Scientific Theories, Models, and the Semantic Approach

Décio Krause & Otávio Bueno
Federal University of Santa Catarina / University of Miami

Abstract

According to the semantic view, a theory is characterized by a class of models. In this paper, we examine critically some of the assumptions that underlie this approach. First, we recall that models are models of something. Thus we cannot leave completely aside the axiomatization of the theories under consideration, nor can we ignore the metamathematics used to elaborate these models, for changes in the metamathematics often impose restrictions on the resulting models. Second, based on a parallel between van Fraassen's modal interpretation of quantum mechanics and Skolem's relativism regarding set-theoretic concepts, we introduce a distinction between relative and absolute concepts in the context of the models of a scientific theory. And we discuss the significance of that distinction. Finally, by focusing on contemporary particle physics, we raise the question: since there is no general accepted unification of the parts of the standard model (namely, QED and QCD), we have no theory, *in the usual sense of the term. This poses a difficulty: if there is no* theory, *how can we speak of its models? What are the latter models of? We conclude by noting that it is unclear that the semantic view can be applied to contemporary physical theories.*

1. Considerations on the semantic approach

As is well known, the motto of the semantic approach to scientific theories is that a theory is characterized by a class of models. The word "model" is, of course, used in distinct ways in the current literature. According to Suppes, the main forerunner of the semantic view, the various kinds of 'models' we consider, e.g. in biological and social sciences, in applied mathematics and in other areas, can be reduced to set-theoretic models, that is, to mathematical structures satisfying the theory's postulates (or equivalently, satisfying the set-theoretic predicate that axiomatizes the theory). As he says, "a possible realization of a theory is a set-theoretic entity of the appropriate logical type" (Suppes 2002, p. 21). Van Fraassen acknowledges this point by noting that "[a]ny structure that satisfies the axioms of a theory [...] is called an *model* of that theory" (van Fraassen 1980,

Principia, **11**(2) (2007), pp. 187—201. Published by NEL — Epistemology and Logic Research Group, Federal University of Santa Catarina (UFSC), Brazil.

p. 43). However, he seems to take the point back, when he asserts that "[t]he semantic view of theories makes language irrelevant to the subject", and that Suppes' idea is that "*to present a theory is to define a class of its models directly*, without paying any attention to questions of axiomatizability" (van Fraassen 1989, p. 222, emphasis in the original).

But it's important to acknowledge that models are models of something, and that in Suppes' approach (which van Fraassen endorses in part), the models in question are models of a set-theoretic predicate (a suitable formula written in the language of set theory) which stands for the conjunction of the theory's postulates. Suppes' slogan is that "*to axiomatize a theory is to define a set-theoretic predicate*" (2002, p. 30). Thus, language *is* of fundamental importance in this approach, and so are the theory's postulates. Without the latter, there are no models of a theory, for there are no models *tout court*. The models must be collected in some way to form the extension of the relevant set-theoretic predicate. In general, we have a proper class whose elements are precisely the models of the predicate.

Although the point can be resisted, it is generally agreed by the defenders of the semantic view that the relevant models are mathematical structures. But mathematical structures are built in a suitable mathematical framework. Depending on the theory we are considering, we have several possible alternatives for this (meta)mathematical framework, such as higher-order logics or category theory. However, typically these mathematical structures are set-theoretic, that is, built in a certain set theory. Usually, the framework that is employed is *informal* (non-axiomatized) set theory. But if pressed, the scientist can turn, say, to ZF (Zermelo-Fraenkel) set theory. (By the way, this point was made by Patrick Suppes himself in conversation with one of us, DK.)

Thus, we should acknowledge that the models of a certain scientific theory T are usually built in a certain set theory. For the sake of precision and without loss of generality, we can assume that set theory to be ZF. In this case, "classical particle mechanics" (that is, the models of a classical particle mechanics) emerges from structures of the form $\langle P, \overrightarrow{s}, m, \overrightarrow{f}, \overrightarrow{g} \rangle$, where P is the set of "particles", $\overrightarrow{s}$ is the position function, m is the mass function, $\overrightarrow{f}$ stands for the internal forces, and $\overrightarrow{g}$ represents the external force function—all of them obeying certain postulates (Suppes 2002, pp. 319ff). As for non-relativistic quantum mechanics, a mathematical structure that can be taken as a model of this theory is: $\langle M_0, S, Q_0, \ldots, Q_n, \rho \rangle$, where M_0, the mathematical part of the structure, is a model of standard functional analysis, while $\langle S, Q_0, \ldots, Q_n \rangle$ is the "operative part" of the structure, and ρ is an interpretation function that assigns an element of M_0 to each element of the operative part—once again, each of these components also obey specific postulates (Dalla Chiara and Toraldo di Francia 1981, p. 85). As we see, *there is* sensitivity to language, and there is a (meta)mathematical framework in which these structures are built.

