the world are the parts which are at once *actual* and *observable*" (1989, p. 228)

It follows that *phenomena* embedded in empirical substructures (this is a limiting case of embedding when a model narrows down to an empirical substructure) have *mathematical* structures (also called by van Fraassen *appearances* (1980, p. 64)). The operation of embedding reveals or exhibits the real structures in phenomena. This view bears a striking resemblance with some of Wittgenstein's ideas in the *Tractatus*, where a true proposition has in common a logical form with the reality it depicts (a proposition functions as a "scale model"). It seems a direct consequence of van Fraassen's constructive empiricism that an empirical substructure has a mathematical form in common with the phenomena it represents. This appears particularly clearly from one of van Fraassen's main examples, the seven-point geometry, which occurs both in *The Scientific Image* (p. 42) and *Laws and Symmetry* (p. 219). In this well-known example, a specific structure of lines and points made of, say, ropes and nails (the "matter" of the phenomena), is embedded in Euclidean space.

Thus, in physics at least, a theory *is* a set of mathematical structures which contain empirical substructures and which exhibit symmetry and invariance properties.

There is quite a large array of mathematical structures as candidates for scientific theories. The main difficulty in physics, as in other mathematized sciences, lies in constructing models that have some chance of being empirically adequate, that is, which have a possible connection with phenomena.

The mere consideration of mathematical models themselves does not permit to draw a demarcation between the substructures which are empirical and the ones which are non-empirical or meta-empirical. Such a distinction is not intrinsic to mathematical models and must then come from elsewhere, namely from experience.

Having approvingly referred to the works of Przelewski, Wojcicki, Dalla Chiara, Toraldo di Francia, Patrick Suppes, Frederick Suppe, Sneed and others, van Fraassen goes on to say:

> "Certain parts of the models were to be identified [underlining is ours] as *empirical substructures*, and these were the candidates for the representation [underlining is ours] of the observable phenomena which science can confront within our experience". (1989, p. 227)

To qualify as such an empirical sub-structure $\mathcal{E}$ must be included in at least one model $\mathcal{M}$:

$$\mathcal{E} \subseteq \mathcal{M}$$

According to constructive empiricism, the aim of science is to save, or rather to embed, the observable phenomena, that is to build up models which contain substructures isomorphic to all possible phenomena within the realm of the theory.

van Fraassen pursues:

> "At this point it seemed that the relationship thus explicated [embedding] corresponds exactly to the one Reichenbach attempted to identify through this concept of coordinative definitions, once we abstract from the linguistic element. Thus in a space-time the geodesics are the candidates for the paths of light rays and particles in free fall. More generally, the identified spatio-temporal relations provide candidates for the relational structures constituted by actual genidentity and signal connections" (1989, pp. 227–228).

Thus, if science aims at constructing mathematical models that save the phenomena and if empirical adequacy is ascertained on the basis of observation, we must be able:

1. To identify in the models the parts which count as empirical substructures or possible representations of phenomena.
2. To empirically ascertain the isomorphism, that is actual identity of mathematical structure, of the structure of phenomena with the empirical

substructures.

The two questions are tightly related. An empiricist can only provide a solution to the first problem on the basis of a solution to the second. For example, geodesics (the metrically extremal paths) are empirical substructures because they possibly represent the paths of light rays. This possibility is guaranteed by the ascertainability, in appropriate circumstances, that observed light rays actually follow (or don't) geodesics. If they do, it shows that the paths of light rays are isomorphic to geodesics of space-time. This cannot be achieved, within the model-theoretic approach, by *fiat* or only by a definition *à la* Reichenbach, but by observations. We must be able to "see", given the meanings of the words "light ray" and "geodesic" (according to a specified metric) *that* the path of a light ray is a geodesic. For someone who is willing to defend the model-theoretic view of science like van Fraassen, phenomena, to the extent that they are embedded in models, must actually possess a mathematical structure. This seems to be clear if we follow van Fraassen when he compares embedding with the relationship that Reichenbach sought to identify by means of his coordinative definitions "once we abstract from the linguistic element".

What are the "actual" and "observable" structures in this case? Even though a mathematical structure is not perhaps *per se* observable, we must be brought to see or observe *that* some phenomena have a mathematical structure. If so, as a consequence of the correspondence view of truth, those mathematical structures are real. If it is true that light rays follow geodesics, it is also true that light rays possess the mathematical structure typical of geodesics. Since the structure of a geodesic is defined by means of metrical relations we cannot escape from the conclusion that light rays manifest a real metrical structure.

