

On the Term 'Metascience' and the Associated Concept

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Summary

In the past 'philosophy of science' and 'epistemology' served as general terms for the various ways of looking at science. However, neopositivism narrowed the legitimate meaning of such terms to a *particular* way of looking at science, i.e. a formalized reconstruction of logical relationships between propositions.

This was presented as the only worthwhile object of study for epistemology, and also as a description of the actual activity of scientists, and of their only preoccupations.

Such a field could have been more correctly designated as 'logic of science' were it not for neopositivists' (successful) attempt at capturing the general term (philosophy of science) and "purifying" it of all its meanings but theirs.

Small wonder scholars who did not subscribe to neopositivist tenets and inclinations ceased to identify their activity as 'epistemology' and looked for new labels, such as 'history of science' or 'sociology'.

1. The need for the term 'metascience' and the associated concept stems from:

- a) the overly narrow meaning currently given to the expression 'philosophy of science' (also: 'epistemology' in continental European languages) by a large majority of authors;
- b) the growth and maturation of a wide range of different disciplines having science as their object;
- c) the current tendency of such disciplines to expand in diverging directions, paying little or no attention to one another's interests and findings.

2. As far as we know, Aristotle's *Second Analytics* contain the first systematic discussion of science. According to McMullin [*The History and Philosophy of Science*, pp. 41-43], the model of science set forth in the *Analytics* is much closer to Plato's views (science is 'episteme', i.e. knowledge guaranteed as true and absolute; geometry is the paradigm discipline) than to Aristotle's actual procedures as a scientist (e.g., in his biological works). While this bent toward an idealized and formalized representation of science has been dominant through the centuries till the modern times, it by no means prevented a slowly growing number of thinkers to look at science through different spectacles.

Saint-Simon and Comte were probably the first to focus on its organizational aspects and on the role of scientists in future society.

Whewell explicitly stated that philosophy of science should be based on the history of science, and acted to his ideas in grounding his *Philosophy of the Inductive Sciences* [1840] on his own *History of the Inductive Sciences* [1837].

Duhem followed the same principle in his writings on science.

In 1912 George Sarton established "Isis. Revue consacrée à l'histoire de la science" and stated that it should be "the philosophical journal of scientists and the scientific journal of philosophers; the sociological journal of scientists and the scientific journal of sociologists".

In Poland, the yearly "Nauka Polska", supported by the Mianowski Foundation since 1918, published all sorts of essays on science; in 1923 its editor, Stanislaw Michalski, made that policy explicit in an editorial calling for the cross-fertilization of approaches to science considered a research object as any other. These ideas were not entirely new in Poland, and one can trace them back to philosophers and logicians Lukasiewicz and Kotarbinski. In 1925 Florian Znaniecki formulated a full-fledged research programme in the sociology of science [see his later *The Social Role of the Man of Knowledge*, 1940].

At the first quarter of this century, studies of science, while a still scarcely developed field, showed promise to become a field which every interested student could plough his own way.

3. However, the main line of events took a different course.

In the first decade of the century, Bertrand Russell and other British philosophers advanced the claim that the proper task for philosophy was the logical analysis of statements [*The Principles of Mathematics*, 1903]. Such a

claim, which in some authors was accompanied by a rather naive blend of gnoseological realism, had little influence on the development of philosophy itself.

However, through Wittgenstein's *Tractatus Logico-Philosophicus* [1922; W. was a student of Russell's], British analytical philosophy oriented the activity of a group of logicians, mathematicians and physicists, gathering in Vienna between 1925 and 1935, which can be considered the first — at least in the West — active and visible group of people claiming to study philosophy of science.

The central — as far as we are concerned — tenet of this group was that philosophy of science should perform the logical analysis of systems of propositions, and nothing else.

As Wittgenstein had stated in his *Tractatus*, science was the system of true propositions about the world.

Whatever could not be reduced to a proposition in an (axiomatic) system was declared outside of science, and therefore not a proper object of its philosophy.

This ban obviously concerned the process whereby propositions are produced: in this process, the intuitions and creativity of the single scientists was singled out and declared irrational and therefore unfit for analysis.

Reichenbach (a member of a smaller Berlin group following the same orientation as the Kreis) neatly summarized this idea with his distinction between 'context of discovery' and 'context of justification' [*Experience and Prediction*, 1938].

The assumptions, values, decisions, attempts, hesitations, errors, contradictions of the individual scientist were flatly ignored, as well as his relationships with the members of his group, his fellow scientists, the learned community, the public, etc.

