

SCIENCE AND PHILOSOPHY

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The Semantic Approach to Scientific Theories

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The purpose of this paper is not to be new or original, but to provide a concise exposition of a certain approach in philosophy of science, at the hands of a loosely associated group of contemporary philosophers.* I do not want anyone to suffer from guilt by association, so I should emphasize that any two authors mentioned here may agree on only a few aspects of the discussed approach, and disagree on others.

I locate the real beginning of the semantic approach, the place where it became consciously distinct, in E. W. Beth's *Natuurphilosophie* (1948), and summarized rather cryptically in two short articles in English and French in 1948/49. I read this book in 1965 and I gave an exposition of his views in my 'On the Extension of Beth's Semantics of Physical Theories' (1970). As I will explain below, some of the main ideas I found there were already in some way 'in the air' in North America. Since his view could be sloganized by saying that *the analysis of quantum logic provides the paradigm for the semantic analysis of physical theory*, it is clear that his view also had antecedents in the thirties and forties, flourishing perhaps entirely unconsciously of the implied opposition to the approach of the logical positivists. A good example is Herman Weyl's article 'The Ghost of Modality' (1940).

My own particular variant of the semantic approach is strongly colored by my attempts to become a more thoroughgoing empiricist, while such other advocates as Frederick Suppe and Ronald Giere are outspoken scientific realists. My exposition could not easily remain unbiased on this issue, but I hope that the essential neutrality thereon, of the semantic view, will be clear nevertheless.

1. The aim of science

Philosophy of science attempts to answer the question *What is science?* in just the sense in which philosophy of art, philosophy of religion, and the like answer the

* The main content of this paper is accordingly a combination of several previous ones; mainly my 'Aim and Structure of Scientific Theories' and 'Theory Construction and Experiment'; see bibliography. A more complete defense of my own particular variant of the approach will be found in *Images of Science*, edited by P. M. Churchland and C. A. Hooker, Chicago: University of Chicago Press, 1985.

similar question about their subject. For better or for worse our tradition has focussed on the scientific theory rather than on scientific activity itself (on the product, rather than on the aim, conditions, and process of production, to draw an analogy, which is already one that points in its terminology to the product as most salient feature). Yet all aspects of scientific activity must be illumined if the whole is to become intelligible. I shall therefore devote a preliminary section to the aim of science, and to the proper form of epistemic or doxastic attitudes toward scientific theories, before entering upon a description of their structure.

The activity of constructing, testing, and refining of scientific theories – that is, the production of theories to be accepted within the scientific community and offered to the public – what is the aim of this activity?

I do not refer here either to the motives of individual scientists for participating, or the motives of the body civic for granting funds and otherwise supporting the activity. Nor do I ask for some theoretically postulated ‘fundamental project’ which would explain this activity. It is part of the straightforward description of any activity, communal or individual, large-scale or small, to describe the end that is pursued as one of its defining conditions. In the most general terms, the end pursued is success, and the question is what counts as success, what are the criteria of success in this particular case?

We cannot answer our particular questions here without some reflection on what sort of thing this product, the scientific theory, is. A scientific theory must be the sort of thing that we can accept or reject, and believe or disbelieve; accepting a theory implies the opinion that it is successful; science aims to give us acceptable theories. To put it more generally, a theory is an object for epistemic or at least doxastic attitudes. A typical object for such attitudes is a proposition, or a set of propositions, or more generally a body of putative information about what the world is like, what the facts are. If anyone wishes to be an instrumentalist, he has to deny the appearances which I have just described. An instrumentalist would have to say that the apparent expression of a doxastic attitude toward a theory is elliptical; ‘to believe theory T’ he would have to construe as ‘to believe that theory T has certain qualities’. I shall not follow that path. Let me state here at once, as a first assumption, that the theory itself is what is believed or disbelieved.

At this point we readily can see that there is a very simple possible answer to all our questions, the answer we call *scientific realism*. This philosophy says that a theory is the sort of thing which is either true or false; and that the criterion of success is truth. As corollaries we have that acceptance of a theory as successful is, or involves the belief that it is true; and that the aim of science is to give us (literally) true theories about what the world is like.

That answer must of course be qualified in various ways to allow for our epistemic finitude and the consequent tentativeness of reasonable doxastic attitudes. Thus we add that although it cannot generally be *known* whether or not the criterion of success has been met, we may reasonably have a high degree of belief that it has

been, or that it is met approximately (i.e., met exactly by one member of a set of ‘small variants’ of the theory), and this imparts similar qualifications to acceptance in practice. And we add furthermore of course that empiricism precludes dogmatism, that is, *whatever* doxastic attitude we adopt, we stand ready to revise in the face of further evidence. These are all qualifications of a sort that anyone must acknowledge, and should therefore really go without saying. They do not detract from the appealing and as it were pristine clarity of the scientific realist position.

I did not want to discuss the structure of theories before bringing this position into the open, and confronting it with alternatives. For it is very important, to my mind, to see that an analysis of theories – even one that is quite traditional with respect to what theories are – does not presuppose it. Let us keep assuming with the scientific realist, that theories are the sort of thing which can be true or false, that they say what the world is like. What they say may be true or false, but it is nevertheless literally meaningful information, in the neutral sense in which the truth value is ‘bracketed’.

