

M. Ferrari & I.O. Stamatescu (eds.), *Symbol and Physical Knowledge. On the Conceptual Structure of Physics*. New York, Berlin, Heidelberg: Springer-Verlag, 2002, 229 pp.

This book is the outcome of an interdisciplinary research project conducted in Heidelberg. It brings together contributions from mostly German authors, mainly philosophers of science, but also incorporating two practicing physicists. The editors' preface opens with the following words: "The question of the symbolic structure of physics is implicitly involved in any discussion about the character of physical knowledge and the development of physical theories. Actually many discussions would greatly profit from an *explicit* reference to and an investigation of this question, and much confusion may be avoided in this way." (p. v) This means that the book holds an interesting promise, which however it does not always fulfil, although surely containing some interesting contributions.

The book consists of three parts, containing in total ten essays. The first part is intended as a general introduction, the second part addresses *Views on Symbol in the Philosophy of Science*, and the third part is entitled *On the Symbolic Structure of Physics*. The two introductory essays give a general overview of respectively the philosophical history of the concept of a symbol and its epistemic functions, and of the place and function of symbols in modern physical theories. Both are intended as background for the other essays in the volume, but are an interesting reading in themselves.

Massimo Ferrari traces a symbolic conception of knowledge back to Leibniz and follows its history and development through Kant, von Helmholtz, Hertz, Wittgenstein and Cassirer in "Sources for the History of the Concept of Symbol from Leibniz to Cassirer". On Leibniz' view, a purely intuitive knowledge was the privilege of God, and therefore forever beyond the reach of mankind. However, there is the possibility of a compensation for the natural bounds of human reason: the use of the mediating function of signs. This naturally led Leibniz to the problem of expression, i.e. how can signs express something else. His answer was threefold: there can be a rigorous similarity (as between map and depicted region), there can be a functional relation, and there is the possibility of an arbitrary stipulation (however not leading to a strict nominalism). The most important thing is that there always has to be a structural relation

between signs and designated things. By introducing this *problématique* Leibniz set large part of the agenda for epistemologists to follow him. The foremost one to take up these problems – and transforming them in doing so – of course was Immanuel Kant. His main attempt was to overcome the Leibnizian dualism between mental sign and designated thing, by introducing a third element. This element was to be a “pure” element of knowledge. The transcendental schemata of the pure concepts of understanding, which were to serve as mediating representation, were at the same time intellectual *and* sensible. In this way they provide an image for a concept, and thus make it possible to ascribe objective validity to the concept. However, it is well known that not all concepts can be assigned an intuition directly corresponding to them, as is the case with all the concepts that refer to the intelligible world. Nevertheless, an objective validity can still be ascribed to these concepts, but only *indirectly* by means of symbols that correspond to the consequences of these concepts. Thus, besides all the differences with Leibniz, also on Kant’s view a compensation for the finite bounds of human reason is part of the function of symbolic knowledge. Of course it would lead us too far to consider all relevant aspects in which both philosophers differ with respect to their views on symbolic knowledge, but it is interesting to mention one feature that Ferrari mentions. For Kant, symbolic knowledge belongs to the intuitive component of knowledge (respectively direct and indirect for schemata and symbols), and the use of characters and signs is relegated to the pragmatic dimension of language; one should not confuse the need for communication with the proper transcendental activity of the mind – or so Kant warns us against the Leibnizian tradition. Ferrari makes clear that the conceptions of symbol that are elaborated in the nineteenth and early twentieth century can be understood as the outcome of a prolongation of this dialogue between Leibniz and Kant. On the one hand the Leibnizian program of a “universal characteristic” was surely influential in the development of modern logic by people like Boole and Frege. In their views the investigation of the laws of thought had to be taken up through a systematic research of the symbolic process of reasoning. On the other hand, the Kantian tradition obtained a new impulse through the works of Hermann von Helmholtz, who combined the transcendental theory of knowledge with new insights from the psychological and physiological sciences. He maintained that all representations of the world are conditioned by our sensory organs, and that signs

mediate between the organization of our mind and the outside reality, i.e. they have to reflect the lawfulness of reality without needing to have any literal correspondence. A successor of von Helmholtz, Heinrich Hertz stressed the possibility of different, equally valid models of reality, i.e. all respecting the lawfulness of reality, but differing in their symbolic constitution. Most famously he elaborated on this in his *Prinzipien der Mechanik*, in which the power of mental activity is shown to make possible different systems of mechanics, all doing justice to the relations that are observed to obtain between the objects. In his *Tractatus* Wittgenstein explicitly refers to Hertz, however transforming Hertz' dynamic view about cognition into a static view about language. The main link is the importance of a kind of isomorphism as a basic condition, in Wittgenstein's case for the possibility of language. One can of course also remark important Leibnizian themes in such a view, and the first philosopher who explicitly took up the task of providing a synthesis between Kantian and Leibnizian insights was Ernst Cassirer. He stressed that symbols should have a functional interpretation (whence should in no way be considered a copy of external reality), and his conception of science was rather Hertzian in that he conceived the role of concepts to be the ordering and connecting of reality in a functional way. From Kant he took the insight that the objectivity of science depends on the conditions of validity of the symbols (so that one should speak of objective signs, instead of signs of something objective), but he abandoned Kant's schematised concepts of pure understanding (in this way discarding the sensible intuitions as an autonomous source of knowledge).

