Unity of Science - 1973

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Some 43 years ago the slogan of "Unity of Science" (or "Unified Science" - Einheitswissenschaft) was created by that living dynamo, O. Neurath, taken up by other members of the Vienna Circle, in particular R. Carnap, and quickly adopted by other groups in Berlin (H. Reichenbach, C.G. Hempel), Prague (P. Frank), and various sympathizers of logical empiricism in England, Poland, France, and the United States, approximately in that chronological order. It reached its apex in 1938, when the Institute of Unified Science was founded by Neurath in The Hague, the Journal of Unified Science made its short one-year stand as the successor of Erkenntnis, which had to cease publication after the Nazi annexation of Austria, and the first issues of the Encyclopaedia of Unified Science began to appear.

After World War II, the Institute continued meeting in Cambridge, Mass., under the leadership of Frank, and I vividly remember that at one particular meeting of the group in 1952, in which I had the privilege of participating, there attended, amongst others, in addition to Frank, the physicist P.Bridgman, the biologist G.Wald (both of whom were shortly thereafter to win Nobel prizes in their respective fields),, the statistician R. von Mises, the logician W.V.Quine, the psychologist B.F.Skinner, the linguist R.Jakobson, and - if my memory does not deceive me - another very young linguist, N.Chomsky.. I am mentioning this list of illustrious participants, each of whom is, or was, if he has since died, one of the foremost thinkers in his field, only to indicate that unity of science and the underlying philosophy of logical empiricism was still a going concern at that time, able to attract the best creative minds.

Little did I then imagine that the worms of decay had already begun to undermine this lofty conception. True, Quine himself had already published his "Two dogmas of empiricism", but his criticism sounded rather like a family quarrel, doubtless meant very seriously, and indeed taken very seriously by Carnap, for instance (as I remember from a talk on this topic given by Quine in Chicago before Carnap's seminar in December, 1950, or thereabouts), but Quine was then, and still is to this very day, to the best of my knowledge, as naturalistic (or scientistic, to use a term now in vogue particularly in Europe) an adherent of the unity of science as anybody. Together with his friend and colleague, Skinner, he represented a rigidly behaviorist conception of the human sciences, and together with his colleague, Bridgman, he embraced a strictly operationist methodology in all sciences, including, in contradistinction to Carnap, also the so-called formal sciences.

But then things quickly began to fall apart. With the publication, in 1953, of the Philosophical Investigations, a different Wittgenstein from the one who authored the Tractatus became known to the world and soon caught the fancy of British, American, Scandinavian, and - with some delay - German philosophy. The all-pervading concept of law (of nature) was challenged by the resurrected concept of rule, and theories had to fight off the competition of language games and life forms. Operationism ran into heavy water, the Hempel-Oppenheim covering-law conception of scientific explanation encountered more and more difficulties, and understanding, as a competitor of explanation, raised its head again, not only in Central Europe, where hermeneutics was never really entirely dead, but also in Anglo-Saxon countries and, moreover, in a much more sophisticated and persuasive form. Behaviorism was dealt a heavy blow by Chomsky's famous 1959 review of Skinner's Verbal Behavior, from which it has not yet really recovered, the observational-theoretical dichotomy came under increasingly severe attacks from all sides, mainly, of course, from Popper and his followers, and so did, finally, another major "dogma" of logical empiricism, the strict demarcation between science and metaphysics.

Let me not continue with this child's history of the Unity of Science movement, in which I really have not done much more than throwing around a few names and slogans. Let me rather come to my announced theme: Unity of Science - 1973. I shall concentrate on three issues which seem to occupy a central position in present-day methodology of science, and discuss their relevance to unity of science.

(1) Understanding vs. explanation. This dichotomy, regarded by many as the differentia specifica that separates human from natural sciences, is still very much alive, in spite of having been declared dead over and over again. One proof of its vitality lies, for instance, in the fact that one of the foremost analytical philosophers of our time, G.H. von Wright, entitled his latest book just Explanation and Understanding. I have no intention of discussing this book here nor of entering now into a critical dialogue - to use another fashionable term - with the hermeneutic philosophers of our time, such as J. Habermas and K.-O. Apel (this is being done elsewhere). Here I would only like to stress that the importance of the concept of explanation has been greatly exaggerated in recent methodology of science, thereby unnecessarily increasing the chasm between the so-called "explanatory" and "understanding" sciences. The natural scientist, whether physicist, chemist, biologist,

geologist, or what have you, is, I claim, much less engaged in explanation than is assumed in the standard textbooks or, for that matter, by the working scientist himself, if he finds time to reflect on these matters at all. This claim of mine will probably sound rather strange, and I must elaborate.