But this approach faces considerable problems, particularly if we take into account contemporary physics. Here, we will only raise the issues without detailed discussion. Our first problem concerns the mathematics used in the metatheory; that is, the set theory we employ to build the models of a given theory. Let's consider an example. An important concept in quantum mechanics is that of an unbounded operator. For instance, the position and momentum operators in the Hilbert space $L^2(\mathbb{R})$ of the equivalence classes of square integrable functions are unbounded; that is, if A is an operator, then for any $M > 0$ there exists a vector α such that $\|A(\alpha)\| \geqslant M\|\alpha\|$. However, consider the theory ZF+DC, where DC stands for a weakened form of the axiom of choice entailing that a 'countable' form of the axiom of choice can be obtained. (In particular, if $\{B_n : n \in \omega\}$ is a countable collection of nonempty sets, then it follows from DC that there exists a choice function f with domain ω such that $f(n) \in B_n$ for each $n \in \omega$.) It can then be proven, as Solovay showed, that in ZF+DC (which is supposed to be consistent) the proposition "Every subset of $\mathbb{R}$ is Lebesgue measurable" cannot be disproved. This proposition is false in standard ZFC. The same happens with the proposition: "Each linear operator on a Hilbert space is bounded" (Maitland Wright 1973). This kind of result poses a difficulty to the defenders of the semantic view: when we speak of the models of a scientific theory, such as quantum mechanics, which metamathematics should we use to define its models? Presumably, it cannot be Solovay's model in ZF+DC, since we need unbounded operators. So, the choice of a suitable metamathematics is crucial.

Here is another example. In the standard Hilbert space formalism, we deal with bases for the relevant Hilbert spaces. More specifically, we deal with orthonormal bases formed by eingenvectors of certain Hermitean operators. This is possible because we can prove, using the axiom of choice (which is part of the metatheory used here) that any Hilbert space H has a basis. Moreover, it can also be shown that each basis has a specific cardinality, which is the same for all bases of H (this is defined to be the dimension of the space). But in certain set theories in which the axiom of choice does not hold in full generality, such as in Laüchi's permutation models, we obtain: (a) vector spaces with no basis, and (b) a vector space that has two bases of different cardinalities (Jech 1977, p. 366). Now, if a vector space has no basis, it cannot be used as part of the standard formalism of quantum mechanics. The latter formalism presupposes the availability of suitable bases. As a result, the formalism depends crucially on the metamathematics that is used.

It should be noted that, despite all the discussion about the concept of 'model' of a physical theory in the literature, the precise characterization of this concept remains elusive. Model theory, which has been the inspiration for much that has been said on models of scientific theories in general, articulates the notion of a model for *formal* first-order axiomatic systems only. Due to the fact that fundamental theorems, such as compactness, completness, and Löwenheim-Skolem, do not hold in higher-order logics (with standard semantics), we can say that there is no higher-order model theory. But scientific theories, in general, are described only informally (consider, for instance, the theories in biology), and involve more than first-order languages. As a result, we don't have a corresponding well defined "model theory" in such cases.[1] Despite this, a *model* for a scientific theory in standard texts on the semantic view, has been typically taken in its "first-order" sense, roughly, as a set-theoretic structure that satisfies the axioms of the theory. Now, suppose that we are considering theories that are stronger than first-order theories. Which metamathematical framework should we use to describe their models? If we do not specify the metatheory that we are using, we cannot guarantee that that certain entities—such as certain models—that we assume that exist do in fact exist. Furthermore, important concepts, such as the concept of truth (Tarskian or not), will depend on the metamathematics too. To know the features of the metamathematics that is used in the understanding of scientific theories seems to be central in philosophy of science.

2. Skolem's and van Fraassen's paradox

Let us suppose that we have somehow solved the problem just raised, and so, we have offered grounds to choose a suitable set theory, such as ZF (more precisely, a particular model of ZF), as our metamathematical framework.[2] Thus, we may assume that we have a set-theoretic predicate and a class of models for this predicate. We can now raise a problem related to Skolem's concept of relative and absolute concepts in set theory. But let us first contextualize the problem.