There are a number of reasons why I advocate an alternative to scientific realism. One point is that reasons for acceptance include many which, *ceteris paribus*, detract from the likelihood of truth. In constructing and evaluating theories, we follow our desires for information as well as our desire for truth. For belief, however, all but the desire for truth must be ‘ulterior motives’. Since therefore there are reasons for acceptance which are not reasons for belief, I conclude that acceptance is not belief. It is to me an elementary logical point that a more informative theory can not be more likely to be true – and attempts to describe inductive or evidential support through features that require information (such as ‘inference to the best explanation’) must either contradict themselves or equivocate.

It is still a long way from this point to a concrete alternative to scientific realism. Once we have driven the wedge between acceptance and belief, however, we can reconsider possible ways to make sense of science. Let me just end these preliminary remarks now by stating my own position, which I call *constructive empiricism*. It says that the aim of science is not truth as such but only *empirical adequacy*, that is, truth with respect to the observable phenomena. Acceptance of a theory involves as belief only that the theory is empirically adequate – but it involves more than belief.

While truth as such is therefore, according to me, irrelevant to success for theories, it is still a category that applies to scientific theories. Indeed, the *content* of a theory is what it says the world is like; and this is either true or false. The applicability of this notion of truth value remains here, as everywhere, the basis of all logical analysis. When we come to a specific theory, there is an immediate philosophical question, which concerns the content alone: *how could the world possibly be the way this theory says it is?*

This is for me the foundational question *par excellence*. And it is a question whose discussion presupposes no adherence to scientific realism, nor a choice between its alternatives. This is the area in philosophy of science where realists and anti-realists can meet and speak with perfect neutrality.

2. Theory structure – models and their logical space

In this section I shall present a view of theories which makes language largely irrelevant to the subject. Of course, to present a theory, we must present it in and by language. That is a trivial point, for any effective communication proceeds by language, except in those rare cases in which information can be conveyed by the immediate display of an object or happening. In addition, both because of our own history – the history of philosophy of science which became intensely language oriented during the first half of this century – and because of its intrinsic importance, we cannot ignore the language of science. Hence I shall make it the subject of the last section.

In what is now called the received view, a theory was conceived of as an axiomatic theory. That means, as a set of sentences, defined as the class of logical consequences of a smaller set, the axioms of that theory. A distinction was drawn: since the class of axioms was normally taken to be effectively presentable, and hence syntactically describable, the theory could be thought of as in itself uninterpreted. The distinction is then that scientific theories have an associated interpretation, which links their terms with their intended domain. We all know the story of misery and pitfalls that followed. Only two varieties of this view of scientific theories as a special sort of interpreted theories, emerged as anywhere near tenable. The first variety insists on the formal character of the theory as such, and links it to the world by a partial interpretation. Of this variety the most appealing to me is still Reichenbach's, which said that the theoretical relations have *physical correlates*. Their partial characters stand out when we look at the paradigm example: light rays provide the physical correlate for straight lines. It will be immediately clear that not every line is the path of an actual light ray, so the language-world link is partial. The second variety, which came to maturity in Hempel's later writings, hinges for its success on treating the axioms as already stated in natural language. The interpretative principles have evolved into axioms among axioms. This means that the class of axioms may be divided into those which are purely theoretical, in which all non-logical terms are ones specially introduced to write the theory, and those which are mixed, in which non-theoretical terms also appear. It will be readily appreciated that in both these developments, despite lip service to the contrary, the so-called problem of interpretation was left behind. We do not have the option of interpreting theoretical terms – we only have the choice of regarding them as either (a) terms we do not fully understand but know how to use in our reasoning, without detriment to the success of science, or (b) terms which are now part of natural language, and no less well understood than its other parts. The choice, the correct view about the meaning and understanding of newly introduced terms, makes no practical difference to philosophy of science, as far as one can tell. It is a good problem to pose to philosophers of language, and to leave them to it.

In any tragedy,¹ we suspect that some crucial mistake was made at the very

beginning. The mistake, I think, was to confuse a theory with the formulation of a theory in a particular language. The first to turn the tide was Patrick Suppes, with his well-known slogan: the correct tool for philosophy of science is mathematics, *not* metamathematics. This happened in the fifties – bewitched by the wonders of logic and the theory of meaning, few wanted to listen. Suppes' idea was simple: *to present a theory, we define the class of its models directly*, without paying any attention to questions of axiomatizability, in any special language, however relevant or simple or logically interesting that might be. And if the theory as such, is to be identified with anything at all – if theories are to be reified – then a theory should be identified with its class of models.²

This procedure is in any case common in modern mathematics, where Suppes had found his inspiration. In a modern presentation of geometry we find not the axioms of Euclidian geometry, but the definition of a Euclidean space. Similarly Suppes and his collaborators sought to reformulate the foundations of Newtonian mechanics, by replacing Newton's axioms with the definition of a Newtonian mechanical system. This gives us, by example, a *format* for scientific theories. In Ronald Giere's recent encapsulation, a theory consists of (a) the *theoretical definition*, which defines a certain class of systems; (b) a *theoretical hypothesis*, which asserts that certain (sorts of) real systems belong to that class.

This is a step forward in the direction of less shallow analysis of the structure of a scientific theory. The first level of analysis addresses the notion of theory *überhaupt*, but we do not want to stop there. We can go still a bit further by making a division between relativistic and non-relativistic theories. In the latter, the systems are physical entities developing in time. They have accordingly a space of possible states, which they take on and change during this development. This introduces the idea of a cluster of models united by a common *state-space*; each has in addition a domain of objects plus a 'history function' which assigns to each object a history, i.e., a trajectory in that space. A real theory will have many such clusters of models, each with its state-space. So the presentation of the theory must proceed by describing a class of *state-space types*.