3. *Objections*

Let's pursue geometrical examples a little further. Phenomena in this case are given by the well-known empirical behaviour of rods and clocks determined on the basis of spacetime coincidences of (quasi-)punctual events. Once the meanings of geometrical terms (like congruence, or equality of lengths and angles) have been specified, experience teaches us that those phenomena are embeddable (locally) in a manifold endowed with a metric. In the case of Minkowski spacetime, the model is an infinite manifold of points endowed with a four-dimensional metric of negative signature and on which the Lorentz-Poincaré symmetry group acts. What are the empirical substructures? They simply are the intervals and angles which can be measured by rods and clocks (or equivalent measuring devices). Of course not all possibilities are actualized; not all intervals and angles, etc. are marked by actually observed bodies or processes. But at the level of empirical substructures, there is no reason not to accept the existence of the metric since it is exactly what is shared by empirical substructures and phenomena.

At least two objections may be raised here. The first would be to retort, in a Reichenbachian guise, that the geometry, i.e. the metric, is relative to a previous coordinative definition of congruence and that we are confronted with a (paradigmatic and abundantly discussed) case of underdetermination of theories by phenomena. Here, however, we would have to deal with *different* empirical substructures, each corresponding to a different definition of congruence (Reichenbach 1957) and not the embedding of the *same* empirical substructure in different models. Surely, the truth (or falsehood) of a statement depends on both the meanings of words and the observable phenomena. If the empirical meaning of the word "congruence" is changed, the structure of the description of phenomena will also have to be modified to preserve truth. This merely shows that phenomena do not impose a unique way of describing and embedding them. Granting this doesn't commit one to antirealism. Moreover, this kind of discussion is certainly too language-oriented for the model-theoretical approach.

Second, and more seriously I think, it can be argued that we don't observe the metric but only the behaviour of metrical devices or even only the (quasi-)coincidence of spacetime events. Such a radical empiricist position, which is not van Fraassen's, implies an extremely narrow construal of what experience or observation is. The main rejoinder to such a radical empiricism is to resort to our daily conviviality with phenomena and the common practice of scientists. To admit that we can observe —or at least empirically ascertain— the congruence of intervals does not amount to the desertion of the empiricist camp but the adoption of a more tolerant posture toward what can count as experience or observation. Now, congruence is mathematically expressed in the model by means of the metric tensor. If phenomena possess the same mathematical structure as parts of the models, phenomena show, or can be made to be showing after an appropriate training, *that* their structure is Minkowskian and therefore that the metric field exists. To oppose this would be tantamount to embracing antirealism at the level of phenomena.

What about the unobserved parts of spacetime? Admittedly, what precedes doesn't commit us to the existence of spacetime points, nor to the continuity of the manifold, but only to the existence of a metric at the spacetime locations where actual phenomena occur. The use of a *continuum* can be considered as an expedient device to perform the embedding. On the basis of the previous considerations, we must remain agnostic with respect to the existence of a continuous or discontinuous spacetime point-manifold. But should we remain agnostic with respect to the existence of a metric in uninvestigated spacetime locations?

Any arbitrary large —but of finite size— region, devoid of matter and energy, of spacetime can in principle be explored by rods and clocks, or traversed by free particles and light rays. Empty regions of spacetime are well-defined within the model (Minkowski spacetime) without any need for reification of those regions or their metrical structure. But then what is the difference between "occupied" and "non-occupied" regions of spacetime as far as their *structure* is concerned? None, in my opinion. They all share the same Minkowskian structure. The main point I want to make is that it is impossible in the present case to distinguish the empirical substructures from the non-empirical ones. Any finite portion of Minkowski spacetime counts as an empirical substructure, i.e. is a *candidate* for the representation of phenomena. The only —and unavoidable— underdetermination present is the underdetermination of the observ*able* relative to the observ*ed*. The acceptance of a theory-model always involves a certain amount of extrapolation, even at the empirical level. A given rod, say, can in principle be observed at any spacetime location and thereby render observable its local structure. (In fact, we know that in our physical world Minkowski geometry is approximately correct only in some regions and for phenomena which can be accounted for without resorting to a non-vanishing curvature (see Ghins and Budden (2001))). Recall that van Fraassen has no qualms in accepting the existence of observable objects like dinosaurs. If so, there seems to be no reason for him to deny the existence of Minkowskian structure, even in empty regions, since they count as the observable form of empirical substructures.