In the 1920's, Thoralf Skolem realized that there are concepts that are, as it were, the same in all models of, say, ZF (which is supposed to be consistent). For instance, the concept of 'ordinal' does not change from model to model as the concept of 'cardinal' does. For example, if ZF is formulated as a first-order theory (as Skolem himself supposed), if consistent, it will have a countable model due to the Löwenheim-Skolem theorem. But, in this model, the set of real numbers, which can be constructed in ZF, must be countable—a fact that apparently contradicts Cantor's theorem, according to which there is no bijection between the set of real numbers and the set of natural numbers. However, as Skolem himself noted, this result does not lead to a "real" paradox, for the bijection must exist *outside* the countable model (Skolem 1922). This result, known as "Skolem's paradox", shows that the set of real numbers may have different cardinalities depending on the model we consider, and this happens in general for other sets. The concept of cardinal is *relative* (to the model under consideration), while the concept of ordinal (which is "the same" in all models) is *absolute*.

We will not examine here the formal definitions, keeping the discussion at an intuitive level. But let us just give a short account on the relevant concepts by considering a countable transitive model M of ZFC (Zermelo-Fraenkel set theory with the axiom of choice). A formula $\varphi(x,y)$ is *absolute* if for $a,b \in M$, we have that $M \models \varphi(a,b)$ iff $\varphi(a,b)$ is *in fact true* (Burgess 1977, p. 408). For example, the following formulas are absolute: $y = \bigcup x$, $z = x \cap y$, $z = x \cup y$, and $z = \{x,y\}$ (these expressions can be rewritten as formulas of ZFC, satisfying the required condition). However, $y = \mathcal{P}(x)$ is not absolute. After all, y may be the set of all subsets of x in the model M, without being the true power set of x. So, $card(x) < card(y)$ is not absolute, for even if there is no one-one mapping of x onto y in M, this does not imply that the mapping does not exist (recall Skolem's paradox). How can these points be applied to our discussion of scientific theories and their models?

To translate these points, that make sense in a precise context, to a general discussion of theories and models in science is not a straightforward problem. But, as will become clear, we can explore certain aspects of the technical results mentioned above. Let's consider a situation that is similar to the one above involving models of ZF. After having presented his modal interpretation of quantum mechanics, Bas van Fraassen addresses the problem of identical particles in quantum physics, which he regards as one of the three main issues in the philosophical discussion on quantum mechanics (see van Frassen 1991, p. 193). And he notes: "identical particles [...] are certainly qualitatively the same, in all the respects represented in quantum-mechanical models—yet still numerically distinct" (1991, p. 376). In a previous paper, he was still more explicit, insisting that "if two particles are of the same kind, and have the same state of motion, nothing in the quantum-mechanical description distinguishes them. Yet this is possible." (van Fraassen 1984)

Van Fraassen's quotations are intriguing. Particles of the same kind and in the same state of motion are 'identical', in the physicists' jargon, and according to their standards, nothing can distinguish them. So, if they cannot be distinguished *in* the quantum-mechanical formalism, how can they still be distinguished at all? The answer, we suggest—following the parallel case made by Skolem in set theory—is that the particles can be distinguished *outside* the framework of quantum mechanics. But what does this mean? As we have seen, in the foundations of set theory, considerations regarding what holds inside or outside a certain model, are quite common. But can we make sense of this way of speaking in philosophy of science as well?

In order to answer this question, recall that van Fraassen's modal interpretation of quantum mechanics takes quantum propositions as modal statements, which give "first and foremost about what can and what must happen, and only indirectly about what actually does happen" (van Fraassen 1980). In other words, the modal account, by offering an interpretation of quantum mechanics, spells out how the world could be if quantum mechanics *were* true (van Fraassen 1991, p. 242). To motivate his proposal, van Fraassen recalls one of the most intriguing features of quantum physics, namely, the sense in which quantum mechanics is an indeterministic theory. Although the dynamics of an isolated system evolves according to Schrödinger's equation (hence deterministically), the system as a whole cannot be analyzed in terms of its component parts. So, apparently, the quantum mechanical state of the whole system contains only incomplete information about the system. Bohr's proposal, recalls van Fraassen, emphasizes that it is still possible to have complete information about the system, given that the states of the system's components and the state of the whole system do not determine each other (van Fraassen 1980). As a result, on the basis of the state of a complete system $X + Y$, we can in general ascribe at most mixed states to X and Y, but from them nothing can be said back about the state of the whole system. As van Fraassen notes:

> [I]f we can predict the future states of an isolated system on the basis of its present state [by means of Schrödinger's equation], then how can we be ignorant about the future events involving its components unless the information in those total states is incomplete? For surely any true description of a component is a partial but true description of the whole? (*ibid.*)

Van Fraassen's answer is obtained from a distinction between quantum dynamical states and experimental events. The former are what a vector or a statistical operator represents. They are things completely *embedded* in the theory, whose evolution is governed by dynamical laws. In other words, we can say that dynamical states are described *in* the formalism of quantum mechanics. Events, on the contrary, are *extra-theoretic* entities that satisfy the probability calculations.

The same conceptual distinction can be drawn by distinguishing between *state attributions* and *value attributions* of a physical system. The former is a theoretic construct, and part of the challenge involved in theory's construction depends upon a proper representation of these states. Value attributions, in turn, express values that an observable actually have. Since the point is important for our argument, let us consider it in more detail.

A value state is specified by stating which observables have values and what they are. A value-attributing proposition then states that an observable m actually has a value in a (Borel) set E. (In symbols, $\langle m, E\rangle$.) The connection between them is that value states are truth-makers of value attributing propositions (van Fraassen 1991, pp. 275–6). On the other hand, we have the dynamic state, which states how the system will evolve, either in isolation or in interaction with another system. A state-attributing proposition then states that a measurement of an observable m must have a value in a (Borel) set E. (In symbols, $[m, E]$.) Again, dynamic states and state-attributing propositions are connected by the fact that the former are what make the latter true. Now, the crucial feature of the modal account is to distinguish value- and state-attributing propositions. The motivation for this distinction comes from difficulties faced by the standard interpretation of quantum mechanics, as articulated by von Neumann, for not distinguishing them (see van Fraassen 1991, and Bitbol 1996).

Von Neumann's interpretation of quantum mechanics identifies these two concepts. After all, not only von Neumann considers that a system can be said to posses a value of a certain variable when it is in an eigenstate of the corresponding observable, but he also accepts that if the state vector is not an eigenstate of some observable, then it has no value at all (van Fraassen 1991, and Bitbol 1996, p. 149). In this case, the system is supposed to be characterized by a well-defined value of the observable when the probability is 1. But if this probability is not 1, then the observable is supposed to have no value at all. To remove this discontinuity, van Fraassen offers an account according to which probability ascriptions are not equivalent to value ascriptions (1991).

On von Neumann's interpretation, attributions of values and classification of states are closely related: an observable B has value b if and only if a B-measurement is certain to have outcome b (where b is a real number). The problem here is that in order to accommodate states for which measurement has uncertain outcomes, von Neumann made a radical move: if the outcome of a measurement of B is uncertain, B has no value at all (van Fraassen 1991, p. 274). To avoid this answer, the modal interpretation introduces the distinction between values and states. With this distinction in place, the introduction of 'unsharp' values of observables is allowed. And this is how the possibility of uncertain outcomes in measurement can be accommodated.[3] As van Fraassen points out (1991, pp. 280–1), if a physical system X has dynamic state (represented by an operator) W at a time t, the state-attributions $[M, E]$ which are true are those such that $Tr(WI_E^M) = 1$.[4] As opposed to state-attributions, value-attributions cannot be deduced from the dynamic state. But, according to van Fraassen, they are constrained in three ways:[5] (i) If $[M, E]$ is true, so is the value-attribution $\langle m, E\rangle$; that is, observable M has value in E; (ii) all true value-attributions could have probability 1 together; and (iii) the set of true value-attributions is maximal with respect to feature (ii) (see van Fraassen 1991, p. 281). So, the assignment of truth-conditions to state- and value-attributing propositions is crucial to spell out the difference between them (the former, but not the latter, can be deduced from the dynamic state).

To sum up, there is an important distinction between state attribution and value attribution, or between states and events, and this distinction cannot be reduced to something more basic. States, as already noted, are described *in* the scope of (the formalism of) quantum mechanics by vectors of an appropriate Hilbert space, while events are not. After all, events are statements such as: *Observable B pertaining to system X has value b*, and such events are described if they are assigned probabilities, but "they are not the same thing as the states which assign them probabilities" (van Fraassen 1991, p. 279).