In the case of relativistic theories, early formulations can be described roughly as relativistically invariant descriptions of objects developing in time – say in their proper time, or in the universal time of a special cosmological model (e.g., Robertson-Noonan models). A more general approach, developed by Glymour and Michael Friedman, takes space-times themselves as the systems. Presentation of a space-time theory T may then proceed as follows: a (*T*-)space-time is a four dimensional differentiable manifold M, with certain geometrical objects (defined on M) required to satisfy the *field equations* (of T), and a special class of curves (the possible trajectories of a certain class of physical particles) singled out by the *equations of motion* (of T).

Clearly we can further differentiate both sorts of theories in other general ways, for example with respect to the stochastic or deterministic character of imposed

laws. (It must be noted however that except in such special cases as the flat space-time of special relativity – its curvature independent of the matter-energy distribution – there are serious conceptual obstacles to the introduction of indeterminism into the space-time picture.)

I must leave aside for now the details of foundational research in the sciences. But I want to point out that the point of view which I have been outlining – the *semantic view* as opposed to the received view – is much closer to practice there. The scientific literature on a theory makes it relatively easy to identify and isolate classes of structures to be included in the class of theoretical models. It is on the contrary usually quite hard to find laws which could be used as axioms for the theory as a whole. Apparent laws which frequently appear are often partial descriptions of special subclasses of models, their generalization being left vague and often shading off into logical vacuity. Let me give two examples. The first is from quantum mechanics: *Schrödinger's equation*. This is perhaps its best known and most pervasively employed law – but it cannot very well be an axiom of the theory since it holds only for conservative systems. If we look into the general case, we find that we can prove the equation to hold, for some constant Hamiltonian, under certain conditions – but this is a metamathematical fact, hence empirically vacuous. The second is the Hardy-Weinberg law in population genetics. Again, it appears in any foundational discussion of the subject. But it could hardly be an axiom of the theory, since it holds only under certain special conditions. If we look into the general case, we find a logical fact: that certain assumptions imply that it describes an equilibrium which can be reached in a single generation, and maintained. The assumptions are very special, and more complex variants of the law can be deduced for more realistic assumptions – in an open and indefinite sequence of sophistications.

What we have found, in this approach, is a way to describe relevant structures in ways that are also directly relevant, and seen to be relevant, to our subject matter. The scholastically logistical distinctions that the logical positivist tradition produced – observational and theoretical vocabulary, Craig reductions, Ramsey sentences, first-order axiomatizable theories, and also projectible predicates, reduction sentences, disposition terms, and all the unholy rest of it – had moved us *mille milles de toute habitation scientifique*, isolated in our own abstract dreams. Since Suppes' call to return to a non-linguistic orientation, now about thirty years ago, we have slowly regained contact.

3. Theory structure – relation to the world

Above I mentioned Giere's elegant capsule formulation of the semantic view: a theory is presented by giving the definition of a certain kind (or kinds) of systems plus one or more hypotheses to the effect that certain real (kinds of) systems belong to the defined class(es). We speak then of the *theoretical definition* and the *theoreti-*

cal hypothesis which together constitute the given formulation (in, so to say, canonical form) of the theory. A 'little' theory might for example define the class of Newtonian mechanical systems and assert that our solar system belongs to this class.

Truth and falsity offer no *special* perplexities in this context. The theory is true if those real systems in the world really do belong to the indicated defined classes. From a logical, or more generally semantic point of view, we may consider as implicitly given models of the world as a whole, which are as the theoretical hypotheses say it is. There is of course a very large class of models of the world as a whole, in which our solar system is a Newtonian mechanical system. In one such model, nothing except this solar system exists at all; in another the fixed stars also exist, and in a third, the solar system exists and dolphins are its only rational inhabitants. Now the world must be one way or another; so the theory is true if the real world itself is (or is isomorphic to) one of these models. This is equivalent to either of two familiar sorts of formulations of the same point: the theory is true exactly if (a) one of the possible worlds allowed by the theory is the real world; or (b) all real things are the way the theory says they are.

But while the subject of truth yields no special conceptual difficulties in this context, I do not believe that it marks the relation to the world, which science pursues in its theories. This, as you will recall, is the point at issue between scientific realism and empiricism. Leaving the issue itself aside, I think that even scientific realists need to be acutely interested in a much closer, more empirical relation of theory to world. I call this relationship *empirical adequacy*.

The logical positivist tradition gave us a formulation of such a concept which was not only woefully inadequate but created a whole cluster of 'artifact problems' (by this I mean, problems which are artifacts of the philosophical approach, and not inherent in its subject). In rough terms, the empirical content of a theory was identified with a set of sentences, the consequences of that theory in a certain 'observational' vocabulary. In my own studies, I first came across formulations of more adequate concepts in the work of certain Polish writers (Przeleweski 1969, Wojcicki 1974), of Dalla Chiara and Toraldo di Francia (1973 and 1977) and finally of course in Patrick Suppes' own writings on what he calls empirical algebras and data models (1967, 1969). While some of these formulations were still more language-oriented than I liked, the similarity in their approach was clear: certain parts of the models were to be identified as *empirical substructures*, and these were the candidates for representation of the observable phenomena which science can confront within our experience.

At this point I perceived that the relationship thus explicated 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. These

actual physical structures are to be embeddable in certain substructures of space-time, which allows however for many different possibilities, of which the actual is, so to say, some arbitrary fragment.