Typically, the systems of propositions analysed by members of the Kreis and their followers did **not** belong to any (non-trivial) science — not even physics, as is commonly believed. Probably, actual systems of scientific propositions are too complicated to treat with the simple tools of formal logic, and unsuited to exemplifications. The propositions treated came from what could be called man-in-the-street physics, or man-in-the-street psychology. They were easy to put in syllogistic form; moreover, even non-specialists could easily understand what they were about, and wonder at how such a trivial stuff could be dignified and turned "scientific" by a formalized treatment, or — at a lower level of insight — at how a formalized treatment helped them to understand "physics" or "psychology".

Kreis members were also wise in choosing a label for their movement: Logical Empiricism, i.e. the best of all possible words, the reconciling of what had so far been two polar opposites in Western thought.

The Kreis' highly formalized, aseptic image of science was apt to gratify the self-image of working scientists, whose activity was portrayed as perfectly rational and purified, empirical but also logical.

In particular, the new doctrine was perfectly timed to suit the contingent needs of disciplines — e.g. sociology, political science — struggling to establish themselves as sciences. Ivory tower and white overall were easily sold to young people whose majors had been and were criticized and snobbed for too much "firsthand involvement with the social world" [as Filstead wrote ironically; also see LaPiere's remarks in *Deutscher, What We Say/What We Do*, 1973], as journalists (the Chicago school), philanthropists (Booth, the 'social surveys'), prince's counsellors, reformers, etc.

To would-be social scientists, logical empiricism had something more to offer, viz. the conviction that following the correct method (and using the correct terms) they would automatically achieve the same level of development, and therefore prestige, as the physical sciences.

The term 'behaviour' has played a crucial role in this connection, as it allowed to study human objects with the same intellectual, and even in part the same practical tools, as any other object. That term was already being used as a weapon in the struggle within psychology when the Kreis was formed; however, one of the leading figures in the Kreis, Otto Neurath, seized it and exploited it to its best in his proposal of a 'Behaviorica' (the unified science of all behaviour: of physical, living, moving, speaking, and social objects: see his *Soziologie im Physikalismus*, 1931).

An important role was also played in the same connection by metaterms supplied or sponsored by Logical Empiricism and parallel movements in specific disciplines (operationalism, behaviorism): variable, operational definition, stimulus, experiment, test, reliability, measurement, scale, model, hypothesis, verification, law, etc. These and similar terms came rapidly to be used (often extensively and sloppily) through the social sciences; they performed the same function as a glossy full dress one wears to feel on a par with one's higher-status neighbours.

Small wonder if the doctrine rendering such distinguished services to the communities of scientists came to be enthroned, almost without opposition, as *the* philosophy of science, to the point that — as Mokrzycki has

In other words, Popper “instant fallibilism” was in fact a dead-born.

In what concerns us here, Popper’s attitude was little different from Logical Empiricists’: exclusive focus on systems of propositions, disdain for “psychology of research”, etc. On the other hand, a feature one would hardly find in Carnap, Hempel, etc. is Popper’s attention for selected episodes in the history of physical sciences and natural philosophy. The episodes were selected with the same criterion Popper had so brilliantly criticized in verification -searching scientists : viz., selecting only the episodes favourable to his own thesis (in particular the thesis that sciences progress through bold conjectures rather than through painful data-gathering: see his *Conjectures and Refutations*, 1969) and ignoring all episodes unfavourable to it.

As Mokrzycki has pointed out [*Philosophy*, p. 135], this unembarrassed treatment of historical material has been inherited by many members of Popper’s school: Agassi [see his *Faraday as a Natural Philosopher*, 1972], Berkson [see his *Fields of Force*, 1974], Feyerabend [see his reconstruction of Galileo’s cosmogony in *Problems of Empiricism*, 1970]. Lakatos has explicitly claimed the superiority of this “rational reconstruction” vis a vis plain unvarnished historiography of science [*History of Science and its Rational Reconstructions*, 1970. See sect. 6 for the reactions of historians].

By the way, one could draw an interesting parallel with Parsons’ rational reconstruction of the theoretical positions of Durkheim, Pareto, Walras, and Weber as supporting his own doctrine [*The Structure of Social Action*, 1937].