The distinction between states and events is similar to the distinction between absolute and relative notions in set theory discussed above, at least in the following way: we are contrasting *intra-theoretic* properties with properties that hold *outside* the models under consideration. It's curious that when Skolem introduced his 'paradox', he intended to use it to show the inadequacy of set theory as a foundation for mathematics. The outcome, however, was precisely the opposite. His result was incorporated as part of the rich conceptual framework offered set-theoretic notions. Similarly, van Fraassen developed the modal interpretation of quantum mechanics as part of a defense of an empiricist view. In the end, however, the modal interpretation became part the revival of realist interpretations of quantum theory.

It should now be clear that both in the philosophy of science and in the foundations of set theory there is room for discussing what holds "inside" a particular model (or formalism) and what holds "outside" the model (formalism). If a theory is presented as a class of models, it makes perfect sense to ask whether there are concepts that remain the same in all models, and concepts that change from model to model; that is, that have a certain extension "inside" a model, but a different one in another model or when considered outside the model.

To begin the search for examples, let's examine a tentative case. Consider the concept of indistinguishable (or indiscernible) object. The idea of indiscernibility is of fundamental importance in contemporary physics (for a historical account and further discussion, see French and Krause 2006). Standard mathematics and classical logic imply that every object is an individual, in the sense that each object can always be distinguished from any other.[6] As a result, to accommodate indistinguishable objects some mathematical trick needs to be introduced.

In quantum physics, this is done by imposing some kind of symmetry condition. Suppose we are to describe how two identical bosons, 1 and 2, can be distributed in two possible states, A and B. As is well known, the vectors in the relevant Hilbert space are $|\psi_1^A\rangle|\psi_2^A\rangle$, which states that both bosons are in A; $|\psi_1^B\rangle|\psi_2^B\rangle$, which states that both are in B, and $\frac{1}{\sqrt{2}}|\psi_1^A\rangle|\psi_2^B\rangle + \frac{1}{\sqrt{2}}|\psi_2^A\rangle|\psi_1^B\rangle$, which states that *one* of them is at A and *the another* is in B. Thus, the indistinguishability between 1 and 2 (in the third case) emerges from the symmetry of the function, which is invariant by permutations of the labels. Some people claim that the individuality of quantum objects is then lost. According to our point of view, there is nothing to lose, for in one of the possible approaches to the subject, these objects do not have identity to begin with (see French and Krause 2006). The artificiality of the problem is that these objects were first assumed to be individuals by their labels 1 and 2. Thus, by an adequate choice of the relevant vectors, *we have made them* indiscernible. However, the objects cannot be said to be indiscernible *outside* the framework, since *we can* distinguish them—e.g., by their labels 1 and 2. In this way, the notion of indistinguishable object seems to be *relative*.

The mathematical trick we used consists in limiting the discourse to the scope of a certain set-theoretic structure (as we saw, the models of quantum physics can be taken to be such kind of structure). We then consider as indiscernible those objects that are invariant by the automorphisms of the structure. Now, in ZF any structure can be extended to a rigid structure, that is, to a structure where the only automorphism is the identity function. Hence, in the rigid structure (the *whole* ZF "model" $\mathcal{Z} = \langle Z, \in\rangle$, seen as structure, is rigid), any object is an individual. In short, there are no truly indiscernible objects in standard mathematics (and logic).

Of course, we need to find more conclusive examples of absolute and relative concepts in particular scientific theories. And to do that, the first step is to characterize the relevant concepts in a precise way. Since the present paper is just a preliminary piece, in which we just outline the main problems, we leave the emerging details for another occasion.

3. Driving without knowing how the car works

In discussing whether quantum field theory (QFT) needs a foundation, the Nobel Prize winner Stanley Lee Glashow notes that, for many particle physicists, QFT is just a useful tool, which is used without much concern for its logical foundation. And he suggests that physicists generally work just like someone who "drives without knowing how the car works" (Glashow 1999, p. 77). The link between using a mathematical model and establishing it as a theory *stricto sensu* is clearly described by him as follows:

> [A] theory cannot become an established part of the scientific edifice until, first, its implications are shown to accord with experiment, and, second, its domain of applicability is established. Newtonian mechanics is absolutely true—within a well-defined envelope defined by c and $\hbar$. Similarly for classical electrodynamics and non-relativistic quantum mechanics. Like its predecessors, quantum field theory offers—and will always offer—a valid description of particle phenomena at energies lying within its own domain of applicability. This domain extend all the way to the Planck scale, but its limits of applicability have yet not be probed. From this point of view, we are discussing the foundations of a theory that, whatever its successes, cannot be accepted as true (Glashow 1999, p. 77).