Scientists of all brands are certainly engaged much of their time, and in particular when preparing their findings for publication, in systematization, though there will clearly be not only great individual but also disciplinary differences - not necessarily between natural and human sciences. They do this by stating generalizations, and by constructing theories, and by formulating conjectures. But are they really that often engaged in explaining why certain events were bound to occur, or even, with Leibniz, cur potius sit quam non sit? Of course, from time to time they do just that. An astronomer will indeed sometimes explain why a certain full eclipse of the sun occurred at a certain time in a certain region, or even why it was bound to occur. Taking their clue from such facts, some philosophers of science raised this procedure to the status of a standard, valid not only for all natural but also for all human sciences. It is known in the trade as the Hempel-Oppenheim DN (deductive-nomological) or covering-law model of scientific explanation. But Hempel also discussed at great length other models, such as the IS (inductive-statistical) model of explanatory arguments in which the conclusion, the explanandum, while no longer following by logical necessity from the premises, still is obtained with high probability, due to the fact that one of the premises is a statistical law stating that something is the case with high probability, so that an IS explanation still satisfies the Leibniz condition. I shall not enter the by now pretty well-worn discussion of the so-called paradoxes of statistical explanation which result from this conception, as Hempel himself was quick to point out, but rather insist, with W. Salmon and, in particular, W. Stegmüller, that not only is it nothing short of preposterous to force explanation in psychology, sociology, not to mention history, into these straitjackets, a point that has, of course, been made innumerably many times before, - let us only recall the well-known distinction between explanations of the why-necessarily and the how-possibly types - but that countless so-called explanations in the natural sciences (think only of meteorology) are neither of the why-necessarily nor of the why-rather (the Leibnizian) type, though they are often still much stronger than the how-possibly type. A meteorologist why "explains" why a hurricane struck a certain region at a certain time - perhaps contrary to the weather-forecast of the day before - will, in general, be in no position to deduce this proposition from determinate (non-statistical) laws and initial conditions, nor even to present an argument that would provide this proposition with a high probability, but having listened to his "explanation", we educated listeners may become convinced that we have indeed got a better (and sometimes deeper) situational understanding (though less educated listeners, in particular if they had suffered heavy grief from the hurricane, might discount his explanation as so much hogwash). Whether one should at all continue to use, in technical discussions, the term "explanation" for such procedures, is a relatively minor terminological decision, based more on pedagogical than on methodological motives - with regard to non-technical discussions, it would anyway be hopeless to try changing the deeply entrenched customary usage. I, for one (let me not hide it), would definitely propose to reserve, in serious, technical discussions, the use of the term 'explanation' without qualifying prefix for the why-necessarily and why-rather varieties only, and call the other types of explanation, incomparably more frequent and useful in scientific practice, in all scientific practice, and in all sciences, natural as well as human, better-understanding-providing-procedures (or, for short, Stegmüller-explanations). A Salmon-explanation (as I propose to call it), i.e. an argument which provides the explanandum with a posterior probability which is higher than its prior one (and fulfils certain other conditions which will have to be formulated carefully), though not necessarily with a high such probability as does a Hempel-(IS)-explanation - which is, of course, a special kind of Leibniz-explanation - is then a special kind of Stegmüller-explanation. But so is an anti-Salmon-explanation, i.e. an argument which provides the explanandum with a posterior probability which is lower than or equal to its posterior one (but still fulfils certain important conditions), which means that such "explanations" are not really explanations in the ordinary sense, which strengthens my claim that the role assigned to explanations in science has been greatly exaggerated.

For a much more thorough discussion of these matters I recommend that you consult the second half-volume of Stegmüller's Personelle und statistische Wahrscheinlichkeit, 1972.

(2) Reducibility: The original thesis of the unity of science was based on the belief that all scientific concepts are definable in the strictest sense of this word, i.e. eliminatively definable, by terms belonging exclusively to the positivistic language (i.e. language of sense-data), or, alternatively, the physical language. In 1935 it became clear to Carnap that this belief cannot be upheld, and he weakened it to that in the reducibility of concepts, in the technical sense of this term, which he carefully explicated. Today, few, if any, are left who adhere even to this weaker belief. Carnap himself, and with him most logical empiricists, came to realize that most, if not all, terms occurring in scientific theories are neither definable by, nor reducible to, the so-called observational terms of the everyday thing-language, even if no limit is set to the length of the chain of definition and reduction sentences. This was, of course, the end of the thesis of the unity of science, in its original as well as in the weakened form.