Ion-Olimpiu Stamatescu claims to take a working physicist's perspective "On the Use and Character of Symbols in Modern Physical Theories". On his view, physical theories are born in a continuous interplay between two factors: on the one hand the search for self-consistent systems of concepts, and on the other hand the responsibility to confront these systems with empirical information. Both demands (consistency and empirical adequacy) ensure that the concepts of the resulting theories will at the same time obey stringent constraints and still have a degree of freedom. The constraints are imposed on the concepts in two directions: "horizontal" through inter-conceptual relations, and "vertical" through the confrontation with empirical phenomena. The freedom comes from the fact that these are the sole basic demands that theories have to meet, and different basic concepts could serve as well. But this freedom tends

to disappear when we look at particular tracks of development of physical theories. As this development takes place in a continuous constructive effort, the historicity of concepts cannot be disregarded, and makes for a kind of “necessity” for the chosen concepts. One very important aspect of this development is that it is essentially a two-way process: on the one hand, the objects about which theories are (re)formulated suggest appropriate concepts; on the other hand, the concepts help to identify the right objects. Stamatescu does not attempt a further philosophical exploration of these basic themes – which supposedly is left for the other essays in the volume – but rather tries to illustrate them with some examples taken from modern physics. He describes four different cases of inter-theoretic relations, which illustrate different kinds of conceptual development. Theory reduction gives rise to conceptual enrichment (as was the case with the concept of temperature in nineteenth century physics). When one theory acquires the status of being an approximation to a more accurate theory, one can notice a more complex conceptual shift (an illustration is the concept of mass after the advent of relativity theory). Unification and synthesis of previously unrelated theories often provide us with new concepts made up of earlier disparate concepts (e.g. space-time). One can see that in all those cases the structure of the symbolic network *and* the objects identified through the concepts undergo change at the same.

As already mentioned, these two essays are supposed to furnish the background to the other parts of the volume. The second part (*Views on Symbol in the Philosophy of Science*) stands in close connection with Ferrari’s essay. The three essays contained herein deal respectively with Duhem’s, Peirce’s and Cassirer’s, and Hertz’ views on part of the *problématique* sketched by Ferrari. It is however a pity that none of the essays goes much beyond introductory sketches of these philosopher’s opinions, and thus do not offer much of an attempt at a better understanding of the conceptual structure of physics (as the introduction of the volume promised).

Karl-Norbert Ihmig’s essay is entitled “The Symbol in the Theory of Science: Duhem’s Alleged Instrumentalism or Conventionalism and the Continuity of Scientific Development”. As the title indicates it is argued here that Duhem was not an unqualified instrumentalist or conventionalist – but we knew this already, didn’t we? The main reason for this is the fact that the concept of a natural classification was essential for Duhem,

since he believed that it provided the possibility of understanding the continuity of scientific development. Nowhere does Ihmig attempt a confrontation of this idea with examples of conceptual development as provided by Stamatescu. Duhem is of course also well-known for the (im)famous holist thesis that was named after him. Ihmig mentions this but does not discuss the many implications this might have for the conceptual structure of physical theories. He does show some scepticism towards Duhem's thesis that physical theories can always be divided in a representative and an explanatory part, the former ensuring the continuity and the latter containing metaphysical ballast. But again, he does not try to assess what this could mean for the symbolic representations provided by theories.

The second essay of the second part is *Enno Rudolph's* "Beyond Realism. Symbolism in the Philosophy of Science by Charles S. Peirce and Ernst Cassirer". It is rather hard to figure out the point of this essay. It contains some remarks on Peirce's pragmatism, his views on the functioning of symbols and their relation with reality. And then in the middle of a paragraph, without further warning, attention is diverted to Cassirer's views on symbolic representation. In one meagre final paragraph there are some remarks on the differences between the two philosophers, but what this is supposed to tell is left for the reader to guess ...