The reason why Glashow thinks that QFT is not true is that the standard model does not encompass gravitation. At the Planck scale (near $10^{-33}cm$ and high energies), gravitation becomes important, but the unification between the standard model of particle physics and general relativity has not been achieved yet. Some physicists suggest that a new theory needs to be developed, and they indicate possible directions to take, such as string theories, which describe "new" symmetries called *supersymmetries*, and quantum gravitation. The future of physics will show whether these proposals work.

The important point to us is that the standard model of particle physics—which describes the weak, electromagnetic and strong interactions between the most basic constituents of matter, leptons and quarks (Cottingham and Greenwood 2007)—is formed by mainly two apparently irreconcilable “theories” (or mathematical models): quantum electrodynamics (QED) and quantum chromodynamics (QCD). Both QED and QCD are used with great success, but physicists acknowledge that there is no Grand Unified Theory (GUT) that satisfactorily unifies both of them. In other words, we know quite well the symmetry gauge group $U(1) \times SU(2)$ of QED, and the group $SU(3)$ of QCD. However, the unified group $U(1) \times SU(2) \times SU(3)$ is still a mystery in the sense that the resulting theory, whose laws would be invariant under this group, offers several consequences not yet fully explained. Some physicists have proposed alternatives to this unifying group, such as Glashow’s $SU(5)$, but there is no general agreement about this proposal. Glashow himself says that this “theory” is false, given that protons live more than what is predicted by $SU(5)$ (Glashow 1980). This means that we have no *theory* of the standard model in the strict sense discussed above.

Even if we consider only QED, we should probably agree with Arthur Jaffe in that “[a]s a consequence of renormalization, most physicists today believe that the equations [we would say, “the postulates”] of quantum electrodynamics in their simple form are inconsistent; in other words, we believe that the equations of electrodynamics have no solution at all!” (Jaffe 1999, p. 136).[7] A similar point could be made about to the so-called M-theory, which would unify superstring theories, and which is recognized as not complete, although it can be applied to many physical situations.

All of this shows that scientists work by applying particular “mathematical models” (a term that, in this context, is better than “theory”) to particular situations—sometimes without paying attention to the fact that there are no “theories”, in a strict sense, behind the mathematical frameworks they use. In other words, we are driving to beautiful landscapes without the knowledge of how our vehicle works. For instance, Tian Cao acknowledges that the unification achieved by the standard model is only partial, for “the electroweak theory and quantum chromodynamics (QCD) for the quark-gluon interaction are still separate pieces” (Cao 1999, p. 1). This apparent incompatibility does not stop physicists of using both of them. As the Nobel Prize winner David Gross notes: “there may be more than one, equally fundamental, formulation of a particular QFT, each appropriate for describing physics at a different scale of energy” (1999, pp. 59–60). It seems that the axiomatization of theories (i.e. their presentation as being grounded on principles or postulates) seems to come only after their application in science; that is, after the application of mathematical models suitable for specific situations.

Yuri Manin seems to be right here: we have learned much about formalisms in the 20th century, but it is time to look to the world again—in order to get additional motivations for mathematical theories (Manin 1976). The current situation in physics seems to suggest that to formulate theories in the formal way—that is, by presenting formally their language, axioms, and underlying logics—seems to be an important goal to be pursued. But the risk is that, due to quick progress in physics, the issue becomes a dead matter. To quote Cao once more, perhaps we should agree that "a *completely* consistent theory [that is, one formulated according to the strict logical standards] is a *dead* theory" (1976, p. 281). We might even suggest that, due to the quick development of science and due to the difficulties in developing axiomatic versions for new theories (the standard model, M-theory, etc.), scientists work as if the mathematical models were kinds of mosaics to be placed together, even inconsistently, to solve particular problems, or to "cover" a particular field of knowledge. As an illustration, recall Bohr's theory of the atom, which combines classical mechanics, electrodynamics and quantization in an inconsistent way. In some cases, such as in Bohr's theory, we can formulate the theory in an axiomatic form, perhaps by using a non-classical logic—in the case of Bohr's theory, a paraconsistent one (see da Costa et al. 2007). However, in order to combine QED and QCD, no proposed GUT has been universally accepted. The same difficulty is found, as is well known, with quantum physics and general relativity.

