Representation and Invariance of Scientific Structures, Patrick Suppes, Stanford: Centre for Logic, Language and Computation (distributed by the Chicago University Press), 2002, pp. ix + 536, price: $ 50.00, ISBN 1-57586-333-2

About half a century ago, Patrick Suppes and collaborators published an axiomatisation of classical particle mechanics Newtonian-style (*Scientific Structures*, p. 316). Until then, in philosophy an axiomatisation of a scientific theory was generally understood in exactly the same sense as it was understood in logic: first erect a 1st-order formal language, notably including predicates for the primitive concepts of the scientific theory and a formal-deductive apparatus in order to be able to reason rigorously (prove theorems); next select a number of sentences of the language as the axioms; the theory is then by definition the deductive closure of these axioms. How to connect a formalised scientific theory to the world? A distinction was introduced in the primitive predicates of the language: theoretical and observational ones. Every formalised scientific theory contains a number of axioms that connect the two. Sentences consisting of entirely observational predicates are in principle open to verification or falsification. That is to say, whether an observational sentence is true or false can, in principle, be determined just by opening your eyes. Thus our sensory experience constitutes the connexion between a theory and the world. This nexus codifies the empirical essence of science. Without it, there is no science. Let us call this answer to the question what a scientific theory is and how it relates to the world the *formal-linguistic view*.

When he axiomatised classical particle mechanics Newtonian-style, Suppes did, however, nothing of the sort: he did not create a formal language, he did not describe his deductive apparatus explicitly, he did not erect a formal-deductive system, and he did not subdivide predicates into theoretical and observational ones. What he did was to define informally a set-theoretical predicate, to consider its set-extension in the domain of discourse of set-theory (**V**); he declared that this set *is* the scientific theory. The members of this set turn out to be the same as what in mathematics are called *structures*. Suppes moved on and conceived experimental science to be in the business of producing *data structures*, which also live in **V** — *which* of all possible data structures in **V** are found in an experiment can of course only be determined by actually performing the relevant experiment. The connexion between the theory (some set of structures) and the world runs via the data structures: they are *embeddable* in at least one of the structures in the set (embeddings are morphisms of sorts depending on the theory and data structure at hand). So the connexion between a theory and the world also lives in **V**. This is only a 1st-order approximation of the practice of science, as Suppes has argued a long time ago; more realistic is to position a hierarchy of structures between the bare data structure at one end and the theory-structure at the other end. We call this view the *informal-structural view*.

The afore-mentioned, formal-linguistic answer to the question what a scientific theory is and how its relation to the world should be characterised was designed and developed by the logical-positivists from 1920 onwards. This programme went down as a result of an accumulation of internal problems and external criticism. In his seminal review article 'The Search for Philosophic Understanding of Scientific Theories' of 1973, Frederick Suppe described its magnificent rise and fall. According to some, this programme has ended in complete failure; others find morsels of philosophical insight in the remains. The last-mentioned, informal-structural answer slowly but steadily gained momentum. Initially only Suppes and some collaborators worked on it; only around the time when Suppe wrote his obituary of the formal-linguistic view, the informal-structural view took off. Today it seems to have conquered the world. Theories of physics, mathematics, chemistry, biology, economy, politics, psychology, linguistics and more have come under its Alexandrian sway. According to some, this programme has met with complete success.

During the 1960ies, mimeographed lecture notes of Suppes began to circulate, often referred to as 'Structures in Science'. During the second half of the 20th-century these notes attained the status of The Most Often Cited Unpublished Work, notwithstanding the fact that most of Suppes' results found their way to the journals. (What came closest to a manifesto of the informal-structural view was the last Chapter of Suppes' wonderful *Introduction to Logic* of 1957 — not the most appropriate place for such philosophical manifestoes.) Now, finally, after almost half a century, Suppes' lecture notes have been published officially, because *Scientific Structures* (the book under review) is largely based on these lecture notes. From one perspective, this is much too late, because these notes have done their work already and *four* collections of Suppes' papers have appeared previously. Yet as soon as we realise that the book is filled with results that Suppes and his many collaborators have achieved over the past decades, spread out over a variety of fields of academic inquiry, the appearance of *Scientific Structures* may be a mile stone along the road of philosophy of science after all. The committee that awarded the 2004 Lakatos Prize for a recently published outstanding book in philosophy of science has decided in favour of the mile-stone judgment.