A really radical alternative to Logical Empiricism in its very heydays could have been offered by a Polish microbiologist working in Lvov, Ludvik Fleck. Influenced by French and Polish conventionalists, neokantians (Brentano through Twardowski) and Polish phenomenologists (Chwistek, Ingarden), he brought to bear on philosophy of science his gnoseological tools of incomparable subtlety. The title of his 1935 book, *Entstehung und Entwicklung einer wissenschaftlicher Tatsache* (only recently translated in English as *Genesis and Development of a Scientific Fact*) beautifully conveys the gist of his main contribution to the philosophy of science; viz., such death-tolls for gnoseological realism as the accurate reconstruction of how a disease, syphilis, far from being a “given” biological fact, had been arrived at after a series of attempts at categorizing and conceptualizing in an acceptable way a vast range of (not self-evidently connected) findings. In ethnomethodological parlance, syphilis was the (occasional, contingent) outcome of a series of negotiations. In the same vein, and by the same means (sophisticated gnoseological analysis of laboratory experience), Fleck showed for instance that neat, rational experimental designs are by no means clear in the experimenter’s minds, and are the fruit of an a-posteriori selection of whatever moves (among the many more attempted) have proved conducive to the outcome obtained — often quite different from the original purpose.

Fleck’s polemical target is the Wiener Kreis, but he is too far ahead to be considered by them, as well as by most of his generation. Barring a few occasional readers (Reichenbach and Kuhn among them), his book will be discovered in the early 1980’s; meanwhile some of his insights have been independently developed, especially by Berger and Luckman and by the ethnomethodologists.

On the other hand, I don’t think that Fleck’s concepts ‘Denkstil’ and ‘Denk-kollektiv’ should be considered as parents of Kuhn’s ‘paradigm’ and ‘normal science’. Denk-kollektiv is a group of scientists, which neither of Kuhn’s concepts is. A Denk-stil is shared by a Denk-kollektiv, therefore by a much more limited set of scientists than those sharing a paradigm.

Besides (as Kuhn points out in his *Foreword* to the English edition of Fleck’s book, pp. x-xii), the former concepts call attention to the ‘social pressure’ aspect, while Kuhn views a paradigm as a set of kantian categories helping to intellectually organize reality.

In the 1930’s, a corollary of the ivory-tower view, i.e. the idea of science and scientist’s value neutrality, is attacked by Marxist or Marxist-inspired sociologists of knowledge [Mannheim, *Wissenssoziologie*, 1931], historians of science [Hessen, *The Social and Economic Roots of Newton’s Principia*, 1931; Bernal, *The Social Functions of Science*, 1939], sociologists [Crowther, *The Social Relations of Science*, 1941], and statisticians [Hogben, *Science for the Citizen*, 1938].

Value neutrality is criticized both on descriptive grounds (claiming that scientific theories and the direction of scientific efforts are more or less directly inspired by class interests) and on prescriptive grounds (advocating that science should serve the interests of the working class, or of social progress in general).

6. After the second world war, while the influence of Logical Empiricism reaches its peak in the social sciences, especially in the United States, the number and variety of alternative approaches to science continues to grow. Wittgenstein emerges from a long silence overthrowing and even ridiculing his own previous gnoseological

realism [*Philosophische Untersuchungen*, 1953]. By the concept of 'linguistic game' he stresses both the conventional and the pragmatic sides of language. Again his views will exert great influence on the philosophy of science. One of his students, Toulmin, brings back to the fore the ancient art of rhetoric, contrasting Aristotle's *Topics* to his wider known *Analytics* [*The Uses of Argument*, 1958]; he accuses Logical Empiricism of being utterly irrelevant to scientific activity [*The Philosophy of Science*, 1953]; he devotes a monumental work to a philosophically oriented historiography of the collective use of concepts, scientific and not [*Human Understanding*, 1972].

Michael Polanyi shows the narrow limits of formalization, pointing to the pervasive role of background knowledge, most of which is so subtle and complex as to escape expression, thence a fortiori formalization [*Personal Knowledge*, 1958; *The Tacit Dimension*, 1962].

Hanson attacks inductivism, i.e. the idea that scientific laws can be and have been arrived at by progressive generalization, and the traditional cumulative view of the progress of science [*Patterns of Discovery*, 1958]. Von Hayek denounces the attitude he labels 'scientism', i.e. the positivist and neopositivist tenet that social sciences should strictly follow the orientations and methods of the natural ones [*The Counter-revolution of Science*, 1952].

Radnitzky exposes the Wiener Kreis' epistemological positions to a thorough and pervasive criticism [*Contemporary Schools of Metascience*, 1968].

Merton starts a tradition of non-Marxist sociological studies of science [*The Sociology of Science*, 1973]. His characterization of scientific ethos (universalism, communism of results, disinterestedness, organized scepticism) will influence many students of his (Barber, Gaston, Storer, Zuckerman, etc.) but also gather harsh critiques from the following generations.