Now, in cases such as these, if there are no theories, in a strict sense, how can we speak of models? Of course, we can say that QCD applies to situations involving high energies where asymptotic degrees of freedom are weakly coupled (Gross 1999, p. 60). But in terms of models and the semantic approach, what does this statement really mean? Can we simply say that all weak interactions are *models* of QED? It seems that something is lacking here. In the end, it seems to us that the semantic view of theories needs to be re-conceptualized in light of current physics.

References

Barwise, J. (ed.) 1991. *Handbook of mathematical logic*. Amsterdam: North-Holland. (First edition, 1977.)

Bitbol, M. 1996. *Schrödinger's philosophy of quantum mechanics*. Dordrecht: Kluwer Academic Publishers.

Bourbaki, N. 1958. *Théorie des ensembles*. Paris: Hermann.

Bueno, O. 2000. Quasi-truth in quasi-set theory. *Synthese* **125**: 33–53.

Burgess, J. P. 1977. Forcing. In Barwise 1977, p. 403–52.

Cao, T. Y. (ed.) 1999. *Conceptual Foundations of Quantum Field Theory*. Cambridge: Cambridge University Press.

Cottingham, W. N. and Greenwood, D. A. 2007. *An Introduction to the standard model of particle physics*. Cambridge: Cambridge University Press.

da Costa, N. C. A.; Krause, D.; Bueno, O. 2007. Paraconsistent Logics and Paraconsistency. In D. Jacquette (ed.) 2007. *Philosophy of Logic*. Amsterdam: Elsevier, 791–911.

Dalla Chiara, M. L. 1985. Some foundational problems in mathematics suggested by physics. *Synthese* **62**: 303–15.

Dalla Chiara, M. L. and Toraldo di Francia, G. 1981. *Le teorie fisiche: Un'analisi formale*. Torino: Boringhieri.

Fraenkel, A. A.; Bar-Hillel, Y.; Levy, A. 1973. *Foundations of set theory*. Amsterdam: North-Holland.

French, S. and Krause, D. 2006. *Identity in Physics: A Historical, Philosophical, and Formal Analysis*. Oxford: Oxford University Press.

Glashow, S. L. 1999. Does quantum field theory need a foundation? *In* Cao 1999, p. 74–88.

——. 2000. Grand unfication. *New Scientist*, September 18, 1980. (Our references are from 'La gran unificación', in S. L. Glashow, *El Encanto de la Física*, Tusquets Editores, 2000, pp. 271–80.)

Gross, D. 1999. The triumph and llimitations of quantum field theory. *In* Cao 1999, 56–67.

Jaffe, A. 1999. Where does quantum field theory fit into the big picture? *In* Cao 1999, 136–47.

Jech, T. 1977. About the axiom of choice. In Barwise 1977, p. 345–70.

Kline, M. 1972. *Mathematical thought: from ancient to modern times*, v. 3. Oxford: Oxford University Press.

Maitland Wright, J. D. 1973. All operators on a Hilbert space are bounded. *Bulletin of the Americal Mathematical Society* **79**(6): 1247–50.

Manin, Yu. I. 1977. *A Course in mathematical logic*. New York: Springer-Verlag.

McCall, S. 2001. Axiomatic quantum theory. *Journal of Philosophical Logic* **30**: 465–77.

Skolem, T. 1967. Some remarks on axiomatized set theory. In J. van Heijenoort (ed.) *From Frege to Gödel*. Harvard University Press: 290–301. (The original paper was published in 1922.)

Suppes, P. 2002. *Representation and invariance of scientific structures*. Stanford: CSLI, Stanford University.

van Dalen, D. and Ebbinghaus, H.-D. 2000. Zermelo and the Skolem Paradox. *The Bulletin of Symbolic Logic* **6**: 145–61.

van Fraassen, B. 1980. *The scientific image*. Oxford: Clarendon Press.

——. 1981. A modal interpretation of quantum mechanics. In E. G. Beltrametti and B.

van Fraassen (eds.) *Current issues in quantum logic*. New York: Plenum Press, p. 229–58.