Thus we see that the empirical structures in the world are the parts which are at once *actual* and *observable*; and empirical adequacy consists in the embeddability of all these parts in some single model of the world allowed by the theory.

Patrick Suppes has very carefully investigated the construction of data models, and the empirical constraint they place on theoretical models. Thought of as concerned with exactly this topic, much apparently 'a prioristic' theorizing on the foundations of physics takes on a new intelligibility. A reflection on the possible forms of structures definable from joint experimental outcomes yields constraints on the general form of the models of the theories 'from below' which can then be narrowed down by the imposition of postulated general laws, symmetry constraints, and the like, 'from above'.

4. Theorizing: data models and theoretical models

New theories are constructed under the pressure of new phenomena, whether actually encountered or imagined. By 'new' I mean here that there is no room for these phenomena in the models provided by the accepted theory. There is no room for a mutable quantity with a discrete set of possible values in the models of a theory which says that all change is continuous. In such a case the old theory does not allow for the phenomenon's description, let alone its prediction.

I take it also that the response to such pressure has two stages, logically if not chronologically distinguishable. First the existing theoretical framework is widened so as to allow the possibility of those newly envisaged phenomena. And then it is narrowed again, to exclude a large class of the thereby admitted possibilities. The first move is meant to ensure empirical adequacy, to provide room for all actual phenomena, the rock-bottom necessary condition of success. The second move is meant to regain empirical import, informativeness, predictive power.

It need hardly be added that the moves are not made under logical compulsion. When a new phenomenon, say *X*, is described it is no doubt possible to react with the assertion that if it looked as if *X* occurred, one would only conclude that familiar fact or event *Y* had occurred. A discrete quantity can be approximated by a continuous one, and an underlying continuous change can be postulated. From a purely logical point of view, it will always be up to the scientists to take a newly described phenomenon seriously or to dismiss it. Logic knows no bounds to *ad hoc* postulation. This also brings out the fact of creativity in the process that brings us the phenomena to be saved. Ian Hacking put this to me in graphic terms when he described the quark hunters as seeking to create new phenomena. It also makes the point long emphasized by Patrick Suppes that theory is not confronted with raw data but with models

of the data, and that the construction of these data models is a sophisticated and creative process. To these models of data, the dress in which the debutante phenomena make their debut, I shall return shortly.

In any case, the process of new theory construction starts when described (actual or imagined) phenomena are taken seriously as described. At that point there certainly is logical compulsion, dimly felt and, usually much later, demonstrated. Today Bell's Inequality argument makes the point that certain quantum mechanical phenomena cannot be accommodated by theories which begin with certain classical assumptions. This vindicates, a half century after the fact, the physicists' intuition that a radical departure was needed in physical theory.

Of the two aspects of theorizing, the widening of the theoretical framework, and it's narrowing to restore predictive power, I wish here to discuss the former only. There we see first of all a procedure so general and common that we recognize it readily as a primary problem-solving method in the mathematical and social as well as the natural sciences, any place where theories are constructed, including such diverse areas familiar to philosophers as logic and semantics. This method may be described in two ways: as *introducing hidden structure*, or 'dually' as *embedding*. Here is one example.

Cartesian mechanics hoped to restrict its basic quantities to ones definable from the notions of space and time alone, the so-called kinematic quantities. Success of the mechanics required that later values of the basic quantities depend functionally on the earlier values. There exists no such function. Functionality in the picture of nature was regained by Newton, who introduced the additional quantities of mass and force. Behold the introduction of hidden parameters.

The world 'hidden' in 'hidden parameters' does not refer to lack of experimental access. It signifies that we see parameters in the solution which do not appear in the statement of the problem.

We can 'dually' describe the solution as follows: the kinematic relational structures are embedded in structures which are much larger – larger in the sense that there are additional parameters (whether relations or quantities or entities). *The phenomena are small but chaotic; they are treated as fragments of a 'whole' that is much larger but orderly and simple.* This point could, I believe, be illustrated by examples from every stage of the history of science. When a point has such generality one assumes that it must be banal, and carry little insight. In such a general inquiry as ours, however, perspective is all; and we need general clues to find a general perspective. (That this particular point may be productive of more specific insights is in any case not an unreasonable hope. The most spectacular recent theoretical development may well be the deduction of Maxwell electrodynamics, Einstein geometrodynamics, and the Yang-Mills quark-binding field dynamics from the requirement of embeddability in space-time, by Hojman, Kuckar and Teitelboim.)

5. Theorizing illustrated

In order to illustrate the general view of what theorizing is like, which I have just presented, and to try and persuade you that it is a reasonable one, I shall now describe some recent activities in the foundations of quantum mechanics.

The whole point of having theoretical models is that they should fit the phenomena, that is, fit the models of data. So we need to look at what the latter must be like in general. Hence the development of Randall and Foulis', 'empirical logic'; Mackey, Jauch, and Piron's preliminary discussions of experimental questions; Ludwig and Mielnik's filters; and so forth. These authors write sometimes as if their program is one of transcendental deduction: study what the data models must be like, deduce what structure theoretical models must have if the data models are to be deducible, demonstrate the basic axioms of quantum mechanics as corollaries to this deduction. Since the theory has clear empirical content, success can be, at most, partial; but it is astonishing how much can be achieved in this way. Moreover the very fact that success must necessarily be partial is what gives the approach its value for the future: it brings within our ken alternatives to the extant theory that can rival it.