4. *The role of symmetries*

Given its close link with visual perception, geometry has always played a privileged role in science, particularly in physics, since its origins. The reach of geometry has considerably been enlarged by Félix Klein's Erlanger program (1872) according to which geometry is the study of symmetry or invariance groups acting on space (which can be extended to spacetime). Symmetry and invariance clearly are key concepts for model construction in modern physics.

The first step toward embedding phenomena is abstraction (van Fraassen 1989, p. 234). The same phenomena can be embedded in several, different ways, depending on which aspects of the phenomena are considered as belonging to the contents and which features are taken to be formal or structural. Once the formal aspects have been abstracted, we have obtained an empirical structure and we can proceed to the embedding of phenomena in wider model-theoretic structures. If we only have set-theoretical inclusion in mind, an empirical structure $\mathcal{E}$ can be included in a very large class of models $\mathcal{M}$: just put in the model ingredients —additional objects, for example— which may have nothing to do with the phenomena at stake. But such moves are devoid of interest and van Fraassen does put constraints on the construction of models which rest on pragmatic considerations, among which the predictive capacity of the theory is prominent. Symmetries play a decisive role in characterizing the empirical content and the predictive power of a theory.

> "A reflection on the possible forms or structures definable from joint experimental outcomes yields constraints on the general form of the models of the theories 'from below'; that class of models can then be narrowed down by the imposition of postulated general laws, symmetry constraints, and the like, 'from above'." (1989, p. 228).

As a constraint to model construction, I propose the following principle, which I call the Principle of invariance of the empirical substructures (PIES):

PIES: *The symmetry group of the embedding model must leave its empirical substructures invariant.*

This principle imposes an integration of the empirical substructures within the models in a stronger sense than mere set-theoretical inclusion since it requires a relation between the symmetries of the empirical substructures and the symmetries of the model as a whole. More precisely, PIES implies that the symmetry group E of the empirical substructures, which may be called the *empirical symmetry group*, can be larger or equal, but not smaller than the symmetry group S of the embedding models:

$$E \supseteq S$$

If we allowed the group S to be larger than E, then S would contain transformations which do *not* leave the form of the empirical substructures invariant. In such a situation, for any empirical substructure $\mathcal{E}$ there exists a transformation $s \in S$ such that:

$$s\mathcal{E} \neq \mathcal{E}$$

And $s\mathcal{E}$ may fail to accommodate the phenomena. Suppose $\mathcal{E}$ is embedded in a particular model $\mathcal{M}$. Then the transformed model $s\mathcal{M}$ may not be empirically adequate since it may not contain $\mathcal{E}$ as an empirical substructure.

To attempt to clarify the issue without becoming too technical, let's take a simple example, drawn again from geometry. Suppose the empirical substructures are invariant under the group of rigid motions (translations and rotations) which is the symmetry group of Euclidean geometry. If we tolerate models the structure of which is invariant under the whole group of linear transformations, some of these models won't be able to accommodate some observed Euclidean structures: the length of an interval, for instance, is not generally conserved under similarities, i.e. transformations which map a triangle into any triangle with the same angles. What would count as empirically true for a model would no longer be so for another isomorphic model. Thus, the symmetry group of the entire model can be smaller or equal but not larger than its empirical subgroup.

Moreover, in accordance with the spirit of an empiricist approach, it is natural to demand that the symmetry group of the model cannot be smaller than the empirical subgroup. Consider classical mechanics. Newton argued in favour of the existence of an absolute space which contains an invariant centre (occupied by the centre of gravity of the solar system). The invariance group of Newton's mechanics is a smaller group than the invariance group of Newtonian classical mechanics of point masses, since it includes only rotations around a privileged point, the "centre" of the world, and not rotations around any arbitrary point. It is well-known that no empirical way exists to find the location of this putative centre, which is better kept out of the realm of physics. (See for example Weyl 1963 p. 74, Friedman 1983 pp. 153–154). In general, the downsizing of the invariance group of the model will lead to the addition of invariant objects (properties and relations) and statements since as the size of the group decreases the number of its invariants increases. If we admit that it is a desirable feature of theory construction not to introduce new objects the relation of which to experience may be problematic, it is safer not to allow models the symmetries of which form a smaller group than the symmetry group of its empirical substructures. This proposal is not to be taken as an absolute constraint but rather as a guide to model construction, in conformity with an empiricist position. Together with PIES, this requirement implies that the empirical symmetry group be identical to the symmetry group of the entire model. I thus propose the following guiding principle, which may be called the Principle of symmetry for model construction (PS):

PS: *The symmetry group of the model must be identical to its empirical symmetry group*

As an illustration of this principle, which I take as a *local* constraint, let us discuss a simple example drawn from statics. Suppose that we set up, in a laboratory, the following device (see Figure 1) with pulleys, strings and bodies and that we observe that the bodies don't move relatively to each other and that the pulls cancel each other out.