——. 1998. The problem of indistinguishable particles. In J. T. Cushing, C. F. Delaney, and G. M. Gutting (eds.) *Science and reality: recent works in the philosophy of science*. Notre Dame: University of Notre Dame Press, 1984. (Reprinted in E. Castellani (ed.) *Interpreting bodies: classical and quantum objects in modern physics*. Princeton University Press, 1998, p. 73–92.)

——. 1989. *Laws and symmetry*. Oxford: Clarendon Press.

——. 1991. *Quantum mechanics: An empiricist view*. Oxford: Clarendon Press.

Keywords

Semantic view, Skolem's paradox, modal interpretation, theories, particle physics.

Décio Krause
Department of Philosophy
Federal University of Santa Catarina
dkrause@cfh.ufsc.br

Otávio Bueno
Department of Philosophy
University of Miami
otaviobueno@mac.com

Resumo

De acordo com a abordagem semântica das teorias científicas, uma teoria é caracterizada por uma classe de modelos. Neste artigo, discutimos algumas hipóteses que subjazem a essa concepção. Em primeiro lugar, recordamos que modelos são modelos de algo, e portanto não podemos desconsiderar a axiomatização das teorias consideradas, bem como a metamatemática usada para elaborar esses modelos, uma vez que uma mudança na metamatemática pode ocasionar restrições nesses modelos. Em seguida, baseados em um paralelo possível entre a interpretação modal da MQ de van Fraassen e o relativismo dos conceitos conjuntistas de Skolem, sugerimos que deveríamos considerar, também no que diz respeito às teorias e seus modelos, uma possível distinção entre conceitos relativos e absolutos. Finalmente, levando em conta a presente física de partículas, colocamos uma questão que nos parece básica: uma vez que não há unificação aceita universalmente das partes que constituem o chamado modelo standard, *a saber, a eletrodinâmica quântica e a cromodinâmica quântica, não temos aqui uma* teoria, *no sentido usual que se emprega esse termo nas* *discussões em filosofia da ciência. Isso coloca um problema: se não há* teoria, *como falar em seus modelos? Modelos de quê? Concluímos observando que não é claro como a visão semântica das teorias científicas se aplica às teorias físicas de hoje.*

Palavras-chave
Abordagem semântica, paradoxo de Skolem, interpretação modal, teorias, física de partículas.

Notes

[1] It has being claimed that the first (first-order) *formal* axiomatization of orthodox quantum mechanics was proposed in McCall 2001. We will not discuss this issue here.

[2] When we speak of a "model" of a set theory such as ZF, we are *not* thinking of set-theoretical structures, such as a structure $\mathcal{G} = \langle G, * \rangle$, which can be a model for group theory, where G is a nonempty set and $*$ a binary operation on G. The "models" of ZF cannot be constructed as *sets* of ZF. These "models" are, in a certain sense, informal structures, built in informal mathematics. They *model* a theory like ZF in the sense that we can "see" that its axioms are (intuitively) true in those structures.

[3] Note that the value-state distinction is cashed out in terms of the concept of truth. The relationship between these ideas and the concept of quasi-truth is developed in Bueno 2000.

[4] A few comments about the notation: (a) *Tr* is a linear functional of operators into numbers (the trace map), which gives us the probability that a measurement of the observable m has a value in the Borel set E; (b) I_E^{M} is an Hermitean operator such that $I_E^{\mathrm{M}}(x) = x$ if $\mathrm{M}(x) = ax$ for some $a \in E$, and is the null vector if $M(x) = bx$ for some value $b \notin E$, where M is the Hermitian operator which represents m. (c) That the trace function *Tr* provides a probability is due to the fact that $P_x^m(E) = (x \cdot I_{\mathrm{E}}^{\mathrm{M}} x) = Tr(I_x I_{\mathrm{E}}^{\mathrm{M}})$, where $P_x^m(E)$ is the probability that a measurement of m has a value in E, $(x \cdot I_{\mathrm{E}}^{\mathrm{M}} x)$ is the inner product of x and $I_{\mathrm{E}}^{\mathrm{M}} x$ and I_x is the projection on the subspace $[x]$ spanned by x. (For details, see van Fraassen 1991, pp. 147–52, 157–65, and 280–1).

[5] Which again are spelled out in terms of truth.

[6] Although the concepts of individuality and distinguishability should not be confused; see French and Krause 2006.

[7] See also Jafee in Cao 1999, p. 165, as well as discussions in the same volume by Schnitzer, p. 163, and Rohrlich, p. 257.