In Suppes' description, the experimentalist brings to the theoretician a small relational structure, constructed carefully from selected data. The examples Suppes mentions are specific and the little structures are algebras; hence he calls them 'empirical algebras'. The authors in quantum mechanics point to such small structures that represent data, and they are not always algebras; they are most generally partial algebras, or just partially ordered sets with some operations. Let us see how this happens.

In the typical sort of experiment discussed in connection with the Einstein-Podolski-Rosen paradox and Bell's Inequalities, we have two apparatus, L (for 'left') and R. Each has (say) three settings or orientations; let, for instance, L1 be the proposition that L has been given the first setting. The experiments have each (say) two distinct possible outcomes, which we may represent by the numbers zero and one. Let, for instance, L30 be the proposition that L has the third setting and outcome zero.

When we carry out a particular run on this dual apparatus, we can give a score of T (for 'true') or F to some of these propositions. For example, the first time we do it, each apparatus was placed in the first setting; L had outcome 1 and R had outcome 0. An experimental report looks, in part, as follows:

Proposition	Score
L1	T
L2	F
R1	T
L10	F
L20	no score
R10	T

We note that L20 received no score. It could have been give F, simply on the basis that L had not been given the second setting. But this is useless information, and does not appear in the experimental report.

This single report is not likely to come to the theoretician's desk. What reaches him rather are reports of the form:

- A) With initial preparation X, the probability of outcome Lia, given setting Li, equals r.
- B) For all initial preparations, the probability of (Lia & Ria), given settings Li and Ri, equals zero.

There was an extrapolation before these conclusions were reached: the extrapolation from found relative frequencies to probabilities. This is regarded as no different from the extrapolation of data points on a graph, to a smoothed curve.

But the report that comes in forms (A) and (B) leads us to a mathematical structure that may properly be called the *data model*. The important relation stated between Lia and Ria in (B) is that when they can receive an informative score at all (i.e., when the preconditions Li and Ri obtain), they cannot both receive the score T. We then call Lia and Ria *orthogonal*. It also means that Ri1 must receive score T when Li0 does (modulo probability zero), and again when the informative scoring conditions obtain; and we call that *implication*. The latter is a partial ordering, and so we have here a partially ordered set (*poset*) with an orthogonality relation.

Reflection on this form of representation leads to assertions of the form: all data models can take the form . . . A popular way to fill in the dots is to say 'ortho-poset' (i.e., poset with orthocomplement). A. R. Marlow, whose work I am about to take as a special example, used the more general characterization *dual poset*; that is, a partially ordered set with zero element and equipped with a single operation, *duality* ($x \neq x'$; $x = x''$; and x implies y only if y' implies x').

In the world of mathematical entities there are many dual posets. Widening our theoretical framework will consist in the provision of models that can have very strange dual posets of experimental propositions embedded. But the embedding must be good, that is, we must be able to see in the theoretical model all the significant features. I mentioned parts of the experimental report labelled (A). They

start 'With initial preparation X, ...' and then they mention probabilities. These probabilities characterize what is called the *state* prepared by procedure X. And these states (they look like fragments of ordinary probability functions, in that they assign probabilities only to propositions for which the informative scoring conditions obtain) must be 'visible' in a certain sense in the computational structure of the theoretical model.

Here is Marlow's theorem. It requires two preliminary definitions. A *probability function* on a dual poset is any function f with the properties $f(0) = 0$, $f(x') = 1 - f(x)$, and $f(x) \leq f(y)$ if x implies y . A *base* for the dual poset is any set B of elements which does not contain the zero element, nor does it contain two orthogonal elements (here defined by the relation (x implies y')), but does contain either x or x' for each x . (Remark: every set with the first two properties can be extended to one that has all three. Note also that any set of elements that have all received score T on a particular occasion is intuitively required to have the first two properties.) The theorem says now that if we have a dual poset and a base, we can embed the poset in the algebra of projection operators on a Hilbert space, in such a way that duality becomes ortho-complementation, the partial ordering is preserved for elements within the base, and each probability function on the poset can be associated with a vector and becomes calculable by means of the familiar trace computation used in quantum mechanics.

This is an extraordinarily general result. Marlow takes the result as justification for his project to write space-time theories in Hilbert space formalism. The result provides good reason, after all, to write all physical theory in that mathematical framework. Of course he realizes that in some ways the theorem is less than totally general (the implication order is preserved only within the base!) and in some ways less than informative (there are enormous Hilbert spaces with room to embed almost anything) but the postulates he adds, and intends to add, will narrow down the *embarras de richesse* to recover empirical content.

Let us look, however, as second main illustration, at a line of thought born from dissatisfaction with the way phenomenal structures are embeddable in the Hilbert space framework. I refer to *operational quantum mechanics*, associated with Ludwig, Mielnik, Davies, and Edwards.

To explain how data models can take the form of ortho-posets, or more generally dual posets, I already gave a brief sketch of a quite typical experimental set-up, of an intermediate degree of complexity. Let me now start an alternative sketch; the two will not be incompatible. In a typical simple test, a system is *prepared* in a certain way; an *operation* is performed on it; and a *question* is asked. In the simplest case the question has *yes* or *no* as possible answers. We may keep count, as we repeat the text, of how often we receive *yes* as answer. We visualize the situation by imagining a *source* which sends out a beam of particles, that encounter a *barrier*, and a *counter* on the other side of the barrier that clicks every time a particle reaches it. This is a good picture for it has all the general features indicated; we appear to lose no generality if we focus attention on it.