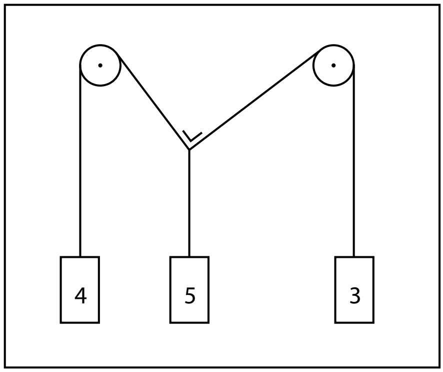

Figure 1

It can readily be shown that these observable phenomena are embeddable in a vector space where bodies exert pulls which can be isomorphically represented by vectors (see Figure 2).

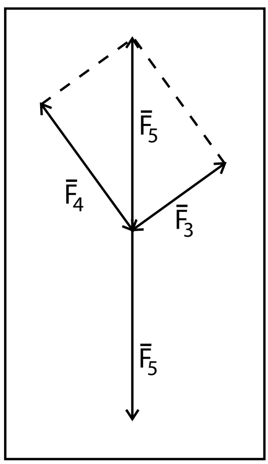

Figure 2

True, we directly observe pulleys, strings, pulls, bodies, etc. and not vectors. It is by abstraction that we isolate in the phenomena or the data a mathematical structure. Here the data are modelled in terms of vectors and vectorial sums. This data structure is isomorphic to an empirical substructure which is a part of a vectorial space. If, as van Fraassen contends, at the level of phenomena "empirical adequacy is tantamount to truth", observation provides evidence in favor of the existence of forces, i.e. vectors which can be measured by means of an apparatus like the one in Figure 1. Vectors and their relations determine the empirical substructure in this case which is "at once actual and observable". Granted, what we have here is a clear case of the celebrated thesis of the "impregnation of data by theories". Forces may be dubbed "theoretical" entities if desired, but this does not entail that they are unobservable. If we accept that the device presented above is observable, it can be embedded in an empirical and mathematical substructure in which pulls are represented by vectors. The state of equilibrium of the system is mathematically expressed by the statement that, at a point, the vectorial sum of forces is equal to zero. Therefore forces belong to the empirical substructures of the model and are real. Far from being a threat to scientific realism, the impregnation thesis supports and reinforces it. Embedding after all means showing that phenomena possess a structure identical to a part of a model and that they are, in this exact sense, theorized.

Models, together with their empirical substructures, are mathematical structures which possess symmetry or invariance properties. In a vector space, linear transformations act on vectors and leave their length invariant. In this sense, length is an objective property (Weyl 1963, p. 73) of vectors. An Euclidean vector space makes true some mathematical propositions (the axioms of vector space). But if mathematical expressions, which we will call 'laws', are unavoidable to characterize the model, axiomatization isn't indispensable and is even sometimes unavailable. Symmetry and invariance are relative to a set of laws and to a set of mathematical objects (properties, relations) which occur in those laws. It is important to draw a careful distinction between the invariance properties of *laws* and the invariance properties of the mathematical *objects*, such as forces, fields, charges and so on, which occur in the laws. In physics the symmetries of the models are the symmetries of the fundamental laws. This symmetry group then determines the way in which the objects are to be transformed. In statics, the law of equilibrium (for n forces) reads:

$$\sum_{i=1}^{n} \vec{F}_i = \vec{0}$$

The mathematical form of this law is invariant under Galilean transformations. Even if the components of the force vectors in equivalent reference frames may vary, their magnitudes remain constant.