It often happened in the history of science as well as in the history of methodology of science that with the death of a thesis (or problem) there arose one or more theses (or problems) which could lay claim to being its lawful inheritors, perhaps emotionally less exciting but, on the other hand, intellectually more interesting. To give an illustration: Hume's original problem(s) of induction may well have been finally laid to rest, but it seems as if a host of interesting and difficult new problems have taken its (their) place, which will probably occupy the attention of the next generation of methodologists.

I am reasonably sure that this is indeed the situation with regard to the unity of science thesis. In 1935, during a talk before the International Congress of Scientific Philosophy, Carnap presented in concise tabular form his views and expectations as to the reducibility of a number of sciences to other sciences, drawing, as usual, careful distinctions between the various claims that can be made under such a slogan, distinctions that were not always heeded in subsequent discussions. Today this table looks rather naive: we can now give such reductionistic claims an incomparably more pregnant and sophisticated formulation than 38 years ago. The problems of reducing biology to physics, psychology to biology, or linguistics to psychology (not considered by Carnap), etc., in their modern formulations, are intellectually more exciting than ever (and I am sure that some of these problems will be discussed in other contributions to our conference) but leave many of us, and certainly myself, emotionally quite undisturbed. Reducibility, as my good friend, Noam Chomsky, would doubtless have commented on this problem, as he did on many other related ones, "is an empirical question" - though I would prefer to add the qualifier "almost entirely", since on closer scrutiny it turns out that some non-empirical, "transcendental" elements may be involved in it. Everybody is entitled to his own hunches with respect to a specific reducibility issue, but the question whether, for instance, the facts of language acquisition by children are better accounted for by assuming a special innate faculté de langage than by relying on general learning theory is almost entirely an empirical one, on condition that the terms are sufficiently sharpened, though some value judgments may be involved in "better". The emotional heat that has been created in the recent debates on nativism and mentalism in linguistics seems to me no more than "a remnant of traditional philosophy, which is occasionally noticeable even with non-metaphysical, empiricist thinkers", to quote Carnap (out of context).

The question whether intensional and other non-extensional logics are reducible to extensional ones, a positive answer to which was for Wittgenstein and Russell a matter of deep metaphysical conviction, was treated by the later Carnap, the man who originally formulated the "thesis of extensionability" as one of the mainstays of Unity of Science, in a very calm and definitely unprejudiced manner.

(3) Theory Dynamics: During the last years, the revolt against logical empiricism has gathered momentum, has spread to ever wider circles, and has occasionally led to an apotheosis of a New Irrationalism by some of the more radical revolutionaries.

One of the major impacts of this development was provided by T.S. Kuhn's book, The Structure of Scientific Revolutions, 1962, which, interestingly enough, was originally solicited by the International Encyclopaedia of Unified Science and indeed was also issued as its vol.II, No.2. Controversial as it was, it certainly put its finger on an essential weakness of logical empiricism: its exclusive preoccupation with the synchronics or statics of theory formation resulting in, or connected with, so it now turns out in hindsight, a very basic misapprehension of the status of theories, resulting, in its turn, in the uncritical acceptance of metamathematics, or proof theory, as the model for the treatment of theories. This neglect of theory dynamics and the relegation of this whole field to purely empirical studies in the sociology and psychology of science created a vacuum which Popper, I.Lakatos, N. Hanson, S. Toulmin, Kuhn and Feyerabend, among others, tried to fill.

But, as has been forcefully argued by Stegmüller in the second half-volume of his Theorie und Erfahrung, 1973, these proponents of theory dynamics continued to adhere to the very same misconception of the status of theories which had misled the logical empiricists they so severely criticized and were, consequently, unable to find a satisfactory solution to the problems they had revealed and even wound up with fierce struggles among themselves, with Lakatos, for instance, accusing Kuhn of promoting "irrationalism","relativism", and "applied mob psychology", and Feyerabend delighting in his well-known anarchistic conclusions.

This basic misconception lies in treating theories as truth-value-bearers, as glorified propositions, though perhaps of certain special structure. This conception has been called by J. Sneed "the statement view" and will be called here "the propositional conception". Now, there have been prior attempts harking back to M. Schlick but later taken up by G. Ryle, Toulmin and others, to deny truth-bearing status of general sentences and to stress their status as inference-licenses, but these attempts got bogged down in the vagaries of "ordinary usage".