The barrier affects the intensity of the beam; for example, if the barrier were not there, the counter would be clicking twice as rapidly. The sort of barrier determines the sort of question being asked. When the situation, so conceived, is embedded in the mathematical apparatus of Hilbert space, each source has an associated statistical operator W (representing the *prepared state*), and each barrier an associated projection operator P (representing the *question asked*); the probability of a single particle passing the barrier (the factor by which the beam intensity is diminished) is calculated by the Born rule as $p = \text{Tr}(PW)$.

Many situations of the general sort described can indeed be modelled in this way. But others have to be treated in more round-about fashion. Intuitively we say that there is an *observable* in such a set-up. Whether that is so in the sense now instilled in us by quantum theory, has a simple criterion: there must be a state such that the *yes* answer becomes certain, that is, receives probability one. That state is then called an eigenstate of the observable being measured.

It is certainly possible to find examples where it looks intuitively as if we are measuring something, but the criterion is not met. Suppose we place an atom of a radioactive substance near a Geiger counter, and ask *Will the counter click within four to five minutes from now?* Is that not a simple form of measurement, of something we could call, say, the decay time of the atom? Yet we cannot prepare the atom in a state so as to make the *yes* answer certain.

A clearer example, first introduced into the literature, I think, by Shimony, and discussed especially by the Dutch authors de Muynk, Cooke, and Hilgevoort, is pictured in Figure 1. We have a battery of three Stern-Gerlach apparatus, testing for spin. They are so arranged that particles exiting from the first, along the top channel, encounter the second, while those exiting in the first top channel will exit along the second top channel, and so forth. We may choose the orientations so that those transition probabilities equal $1/2$. As is easily seen in the diagram, there is no initial state which makes certain an exit along the second top channel; for the probability of

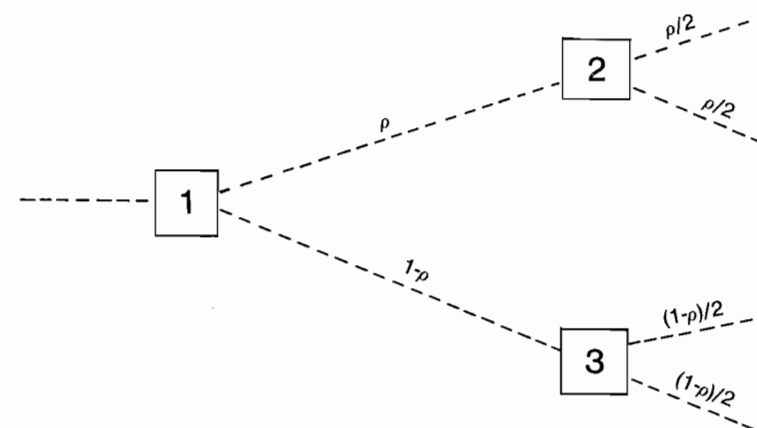


Fig. 1.

that exit equal $p/2$, which has maximum value $1/2$. Here we have a question for which the *yes* answer *cannot* be certain. We can at most represent it by means of a sequence of questions, of the directly representable sort. Perhaps we could say that a 'derived observable' has been measured.

Operational quantum mechanics may be thought of as enlarging the theoretical models so as to allow the embedding of all such empirical structures in the same way. The questions asked by our battery of apparatus are treated on a par with those asked by a single apparatus. This enlargement proceeds as follows. We represent the (ensemble prepared by the) source by means of two parameters: the statistical operator W and a number t which represents the production intensity. The barrier then affects both state W and intensity t , changing W to some other state W' , and reducing the intensity t to rt (with $0 \leq r \leq 1$). We may set the initial intensity equal to 1, so that t itself must belong to the interval $(0, 1)$. Referring to the statistical operators W – positive definite Hermitean operators with unit trace – as the 'old states', we represent the new states by their multiples rW . These are not in general themselves statistical operators, for if W has unit trace then rW has trace r .

Mathematically the enlargement proceeds in several steps. We begin with the 'ball' S of old states; generate the real cone

$$B+ = \{rW : W \in S \text{ and } 0 \leq r \leq 1\}$$

and then enlarge that cone by closing it under the usual linear operations, thus forming a real vector space B , a Banach space with the trace as norm. The physical operations can now be represented uniformly, by considering their effects on 'old state' and trace/intensity.

These two longish examples I take to reveal philosophically especially significant aspects of scientific theorizing. I turn to experimentation.

6. Experimentation: as test and as means of inquiry

Theory construction I have described as being ideally divisible into two stages: the construction of sufficiently rich models to allow for the possibility of described phenomena, and the narrowing down of the family of models so as to give the theory greater empirical content. There must be a constant interplay between the theoretician's desk and the experimenter's laboratory. Here too I wish to distinguish two aspects, of which I shall discuss one at greater length than the other.

The well-known function of experimentation is hypothesis testing. The experimenter reads over the theoretician's shoulder, and designs experiments to test whether the narrowing down has not gone too far and made the theory empirically inadequate. This characterization is simple and appealing; it is unfortunately over-simple. It overlooks first of all the fact that hypothesis testing is in general comparative, and ends not with support or refutation of a single hypothesis but with support

of one hypothesis against another. It overlooks secondly that testability has to do with informativeness; a theory may be empirically adequate – or at least adequate with respect to a certain class of phenomena – but not sufficiently informative about them to allow the design of a test which could bring it support. Hence when we try to evaluate theories on the basis of their record even in the most assiduous testing, the ranking will reflect not only adequacy or truth, but also informativeness. Certainly these are both virtues, and both epistemic values to be pursued (as has been cogently argued by Isaac Levi), but it means that it is a mistake to think in terms of pure confirmation. To give an analogue: the number of votes a candidate receives is a measure of voter support for his platform but it is also a function of media exposure, and so it is not a pure measure of voter support. These reflections clearly bear on Glymour's theory of testing and relevant evidence, and his use of this important and original theory in arguments concerning scientific realism; but I have pursued them elsewhere.