Koyré is probably the first historian of science to strongly emphasize the need for an accurate reconstruction of the cultural environment (including metaphysical beliefs,gnoseological convictions, fads, etc.) of each scientist [*Etudes d'histoire de la pensée scientifique*, 1966]. His critique of Whig history (the habit of seeing past events through the spectacles of present orientations and interests (a specialty of Popper's school, but also an almost inevitable pitfall for retired scientists turned amateurish historians of their own science) may be considered a decisive contribution to the institutionalisation of a profession of historians of science.

In the latest decades, specialists like Pearce Williams [*Should Philosophers Be Allowed to Write History?* 1975], Young, Weimer, McMullin [*The History and Philosophy of Science*, 1976] have constantly tackled the attempts at whig history by philosophers and amateurs.

Kuhn as a historian of science [*The Copernican Revolution*, 1957] has followed the same criteria as Koyré. To the historian's craft, however, he has joined unusual sociological skills in analysing the various processes and institutions in charge of socializing newcomers into a scientific field [*The Structure of Scientific Revolutions*, 1962], while making excellentgnoseological remarks on the function of paradigms in providing common conceptual frameworks and therefore speeding up the exploration of all the theoretical possibilities compatible with the framework itself [*The Function of Dogma in Sociological Research*, 1966].

While he has been correctly charged of using some of his favourite terms (paradigm, revolution) equivocally, still Kuhn by the mastery of several disciplinary skills and the impact of his contributions may be judged to deserve the role of standard-bearer of the new approach to science.

Many further developments both in historiography and in sociology of science take him as their point of departure. For instance, Laudan presents a reconstruction of developments in science rather similar to Kuhn's, while correctly criticizing him for only considering endo-scientific factors rather than the more general cultural climate [*Progress and its Problems*, 1977].

Barnes [*T. Kuhn and Social Science*, 1982] draws a line connecting Kuhn and the latest developments in the sociology of science — some of which owe more, however, to an ethnomethodological inspiration [see e.g. works by H. M. Collins, Latour, Restivo, Woolgar].

7. Most of the developments cursorily reviewed in sect. 6, and several others, go under the label 'sociology of science'. The label also covers direct observation of laboratory life (a field fast developing after the fatal threshold has been crossed by the sociologist for the first time); interview- or mail-surveys of scientists' beliefs, ratings of peers, assessments of past achievements and future programmes; trend studies of inputs (resources, posts, grants) and outputs (papers, patents); frequency counts of citations and detection of co-citation clusters; and so on.

In general, the impression is that the field is presently undergoing the same process of empiricization which characterized sociology in the 1950's and 1960's, and exhibiting the same cases of quantophobia and barefoot

empiricism.

On the other hand, the late heirs of Logical Empiricism show no intention to bridge the gap between their system of axiomatized propositions and the actual life and problems of science. Due to the amazing stickiness of labels, they continue to be recognized by most as the legitimate depositors of the right, or rather the only, way of doing philosophy of science.

Truly philosophical reflections on the nature, possibilities, and limits of scientific knowledge are cultivated by very few, and usually fare under the label 'sociology of science'. Another classical topic for philosophy, the legitimacy of science and its relationships (duties and rights) with society at large, has been explored almost exclusively by the Frankfurt school and other more orthodox Marxist thinkers. There seems to be no recognized label for this kind of studies.

In our opinion, the consequences of this terminological chaos on the orderly development of studies of science cannot be overestimated. There is nothing to be gained from calling 'fishes' the insects, and leaving fishes partly un-named, partly grouped with amphibians; as well as nothing is there to be gained in calling 'fishes' the fishes, plus the cetaceans, the amphibians, the ships, the rubber boats, and whatever happens to float.

Although making no pretence to completeness or authoritativeness, and cherishing no hope of having an effect whatsoever on the present situation, yet we feel that a few commonsensical remarks on what could be a more orderly partition of the field may be not entirely useless.

In the light of what has been argued so far, studies considering the logical structure of systems of propositions should be called LOGIC OF SCIENCE rather than 'philosophy of science' as they are presently called.