The structural (non-propositional) conception, advocated by Sneed and vigorously developed by Stegmüller (under the name "non-statement view"), is of an entirely different type, and among other advantages it permits the representation of the logical aspects of Kuhn's new ideas, aspects which Kuhn himself did not countenance. His professed aim was to challenge the textbook conception of the development of science as a linear accumulation of knowledge. Therein he saw the sources of modern positivism and empiricism, and by exposing its radical inadequacy he intended to undermine the very raison d'être of standard theory of science, since the rationality which its empiricist advocates ascribe to the natural sciences just isn't there.

Kuhn's revolution has, of course, been severely criticized, and he has been accused, for instance, of advocating irrationalism, while in reality he only intended to expose the irrationality of scientists, in normal and extraordinary periods alike. The debate between Kuhn and his opponents, such as I. Scheffler, D. Shapere, and I. Lakatos, was a dialogue des sourdes, a clash of incommensurable paradigms, if ever there was one, with the additional ironical touch that the partners in this dialogue never fully understood the basic nature of their own paradigms. Popper, Lakatos, and the other proponents of the "critical approach" never realized that they had uncritically accepted the propositional conception of the devil, the logical empiricist, carried on endless discussions on verification, falsification, confirmation and refutation of theories which they had simply assimilated to general propositions, and discussed the various immunization strategies by which theories, during period of normal science, are defended against the menace of refutation. Kuhn did not realize that a careful, model-theoretical presentation of his intuitive conception of the nature of theories would show that they were, at all times, immune against falsification to begin with, for the simple reason that under such a presentation theories turn out to be certain structures which just are not truth-value-bearers and therefore can neither be falsified nor need be immunized. Theories, not being refutable by recalcitrant experiences, can only be replaced - through a scientific revolution - by another theory. And all this is perfectly rational, as rational as the behavior of the inhabitant of a bombed house whose roof is leaking and who at first tries to mend the leaks and even, when it finally turns out that this is hopeless, still stays on living in his leaky house until a new house with a solid roof is constructed. It would be irrational of him, wouldn't it, to leave the house in disgust and sleep in the open, only because the roof is leaking.

For someone like Feyerabend, who wants to have fun with science, it is quite alright to advocate "carelessness in semantic matters". And I think that the academic society can afford not only to tolerate such eccentrics but even to support some of them financially by providing them with lucrative academic jobs; after all, court jesters, too, in their time, fulfilled an important function. But <u>if</u> one wants to get a clear understanding of what science and scientific theories are, and how such theories change and relate to each other, then Aristotle, Frege, Gödel, Tarski, and Carnap cannot be left aside.

There is only time left to present the structural conception of theories in its barest outline.

A theory is conceived as an ordered couple $<K, I>$ where K, the structural core, is itself a certain complex ordered quintuple whose elements are classes of potential models, of partial potential models, of special laws, of constraints, and the like, and I is the class of intended models which is, in general, given only intensionally, through some paradigmatic examples. With the help of such concepts as the expanded core, which can vary, while the theory remains invariant, and the class of intended applications known to be true at a certain time, the dynamics of theories can now be rigorously formulated.

Though theories as such are no longer truth-value-bearers, their central empirical (Ramsey-Sneed) propositions (called simply "claims" by Sneed) at any given time are. If the R-S proposition of a theory is falsified, it is perfectly rational to stick to the theory and try a different expansion of the core.

Finally, theory equivalence and theory reduction can be dealt with more precisely and fruitfully under the structural conception.

Let me summarize and draw some conclusions: While the original thesis of unity of science has turned out to be a mirage, more sophisticated versions of this thesis are still in a position to serve as inspiring and fruitful hyper-hypotheses (Oberhypothesen) - which anybody who likes the terms is free to call "metaphysical" or "transcendental" assumptions - for science and theory of science alike.

While there are, of course, important differences between the methods of natural and human sciences, these differences do not reside in that the natural sciences are explanatory and the human sciences - understanding. Explanation is much less, and situational understanding is much more central in all sciences than is customarily assumed. In addition, reducibility of theories is an incomparably more complex matter than originally assumed and can be seriously treated only after a radical gestalt-switch in the conception of theories from the propositional to the structural one.

Such a switch also seems to be needed for the firm establishment of a new metatheoretical discipline, theory dynamics, as well as for the dissolution of a number of current controversies in the methodology of science.

When viewed from the vantage point of the recently proposed structural conception of theories, there seem to remain, after all, no good reasons for not believing that not all scientific theories have the same basic structure.