There is another function of experimentation which is less often discussed and in the present context more interesting.

This second function is the one we describe in the language of discovery. Chadwick discovered the neutron, Millikan the charge of the electron, and Livingstone the Zambesi river. Millikan's case is a good illustration. He observed oil droplets drifting down in the air between two plates, which he could connect and disconnect with a battery. By friction with the air, the droplets could acquire an electrostatic charge; and Millikan observed their drifting behavior, calculating their apparent charges from their motion. Thus he found the largest number of which all apparent charges were integral multiples, and concluded that number to be the charge of a single electron. That number was *discovered*. The number of such charges per Faraday equals Avogadro's number! No one could have predicted that! You would think that empiricists would be especially bothered by such a scientific press release. For it appears that by carefully designed experiment we can discover facts about the unobservable entities behind the phenomena.

We must note first however that the division of experiments into means of testing and means of discovery is a division neither by experimental procedure nor by experimental apparatus. The set-up and operations performed would have been just the same if Millikan had made bold conjectures beforehand about that number, and had set out to test these conjectures. The division is rather by function *vis à vis* the ongoing process of theory construction.

The theory is written, so to say, step by step. At some point, the principles laid down so far imply that the electron has a negative charge. A blank is left for the magnitude of the charge. If we wish to continue now, we can go two ways. We could certainly proceed by trial and error, hypothesizing a value, testing it, offering a second guess, testing again. But alternatively, we can let the experimental apparatus write a number in the blank. What I mean is: in this case the experiment shows that unless a certain number (or a number not outside a certain interval) is written in the

blank, the theory will become empirically inadequate. For the experiment has shown by actual example, that no other number will do; that is the sense in which it has filled in the blank. So regarded, *experimentation is the continuation of theory construction by other means*.

Recalling the similar saying about war and diplomacy, I should like to call this view the 'Clausewitz doctrine of experimentation'. It makes the language of construction, rather than of discovery, appropriate for experimentation as much as for theorizing.

7. The language(s) of science

We arrive now finally at the subject which I have mostly tried to banish from our discussion: language. I must admit that I too was, to begin, overly impressed by certain successes of modern logic. Thus one reviewer (John Worrall) of my book *The Scientific Image*, was able to quote the remark in my first paper on the subject, that the interrelations between the syntactic and semantic characterizations of a theory 'make implausible any claim of philosophical superiority for either approach'. The interrelations referred to are of course those described by the generalized completeness proof. I have long since changed my mind about its significance, both theoretical and practical.

To begin with the theoretical point, when a theory is presented by defining the class of its models, that class of structures cannot generally be identified with an elementary class of models of any first-order language.³ The reason is found in the limitative meta-theorems, which brought to light the dark side of completeness. To take only the most elementary example, if a scientist describes a class of models, the mathematical object he is most likely to include is the real number continuum. There is no elementary class of models of a denumerable first-order language each of which includes the real numbers. As soon as we go from mathematics to metamathematics, we reach a level of formalization where many mathematical distinctions cannot be captured – except of course by *fiat*, as when we speak of 'standard' or 'intended' models. The moment we do so, we are using a method of description not accessible to the syntactic mode.

On the practical side we must mention the enormous distance between actual research on the foundation of science, and syntactically capturable axiomatics. While this disparity will not affect philosophical points which hinge only on what is possible 'in principle', it may certainly affect the real possibility of understanding and clarification.

Given this initial appreciation of the situation, shall we address ourselves to language at all? The answer I think is *yes*, not on any general grounds, but for a number of specific reasons.

Before detailing those specific reasons, let us look for a moment at language and

the study of language in a general way. Russell made familiar to us the idea of an underlying *ideal language*. This is the skeleton, natural language being the complete living organic body built on this skeleton, the flesh being of course rather accidental, idiosyncratic, and molded by the local ecology. The skeleton, finally, is the language of logic; and for Russell's contemporaries the question was only whether *Principia Mathematica* needed to be augmented with some extra symbols to fully describe the skeleton.

Against this we must advance the conception of natural language as not being constituted by any one realization of any such logical skeleton. Logic has now provided us with a great many skeletons. Linguists have uncovered fragments of language in use for which no constructed logical skeleton yet provides any satisfactory model. Natural language consists in the resources we have for playing many different possible language games. Languages studied in logic texts are models, rather shallow models, of some of these specific language games, some of these fragments. To think that there must in principle exist a language in the sense of the objects described by logic, which is an adequate model for natural language taken as a whole, may be strictly analogous to the idea that there must exist a set which is the universe of set theory.

So if we now apply our logical methods in the philosophy of science we should, as elsewhere, set ourselves the task of modelling interesting fragments of language specially relevant to scientific discourse. These fragments may be large or small.

In my opinion, choosing the task of describing a language in which a given theory can be formulated, is a poor choice. The reason is that descriptions of structure in terms of satisfaction of sentences is, as far as I can see, generally less informative and less illuminating than direct mathematical (instead of meta-mathematical) description. It is the choice, explicit or implicit, to be formed in almost all linguistically oriented philosophical studies of science. It was the implicit choice behind, certainly, almost all logical positivist philosophy of science.