Andreas Hütteman's "Heinrich Hertz and the Concept of a Symbol" repeats some of the points already discussed in Ferrari's essay, but also expands in more detail on them. Hertz' main attempt was to separate the features of physical theories that have their origin in nature from the ones that can be entirely ascribed to man. To this end he tried to make a distinction between the representation and content of a theory. The former was supposed to add physical significance to a system of equations. As Hütteman indicates, one can be highly suspect of the possibility of such a strict distinction, but this failure can teach us highly relevant things about the nature of symbolic representations. These lessons to be drawn, once again, are left for another time, occasion, or whatever...

Luckily, the third part of the book, *On the Symbolic Structure of Physics*, contains some very interesting essays. The first essay, by *Martin Carrier*, is entitled "Shifting Symbolic Structures and Changing Theories: On the Non-Translatability and Empirical Comparability of Incommensurable Theories". In it Carrier takes up the well-known problem of incommensurability. His essay is interesting for the detailed considera-

tions he gives to one particular example, and for the moderate conclusions – sometimes this really is a virtue, as philosophers of science should be well aware by now – he draws from this. The fact that scientific theories contain many concepts for unobservable entities, makes clear that the relation between concept and designated thing will be of a complex kind. Following Kuhn, Carrier assumes semantic holism, i.e. the idea that the use of a concept is what determines its meaning. In the case of scientific concepts the relevant laws determine their application, and since it is well known that these laws can undergo profound changes, incommensurability seems unavoidable. For example, Carrier argues that notwithstanding the superficial similarities between the Lorentzian (absolute space and time) and Einsteinian (relative space-time) versions of electrodynamics (both sharing the same equations, and containing the idea of e.g. length-contraction), conceptual discrepancies make a genuine translation of concepts like mass, time etc. impossible. To this end he shows, by a detailed comparison, that it is impossible to transfer at the same time the conditions of application *and* the inferential relations of concepts from the Lorentzian to the Einsteinian scheme, or vice versa. The reason for this is that physics is more than a system of equations, but deals with interpreted quantities. If meaning determines reference, as is widely (but not universally) assumed, it looks as if this really spells problem – this is the message that Kuhn and Feyerabend had for us. The least controversial that one can say, is that this teaches us that progress in science cannot be understood as coming to understand more and more aspects of the same entities. But often more is claimed, namely that this does imply the impossibility of an empirical comparison of theories using incommensurable concepts. How can one identify the relevant experiments – which, remember, cannot be described in a theory-neutral way? Carrier's answer is plain and simple: comparison does not require translation. He even makes a stronger claim: the translation problem can only arise in the first place *if* there is comparability! As everyone involved in the discussion would agree, there is a relevant difference between stating that Darwin's theory of natural selection is not translatable into hydrodynamics, and stating that Lorentzian electrodynamics is not translatable in Einsteinian. But where does this relevant difference come from, if it isn't from the fact that the latter theories are considered to be theories about the same phenomena? And this is all we need to have comparability. It is not necessary that the results of an experiment are translatable from

one theory into another one, once it is agreed that the experiment lies within the domain of application of both theories. If this is the case, both should be able to account for the experimental results, and they can be judged on how well they fare in their job.

In her essay "Symbol and Intuition in Modern Physics", *Brigitte Falkenburg* makes a very useful exercise. It is often claimed that modern physics is highly non-intuitive, but Falkenburg rightly remarks that what we understand by the concept of intuition is in itself the outcome of a historical process. In her essay she traces our concept of intuition back to Kant, in her views the first to have formulated a unified theory about intuition. On this theory, intuition was considered a distinct cognitive faculty, responsible for the possibility of experience. Moreover the form of these possible experiences was fixed a priori to obey the Euclidean axioms. (Falkenburg claims that Kant was aware of the "logical" possibility of non-Euclidean geometry, but that he did not grant it the status of a "real" possibility because this was not constructible in intuition.) One important reason for this fixation was that it allowed Kant to solve the riddle of the applicability of mathematics to nature, but Falkenburg warns us that it is important to be aware of the fact that intuition had more functions for Kant than just this one. To this end she points to the sharp distinction that Kant made between intuitions and concepts (in contradistinction with his forerunners like Descartes and Leibniz – see also Ferrari's essay on this). Whereas concepts are abstract and symbolic representations that subsume specific contents under them, intuitions are concrete and image-like representations that represent specific contents in them. This allowed Kant to conceive of intuition as the *individuating* cognitive faculty that supplies a domain of application for abstract concepts, by constituting objects in space and time. As a result it was intuition that made possible the interpretation of formal theories for Kant. With respect to this, Falkenburg claims that there is no reason why this individuating function should be dependent on his attempt to restrict the domain of application to Euclidean structures (as a necessary framework for Newtonian mechanics, of course). (As a matter of the fact, Falkenburg gives some interesting hints on the central role that the problem of individuation played in the development of Kant's thinking.) If one looks to modern physics, it becomes very clear that it are precisely problems with the individuality of systems that constitute the non-intuitiveness of quantum theory. And still, this theory somehow *is* related to observed