5. *Cartesian versus Newtonian mechanics*

In *Laws and Symmetry*, van Fraassen briefly considers the transition from Cartesian to Newtonian mechanics. Cartesian mechanics was not deterministic: from the (supposedly exact) values of the initial positions and velocities of a point-mass it is not possible to univocally calculate its positions and velocities at some other times. If we want to "functionally" accommodate the phenomena of motion (van Fraassen 1989, p. 230) and obtain bigger predictive power, we have to inject additional structure or "hidden parameters" in the model (Newton added forces to the kinematic quantities which were the only ones used by Descartes), i.e. mathematical objects which, and this is essential, behave in accordance with specific mathematical formulae or laws. In physics, the requirement that we want to save more and more phenomena, demands, according to van Fraassen, a "widening of the theoretical framework", that is adding up new structure: the forces in the present case. "This method can be described in two ways: as *introducing hidden structure*, or 'dually' as *embedding*" (*Id.* p. 229). "The word "hidden" in "hidden parameters" does not refer to lack of empirical access. It signifies that we see parameters in the solution which do not appear in the statement of the problem." (*Id.*, p. 230). The problem, i.e. to achieve functionality, was solved by the introduction of "hidden parameters" (forces) which are indeed accessible to observation, as we saw, in the example drawn from statics.

Arguably, the invariance group of Cartesian mechanics can be taken to be the invariance group of the so-called Galilean law of inertia (which was actually first formulated by Descartes) and the Cartesian scalar law of collisions (for n bodies):

$$m\vec{v} = \vec{k}$$

$$\sum_{i=1}^{n} m_i v_i = \sum_{i=1}^{n} m_i v_i^*$$

Provided we assume the following three hypotheses: there exists an invariance group, spacetime is isotropic and homogeneous, the infinite velocity is invariant (for Descartes, the velocity of light was infinite), it can be shown (Berzi and Gorini 1969) that the invariance group of the law of inertia alone is the Galileo group. The scalar law of collisions restricts Galileo group to the group of Euclidean rigid motions (translations and rotations) and time translations. In other words, Cartesian mechanics is not invariant under boosts (except in the particular case of linear collisions in which the system is boosted in the direction of the initial —and final— velocities). The Euclidean group is also the invariance group of the structures considered empirical in Cartesian mechanics, namely the purely kinematical quantities.

To the law of inertia, Newton added the force-law (the fundamental law of dynamics or Newton's second law) and the action-reaction law:

$$\vec{F} = m\vec{a}$$

$$\vec{F}_A = -\vec{F}_R$$

The correct, deterministic, vectorial law of collisions is deduced from those two laws. The suppression of the scalar Cartesian law of collisions does lead to an enlarging of the symmetry group since the invariance group of the Cartesian law of inertia is already the Galileo group. In fact, forces must transform in such a way that the second law remains invariant under the Galileo group, that is, forces transform like vectors as already ensured by the statics.

Functionality is regained together with an enlarging of the global symmetry group. But this goes along with a broadening of the invariance group of the empirical substructures as well, which is also Galileo group. Actually, the structures of phenomena observed in systems in uniform relative motion are now all isomorphic to empirical substructures of Newtonian models according to the principle of relativity of Galilean mechanics. And forces belong, as vectors, to the empirical substructures. The new objects (forces) in Newtonian dynamics represent and are isomorphic to some observable phenomena (pulls). Forces may be added to regain functionality, but they are allowed in the model only because they can be correlated with phenomena within statics, a branch of mechanics which precedes (is logically prior to) kinematics and dynamics as Kant (1970) had already emphasized.

This example is illuminating in another respect. It shows that PS does not forbid the addition of new structure and new objects (forces in this case). It only requires that the new objects and the laws which hold for them do not lead to a downsizing of the symmetry group of the whole model with respect to the empirical symmetry group. This again is desirable from an empiricist standpoint. It makes possible in principle the inclusion of the new structure within the empirical substructure. Imagine that some newly introduced structure is not invariant under the empirical subgroup. In that event, its incorporation at the level of empirical substructures would render the theory empirically inadequate: transformations allowed by the theory would alter the empirical substructures which will then cease to be correct representations of phenomena. The introduction of forces not only led to the modification and enlargement of the symmetry group of the "Cartesian" model, but to an enlargement of the empirical substructures as well.

6. *The issue of the underdetermination of theories by phenomena*

The symmetry principle PS was introduced, as we saw, as a recommendation in accordance with the spirit of empiricism and not as an absolute demand. PS is taken to be a guide for theory construction rather than a strict requirement. Moreover, PS does not preclude the possibility of embedding the same phenomena in different, non-isomorphic, models, provided those distinct models share the same symmetries. The isomorphy of models implies the identity of their invariance group, but not conversely. Models with the same symmetry group may differ by the laws which hold in them and the objects which occur in those laws. In such a case we would be confronted to a genuine case of underdetermination of theories by phenomena. Then, an honest realist would have to withhold judgement about the truth of some laws and the existence of some objects (See McMullin 1984, p. 11, and Ghins 1992 for more on this viewpoint). Such a situation does not carry with it a widespread scepticism with respect to theoretical claims about unobservable processes in general. A painful thorn in the flesh of the realist would be the existence of a proof that alternative, distinct models which are empirically equivalent for all phenomena and satisfy the symmetry principle PS can *always* be constructed in principle. To my knowledge, such a proof, the burden of which rests on the shoulders of the antirealist, remains to be provided.