In the light of what we just said, one might have expected *Scientific Structures* to be the definitive exposition of the informal-structural view (this reviewer certainly did): a First Part consisting of a careful treatment of this view on scientific theories and their relations to the world, and the foundations and implications of this view, philosophical, mathematical, logical and pragmatical; a point-wise comparison with its only competitor, the formal-linguistic view; illustrations from the practice of science to see how seamlessly Suppes' view fits it; and a discussion of how the structural view ties in, or does not tie in, with issues debated in contemporary philosophy of science; and a Second Part filled with results of *philosophical* importance that have been achieved on the basis of the structural view. If anyone ought to be able to live up to such high expectations, it is Patrick Suppes. *Scientific Structures* is, to put it bluntly, neither. The First Part is not really there, some scattered pedestrian philosophical remarks, and *idem dito* quotations and comments of Great Names from the history of Western philosophy notwithstanding, whereas there are no more than allusions to the Second Part.

No matter how reasonable this reviewer's high expectations were, it would be unfair to blame Suppes for not having met them, because in all honesty Suppes nowhere *says* that *Scientific Structures* is anything like the imaginary First and Second Part — ignoring the fact that suggestions to the contrary loom large in the book. Figments of this reviewer's imagination. What Suppes *says* is no more and no less than the title suggests: the concepts of *representation* and *invariance* show their face in many branches of science, they are important for science, and the informal-structural view provides the most expedient means to deal with these concepts rigorously in a unified fashion. The aim of the book is to demonstrate this claim by proving representation theorems for the structures that constitute a particular scientific theory (characterised by a set-theoretical predicate), and then to pinpoint the invariants of the theory by proving their existence. So when we measure the success of a programme by measuring how close the programme comes to reaching its aim, Suppes' programme in *Scientific Structures* is a Herculean success. The book contains, besides a chapter on Representation (Chapter 3) and Invariance (Chapter 4) in general, a long one on Probability (Chapter 5), one on Space and Time (Chapter 6), one on Mechanics (Chapter 7), a long one on Language (Chapter 8) and a Summary Table of the Representation and Invariance Theorems presented in the book. Suppes' sweep is truly intimidating when one realises that he has actually contributed to all these subjects during the past decades. One's knowledge of various mathematical results concerning scientific theories undoubtedly will have grown considerably after having read the book. You will never forget the importance of the concepts of representation and invariance for science.

But when the expectations of this reviewer are those of a garden-variety philosopher of science (or physics), then the question what there is to gain by reading *Scientific Structures*, with its more than five hundred large pages and 11pt letter, becomes urgent. In particular, the question what is *philosophically* interesting about representation and invariance in science becomes urgent. These concepts are (sometimes, frequently, often) important *for science* all right, but are they important *for philosophy*? Having worked his way through almost the entire book, this reviewer admits to have been unable to find an answer. This is not to say there is nothing of philosophical interest in *Scientific Structures*. On the contrary, numerous issues of supreme philosophical interest will pass the ardent reader's eyes when reading the book. It makes the philosopher of science run at the mouth. But all he is left with after having read the book is a whet shirt: many philosophical dishes are served but they are never eaten, let alone digested.

To be specific, let us consider Suppes' conception of a model, the beating heart of *Sci-*