phenomena. As is well known, Bohr tried to tackle this problem of interpretation in a Kantian vein, by claiming that the language in which the theory is interpreted must use classical concepts in order to secure reference. The largest part of present-day theorists seem to have abandoned the Kantian coupling of reference to intuition, but still value the search for pictorial language. The main function of this search lies in an attempt to make the content of scientific knowledge somehow cognitively graspable and more manageable. But Falkenburg remarks that also this function was already elaborated on by Kant, under the heading of the aesthetic ideals of cognition. These ideals mainly had to do with the subjective grasping of theories, and were of prime importance in teaching and communicating scientific knowledge. The essay ends with an illustration of the function – and limitations – of these ideals in the use of Feynman diagrams in quantum field theory.

Andreas Hütteman's second essay in the volume is about “Idealizations in Physics”. In it he claims that three distinctive features characterize idealizations: (i) they are replacements; (ii) these replacements are conscious and voluntary (i.e. it is known that the replacement is not true of what it is an idealization); (iii) these replacements are not undertaken arbitrarily. Following this general characterization Hütteman sums up eight kinds of idealizations that are used by physicists. (a) The production of physical systems. (b) The isolation of physical systems. (The classification of these first two kinds as idealizations, supposes that physical theories are about nature, and not about artefacts.) (c) Data interpolation. (d) Data fitting. The last four kinds are called theoretical idealizations by Hütteman, and are the least controversial (or so he claims). (e) Abstraction (i.e. the decomposition of a system in different subsystems). (f) Idealization in the narrow sense (i.e. the replacement of specific properties of a system). (g) Neglect of (higher order) terms in an equation. (h) Simplification of mathematical functions. The task that Hütteman then sets up is to provide a viewpoint that can account for all these idealizations. He claims that essentialism will not do, because no one has ever produced a convincing argument to the effect that the more simple mathematical descriptions are more likely to be true. But also Nancy Cartwright's view – that idealizations are *only* undertaken for their explanatory power, since they take us away from the truth (empirical adequacy) – cannot be right on Hütteman's opinion, since there is no reason to suppose that abstractions take us away from the truth. He then goes on

to argue that the aim of providing what he calls “mechanistic explanations” can account for the use of all the different kinds of idealization. On this ideal a complex system’s properties must be deducible from: (i) the properties of isolated components; (ii) general laws of combination; (iii) general laws of interaction.

Carsten Held’s “Symbolizing States and Events in Quantum Mechanics” can be considered a piece of applied epistemology in which he investigates the possible meanings of the concepts “state” and “event” in quantum mechanics. His starting point is the formalism of quantum mechanics, and his conclusion is that there is a fundamental difference between the basic concepts of classical and quantum physics. He gives an argument to the end that the quantum mechanical state vector must be understood either as the encoder of probability information, or as the description of a system, but cannot have both functions (as some philosophers, arguing for a propensity interpretation, would have it). If one assumes with the majority of the physicists that it certainly is a probability encoder, then it follows that classical dynamical models *cannot* be construed on the basis of the state vector. Still, on this understanding it seems a mystery what the state vector is about. Held’s solution consists in a reinterpretation of the concepts “state” and “event” in the quantum theory: a quantum state is the state of an experimental setup, and a quantum event is the original appearance of a certain quantum object upon observation. None of this is highly original of course, but Held’s essay is carefully argued and it makes very clear the large gap that separates the conceptual structure of classical and quantum theories.

The last essay of the volume is by *Hans J. Pirner*, a distinguished theoretical physicist, and is entitled “The Semiotics of “Postmodern” Physics”. I am afraid that at this point I have to take a more personal style as reviewer, just to be able to make some sensible remarks. This essay takes up a topic that I think very interesting and useful: the way information is contained in signs and can be extracted from them. I also very much sympathize with Pirner’s plea that hoaxes like Sokal’s should not prevent physicists from being open-minded towards the hope of a better understanding of their own activity that philosophy still can offer to them. But I admit that I was not able to understand one iota of the main part of the article, which for my part could have been just another hoax. As a disclaimer I must immediately add that I am not at home in semiotics, and thus do not want to take an authoritative stance from

which to blame Pirner for this. Still, it is a highly ironic fact that such a communication breakdown should occur in an essay dealing with information transition.

As this review should make clear, it is hard to give a straightforward appreciation of this volume. It surely contains some essays that are worth a closer look, but it lacks the overall systematic outlook that would have made it more than just another volume with essays on epistemology and philosophy of science.

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