At the other extreme, we may choose a very small fragment, such as what I have called the fragment of *elementary statements*. Originally I characterized these as statements which attribute some value to a measurable physical magnitude. The syntactic form was therefore trivial – it is always something like 'm has value r' – and therefore the semantic study alone has some significance. Under pressure of various problems in the foundations of quantum mechanics, I broadened my conception of elementary statements in two ways. First, I admitted as possibly logically distinct the attributions of ranges (or Borel sets) of values. Second, I admitted as possibly distinct the attribution of states of certain sorts, on the one hand, from that of values to measurable magnitudes. (It should be added however that I soon found it much more advantageous to concentrate on the propositions expressible by elementary statements, rather than on the statements themselves. At that point there is not even a bow in the direction of syntactic description.)

There are points between these two extremes. I would point here especially to

certain forms of natural discourse that are prevalent in the informal presentation of scientific theory, but which have a long history of philosophical perplexities. The main examples are causality and physical modality. From an empirical point of view, there are besides relations among actual matters of fact, only relations among words and ideas. Yet causal and modal locutions appear to introduce relations among possibilities, relations of the actual to the possible. Since irreducible probability is now a fact of life in physics, and probability is such a modality, there is no escaping this problem. Yet, if we wish to be empiricists, we have nowhere to turn besides thought and language for the locus of possibility. In other words, an empiricist position must entail that the philosophical exploration of modality, even where it occurs in science, is to be part of the theory of meaning.

In sections 1 and 3 I already made clear one important point in the empiricist view of scientific models. They may, without detriment to their function, contain much structure which corresponds to no elements of reality. The part of the model which represents reality includes the representation of actual observable phenomena, and *perhaps* something more, but is explicitly allowed to be only a proper part of the whole model.

This gives us I think the required leeway for a program in the theory of meaning. If the link between language and reality is mediated by models, it may be a very incomplete link – without depriving the language of a complete semantic structure. The idea is that the interpretation of language is not simply an association of a real denotata with grammatical expressions. Instead the interpretation proceeds in two steps. First, certain expressions are assigned values in the family of models and their logical relations derive from relations among these values. Next, reference or denotation is gained indirectly because those model elements *may* correspond to elements of reality. The exploration of modal discourse may then draw largely on structure in the models which outstrips their representation of reality.

A graphic, if somewhat inaccurate way to put this would be: causal and modal discourse describes features of our models, not features of the world. The view of language presented here – that discourse is guided by models or pictures, and that the logic of discourse is constituted by this guidance – I recommend as a general empiricist approach for a theory of meaning without metaphysics.⁴

Notes

1. I use the word deliberately: it was a tragedy for philosophers of science to go off on these logico-linguistic tangles, which contributed nothing to the understanding of either science or logic or language. It is still unfortunately necessary to speak polemically about this, because so much philosophy of science is still couched in terminology based on a mistake.
2. The impact of Suppes' innovation is lost if models are defined, as in many standard logic texts, to be partially linguistic entities, each yoked to a particular syntax. 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. See section 7.

3. This answers a question posed in another review, the one by Michael Friedman. Unfortunately Friedman assumed the contrary answer, and built part of his critique on that conjecture.
4. For the background of this paper I wish to refer the reader to pp. 21–30 of the Introduction to F. Suppe (ed.) *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1974); Ch. 5 'Theories' of R. Giere, *Understanding Scientific Reasoning* (New York: Holt, Rinehart, Winston, 1979); Ch. 3 section 4 of my *An Introduction to the Philosophy of Time and Space* (New York: Random House, 1970); my book *Scientific Image* (Oxford: Oxford University Press, 1980); Patrick Suppes, 'What is a scientific theory?' pp/55–67 in S. Morgenbesser (ed.) *Philosophy of Science Today* (New York: Basic Books, 1967).

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The Garden in the machine: Gender relations, the Processes of Science, and Feminist Epistemological Strategies

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Feminist inquiry in the natural and social sciences has challenged science at three levels.* In the first place, many beliefs claimed to be well-supported by research in biology and the social sciences now appear as androcentric. Thus the processes of inquiry which have supported the androcentric claims no longer appear to be gender-free – and in that sense value-neutral, objective, disinterested, dispassionate, and so forth.¹ In the second place, critics have pointed to constant historical alliances between fledgling sciences and local projects of sexual politics. New sciences have appealed to sexual politics as support for their legitimacy; and men, when threatened by the possibility of shifting social relations between the sexes, have appealed to the new sciences to support the legitimacy of subjugating women. Each has provided moral and political resources for the other.² Furthermore, while the processes of inquiry in physics – the model of objective inquiry – escape incriminating challenges at the first level, they are not so lucky at the second. The conceptions of nature and inquiry central both to classical and contemporary physics now appear as suspiciously androcentric as do those central to biology and the social sciences.³ Thus it should not be surprising to find clear signs of androcentrism in heretofore well-supported scientific beliefs. Sexual politics as old as the Garden of Eden appear to have been omnipresent in the purportedly objective 'mechanisms' of scientific inquiry.

My focus here is not these kinds of criticism, but instead feminist justificatory strategies – feminist epistemologies. These justificatory strategies produce a third level of criticism of science, for they challenge the theories of knowledge developed to explain the cognitive legitimacy of science's processes of inquiry. Very different explanations of and prescriptions for the processes of science can be gleaned from these attempts to explain how it is that feminist inquiry can be producing more adequate support for scientific claims than have purportedly value-free processes.

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