entific Structures. Suppes says that his notion of model is Tarski's (pp. 20-21). This is however not quite true, although it is quite true that Suppes was inspired by Tarski — with whom he collaborated a few times. Tarski's concept of a model is, properly denoted, a triple $\langle \mathfrak{U}, f, \models \rangle$, consisting of a set-structure $\mathfrak{U}$ living in the domain of meta-discourse (**V** whenever the meta-theory is also some set-theory), a reference-map f sending terms in a formal language to objects in $\mathfrak{U}$ and a recursively defined map $\models$ from sentences in the formal language to the semantic values 'true' and 'false'. A Suppesian model exactly is the set-structure $\mathfrak{U}$; and Suppes argues convincingly that *this* can be identified with what is called a 'model' by working scientists, rather than Tarski's triple. But the semantics is discarded — which is why the name 'semantic view' for Suppes' view is a terminological howler. A Tarskian model is a model *of a formalised theory, of a set of sentences of a formal language*; a Tarskian model presupposes, *contra* Suppes, the formal-linguistic view on theories. A model in science is a model *of something in the world*, of the planetary system, of the brain, of the human cell, of a Uranium molecule, of the universe, and so forth (call these *systems*, for want of a name). When Suppes wants to make sense of how models are used in science, and these are identified as Suppesian models, then these Suppesian models must also be models of something in the world, *of systems* as we just called them. What we know about the modelled systems we know via our scientific theories. About *this* putative connexion, between Suppesian models (set-theoretical structures in **V**) and systems in the world, Suppes passes over in silence. Suppes favors saying that his structures *are models of data-structures.* But since data structures are themselves set-theoretical, quantitative representations of the qualitative results of experiments or observations about some system in the world, this relation lives in **V**. The world has thus been put between brackets.

This reviewer ventures to assert that science definitely does not put the world between brackets. Every scientific theory and scientific model is all about the world; it *says* something about the world. Without the world there is no science. The models of the working scientist, that is, Suppes' set-structures, are supposed to somehow provide us with 'scientific knowledge' about what they are models of, that is, of systems in the world — surely they do not provide us with 'scientific knowledge' *of data structures.* Whether the set-structures embed the relevant data structures surely is the test which the set-structures must pass in order to be considered as providers of scientific knowledge of the systems. But what does a set of set-structures *say* about the world? What a theory *says* about the world can be true or false, or indeterminate if *tertium datur.* How does truth fit in here? What is the place of truth in science according to Suppes? Are the ontological black boxes truth-makers? What is scientific knowledge? Is it a set of set-theoretical structures in **V**? Is Suppes a hard-nosed instrumentalist in that science is no more than a game of fabricating set-structures wherein we can embed data-structures fabricated in the laboratory? Does this game have an epistemic aim? Is all the rest disreputable metaphysics? (Science according to Suppes, as depicted in the Figure, suggests this.) Can measurement theory, which is part and parcel of Suppes' view of science (Chapters 3 and 4), be used by nominalists to justify (via representation theorems) the use of numbers without having to believe in their Platonic existence? Crowds of philosophical questions are banging on the door. Suppes keeps the door locked. We are only permitted a peek through the window.

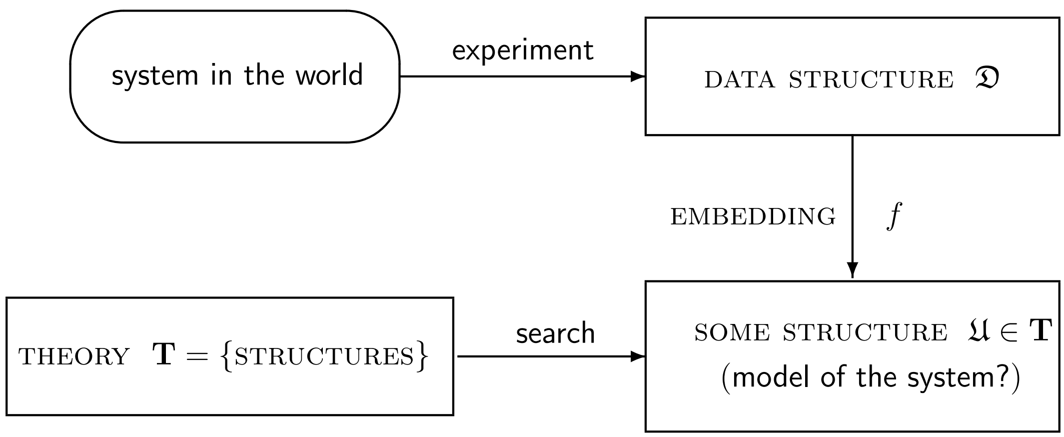

Figure 1: *First approximation of scientific practice according to Suppes.* What is in the rectangles and what is in SMALL CAPITALS lives in $\mathbf{V}$ and can be characterised in the language of axiomatic set-theory. There is no direct connexion between theory ($\mathbf{T}$) and reality (the system out there, in the world). Suppes passes over this connexion in silence. (More realistic approximations have a hierarchy of structures between the bare data structure $\mathfrak{D}$ and the theoretical structure $\mathfrak{U}$.)