Let me stress again that the symmetry principle PS only has a *local* import. It does allow for the underdetermination of some global properties. Glymour (1977) and Malament (1977) have shown that, within the framework of current standard cosmology (Robertsonian models), all possible observations fail to univocally determine the global topology of spacetime. This doesn't preclude that local models have to satisfy PS. In fact, we know that in rather large regions of the universe we can safely use the special theory of relativity provided the phenomena studied are not too sensitive to spacetime curvature. We also know that the invariance group of the general theory of relativity, the group of diffeomorphisms, is wider than the Lorentz-Poincaré group. However, this doesn't prevent us to embed local phenomena in Minkowski spacetime as is guaranteed by the Principle of equivalence. In other words, locally, the symmetry principle holds. And the local validity of the special theory of relativity imposes constraints on the kind of metric which is globally acceptable. More precisely the metric must be Riemannian, 4-dimensional and of negative signature (See Brown 1997).

Of course, the possibility of giving alternative and incompatible *metaphysical* interpretations of the same theory remains open. For example, we could ask ourselves if there exists some sort of substantival spacetime or if spacetime points are individuals. Such moves are neither allowed nor forbidden by the brand of moderate and selective scientific realism advocated here (see also Ghins 1992), which confines its existential claims to physical objects only to the extent that they are represented by invariant mathematical objects in the context of empirical substructures of models which, on top of satisfying the symmetry principle PS, are also empirically adequate.

Centre de philosophie des sciences
Institut Supérieur de Philosophie
Place du cardinal Mercier, 14
B-1348 Louvain-la-Neuve, Belgium
E-mail: ghins@risp.ucl.ac.be

REFERENCES

BERZI, V. and GORINI, V. (1969), "Reciprocity Principle and the Lorentz Transformations", *J. of Math. Physics* 10, 8, 1518–24.

BROWN, H. R. (1997), "On the Role of Special Relativity in General Relativity", *International Studies in the Philosophy of Science*, 67–81.

FRIEDMAN, M. (1983), *Foundations of Space-Time Theories.* Princeton University Press.

GEMIGNANI, M. C. (1971), *Axiomatic Geometry*, Addison-Wesley.

GHINS, M. (1992), "Scientific Realism and Invariance". *Proceedings of the Third SOFIA Conference on Epistemology. Campinas. July 30–August 1, 1990. Philosophical Issues (Vol. 2: Rationality in Epistemology).* 249–62. California: Ridgeview.

GHINS, M. and BUDDEN, T. (2001), "The Principle of Equivalence". *Studies in History and Philosophy of Modern Physics.* Vol. 32B, n. 1. March 2001, 33–51.

GLYMOUR, C. (1977), "Indistinguishable Space-Times and the Fundamental Group" in J. Earman, C. Glymour, J. Stachel. *Foundations of Space-Time Theories. Minnesota Studies in the Philosophy of Science. Vol. VIII.* Minneapolis. 50–60.

KANT, I. (1970), *Metaphysical Foundations of Natural Science.* (Transl. by J. Ellington). Indianapolis.

MALAMENT, D. (1977), "Observationally Indistinguishable Space-times: Comments on Glymour's Paper" in J. Earman, C. Glymour, J. Stachel. *Foundations of Space-Time Theories. Minnesota Studies in the Philosophy of Science. Vol. VIII.* Minneapolis. 61–80.

McMULLIN, E. (1984), "A Case for Scientific Realism" in Leplin, J. (ed.) *Scientific Realism.* Berkeley: University of California Press.

REICHENBACH, H. (1958), *The Philosophy of Space and Time.* New York: Dover.

SUPPE, F. (1989), *The Semantic Conception of Theories and Scientific Realism.* University of Illinois Press.

VAN FRAASSEN, B. (1980), *The Scientific Image.* Oxford University Press.
VAN FRAASSEN, B. (1989), *Laws and Symmetry.* Oxford University Press.
WEYL, H. (1963), *Philosophy of Mathematics and Natural Science.* New York: Atheneum.