Furthermore, recent developments in the philosophy of science, led by philosophers like Nancy Cartwright, Margaret Morrison and Mary Morgan, witness an (almost myopic) attention to models in science at the expense of theories. Without exaggeration we may say that Suppes' work has been instrumental in causing this shift of emphasis with his Suppesian models. In Chapter 2, Suppes restates his case that the ubiquitous model in science can be identified with a set-theoretical structure (and not, to repeat, a Tarskian model; *vide supra*), and that a theory is no more than a set of Suppesian models: as soon as you have put the finger on what all classical-particle-mechanical models Newtonian-style of physical systems occurring in the physics literature have in common, you have characterised the theory called 'Newtonian classical particle mechanics'. If one finds a model in science that has nothing interesting in common with any other model in science, then characterising it leads to a meager theory; if one finds a host of models which share a lot, then they lead to an impressive, encompassing theory. Models constitute theories and theories consist of models. The shift of attention in contemporary philosophy of science boils down to one from a set to its members! Sadly Suppes make no contact to the current philosophical literature on models in science. Consequently the question whether *Scientific Structures* has any bearing on the problems raised in that quarter of the philosophy of science remains unanswered.

Here is why invariance theorems can be important for the philosophy of science: Hermann Weyl, and recently Robert Nozick, held that what is *real* according to some theory should be identified with its invariants. Enter the issue of realism. The dish is served and then Suppes leaves the table.

A general philosophical point which is not but could have been made by Suppes is that *Scientific Structures* is a marvellous celebration of the unity of scientific knowledge, and thus surreptitiously militates against the view that what sails under the flag of Scientific Knowledge is an incoherent, gerrymandered patch-work of theories, models and hypotheses, as for instance Arthur Fine and Nancy Cartwright have attempted to argue. On the contrary, there are deep-seated similarities in *prima facie* unrelated branches of science and the existence of these similarities can be demonstrated rigorously. These similarities reside exactly in the fact that all scientific theories can be characterised in the same rigorous manner, in the fact that the results of experiments and observations can be characterised in the same rigorous manner, in the fact that the relation between these can be characterised in the same rigorous manner (see the Figure), and in the fact that representation theorems and the existence of invariants can be demonstrated. So for anyone who has completely succumbed to the patchwork-view and for whom phrases like 'the unity of scientific knowledge' are echoes from a distant logical-positivist past, reading *Scientific Structures* will be a salient experience.

In Chapter 5, on probability, Suppes can be seen to initiate a novel approach to the debates about the interpretation of probability. The standard approach is to let Kolmogorov's axioms take care of the mathematics, or calculus, of probability; how to connect a probability-structure $\langle \Omega, \mathcal{B}(\Omega), P \rangle$ to the world is then called the problem of interpretation: the probability measure $P : \mathcal{B}(\Omega) \to [0, 1]$ is interpreted as a limiting relative frequency, as the strength of belief of a person, as a propensity, as a degree of confirmation, or what not. Suppes considers the following approach to be the right one, which he ascribes to De Finetti (p. 226). Rather than simply asserting that the normed additive measure P measures 'strength of belief', one characterises the qualitative relation 'is believed more strongly than' axiomatically and then proves a representation theorem, which essentially says that this relation can always be mimicked by 'is larger than' (on $\mathbb{R}$) between values of some existing P (we gloss over some significant details here). Suppes provides *mutatis mutandis* a qualitative characterisation of propensity and proves a representation theorem for it in terms of P. This seems an excellent idea, because rather than verbal battles over how to interpret P in terms of another concept, we axiomatise this other concept directly and then connect it to P via some representation theorem. Verbal arguments in favour of one 'interpretation' may then assume the status of mathematical proofs, because the different interpreting concepts will yield different axiomatisations, thus leading to different theorems. Such a gain in rigour is beautiful. There is however a problem with this approach.

In order to prove a representation theorem, the qualitative concept needs to obey sufficiently strong axioms. Whenever the axioms are too weak and no representation can be proved in terms of P, standard Kolmogorovian probability theory is out of reach, which surely is unacceptable. But the axioms also must be natural for the concept being axiomatised. In De Finetti's case the axioms are precisely that, although even in the finite case certain additions have turned out to be necessary, as Suppes honestly admits (pp. 229-230). Let us be charitable and grant Suppes the success of this approach in the case of subjective probability. Does Suppes succeed for the other concepts that are traditionally taken to be interpretations of P? Consider propensity. Suppes criticises Karl Popper, the originator of this interpretation of probability, for not having provided a rigorous characterisation of propensity so that nothing of genuine interest can be proved about it (p. 202). First Suppes defines a 'qualitative conditional probability [*sic*] structure' (p. 204); then what it means for such a structure to be Archimedean (p. 205); next he proves a representation theorem for finite sequences occurring in this structure (*ibid.*); he says that some axioms are 'necessary', namely those which seem necessary in order to prove the desired representation theorem; and, finally, 'structural' axioms have to be added, depending on the application one has in mind, to make the axioms jointly sufficient for actually proving the representation theorem. Suppes gives the example of radio-active decay. The structural axiom here is a 'waiting-time axiom', thus resulting in a 'waiting-time structure with independence of the past' (p. 208). Then, finally, he proves the representation theorem: there exists a probability measure of a particularly desirable form (*ibid.*). Now, this waiting-time axiom is very specific to radio-active decay. Most 'propensity-structures' will not have it. The tacit claim is now that one can always find 'structure axioms' which one has to add to the 'necessary' ones, so that a representation theorem is provable. But surely what propensity *is*, or is supposed to be according to its defenders, is what all propensity-structures have in common. Thus we are left with the 'necessary axioms' it seems. *They* are however too weak to prove the relevant representation theorem. Since no further, natural axioms seem available which hold for all propensity-structures, and which one can add in order to arrive, via a representation theorem, at Kolomogorovian probability theory, we seem forced to conclude — contrary to Suppes' intentions — that the propensity interpretation, at least as approached in this Suppesian manner, is unacceptable.

Suppes emphasises that the waiting-time axiom is distinctive for propensities "that would never be encountered in the theory of subjective probability as a fundamental axiom" (p. 210). But it will never be encountered in the theory of propensity as a fundamental, or 'necessary', axiom either because it is too special. Furthermore, when a subjectivist contemplates his beliefs about radio-active decay, reading experimental reports about it, he is free to use the waiting-time axiom to assign the strength of his belief in radio-active decay events.

All in all, there is a general philosophical programme in the interpretation of probability theory looming large in Chapter 5, but the problem just identified in the case of propensity seems to be there generally. If it is, this programme is doomed.

Scientific Structures. Philosophical issues in abundance. Rigorous definitions and theorems in abundance too. But philosophically they seem to lead nowhere and everywhere. There is little that would satisfy any philosopher of science: no formulation of a specific philosophical problem which is then fully addressed or solved; no formulation of a philosophical thesis for or against which arguments are propounded; no performance of systematic analyses of philosophical problems or theses or arguments. The long quotations from works of the Great Names from the history of Western philosophy only highlights this absence. Suppes has a great mind and this reviewer counts himself among his admirers. But Suppes cannot make up his scientific mind philosophically.

The time has come to pass judgment on *Scientific Structures.* I can't. My mind is torn and in the tear trickle tears. Suppes calls himself "an unreconstructed pluralist" (p. 53). He doesn't *want* to make his mind up philosophically? *Magister, relinquo.*

It is fitting that Patrick Suppes has won the 2004 Lakatos Prize, provided we consider it as a Life-Time Achievement Award.

F.A. Muller
Institute for the History and Foundations of Science and Mathematics
Utrecht University, P.O. Box 80.000, 3508 TA Utrecht, The Netherlands
E-mail: f.a.muller@phys.uu.nl