The label PHILOSOPHY OF SCIENCE is better preserved for what is typically philosophical, i.e. speculations on the nature, possibilities, limits of science in general, and of particular sciences, and of the role of science(s) in society. As this is just one among the many possible ways of looking at science, the label should *not* be restored to the status of un-differentiated, overarching genus term, as it had before the diffusion of Logical Empiricism. Studies of history of science are fairly well demarcated; as it has been remarked in section 3, however, it seems unwise to label them by the same expression designating their subject. The newly-coined label HISTORIOGRAPHY OF SCIENCE might therefore be adopted, following continental European usage. Besides, broadly gauged reconstructions — à la Kuhn, or Lakatos, or Laudan, with or without prescriptive overtones — of "how science has progressed" should be considered apart from more idiographically oriented historian's works. By analogy with the name usually given the works of Collingwood, Croce, Spengler, Toynbee, etc., the label PHILOSOPHY OF HISTORY OF SCIENCE is an obvious though rather awkward candidate. All non-historical empirical studies of science are presently designated by the expression 'sociology of science'. In this instance, it is not hazardous to forecast that more detailed specializations will sooner or later single themselves out of this too vast set.

Studies of the relationship of science with political power, both in its more visible institutions and under the form of class interests (including studies à la Foucault of the control exerted by power through definitions of the situation, conceptualization, choice of terms, etc.) might possibly be labelled POLITOLOGY OF SCIENCE. Studies of the allocation of resources to and within science (between fields, subjects, research programmes, groups, etc.) and studies of output (inventions, patents, papers, publications) and of technological fallout might be labelled ECONOMICS OF SCIENCE.

Studies of "scientific ethos", professional and group norms, individual values, and related behaviours, may come to be called ANTHROPOLOGY OF SCIENCE, as well as studies of the impact of science and scientists' values and behaviours on the cultural system of (a) society at large, and vice-versa.

PSYCHOLOGY OF SCIENCE need not be confined — as Popper and Logical Empiricists would have it — to the problematic reconstruction of an individual scientist's mental process leading to a hypothesis, a discovery, etc. A whole array of needs and motivations may be imagined to lie behind decisions taken in doing research, as well as behind theoretical and terminological choices made in reporting results. The generalized quest for status that we suspect to have caused the adoption of (or rather lip service paid to) a Logical Empiricist framework is a topic for a (SOCIAL) PSYCHOLOGY OF SCIENCE.

Those who, following suggestions by (the later) Wittgenstein, or perhaps Habermas, focus on the language used by scientists as a special game, or communication system, might wish to be grouped as doing LINGUISTICS OF SCIENCE.

What is there left for a SOCIOLOGY OF SCIENCE? The typical objects of sociology, i.e. the forms and instruments of sociability: not only scientific institutions *qua* institutions, i.e. systems of roles originating

expectations, statuses, etc.; but also forms of division of labour and stratification, with associated tensions and conflicts; group and clique formation (invisible colleges, etc.); such broadly “sociometric” topics as peer ratings, co-citation clusters; and so on. Therefore, in my proposal the proper object of a Sociology of Science is not confined to the subfield now beginning to emerge as Sociology of Scientific Institutions (or: Organizations); while it covers most of them, it also includes the truly sociological (in the sense just described) aspect of the complementary subfield labelled Sociology of Scientific Knowledge.

8. Specialization usually leads to isolation as a visible mark of autonomy. Logical Empiricists are a case in point: in their effort to impose their own specialized way of looking at science, they went so far along the isolationist path as to deny legitimacy or existence to all other disciplines studying science.

This denial has been a reasonable motivation for a drive — noticed by Merton himself — toward isolation in the first year of specialized sociology of science too. However, Merton stated that the legitimisation of the new discipline was having the effect of reversing that drive; we have the contrary impression that even minor subfields within (the nebula presently called) sociology of science increasingly tend to ignore each other. If — as we have forecast — new disciplines will begin a struggle for identity within that nebula and separation from it, we must expect more isolationist drives in the future.

We don't know whether this trend toward diaspora is going to be reversed, or is at all reversible. We just suggest that the many scholars who consider it an evil should not neglect the possible impact of terminological measures. Besides being recommendable for taxonomic reasons, a new term-of-genus may act as a reference point for all those, in the various disciplines, who believe that the sound development of a specialty is promoted rather than hampered by a policy of open windows and attention to what the neighbours are doing.

An accepted term-of-genus need not remain a mere symbol; it may favour the establishment of interdisciplinary associations and journals, and the summoning of symposia of scholars interested in listening to the points of view of philosophers, logicians, sociologists, psychologists, etc., on a given aspect of science.

We submit that we do *not* intend here to advocate — as, e.g., Mannheim did — the unification of all these points of view and the creation of a new “more fundamental and inclusive doctrine of science”. While we believe that cross-fertilization is advantageous, we are convinced that unifying designs and drives may only have crippling effects and fictitious outcomes. At any rate, such designs stand no chance whatsoever of success in the present state of the field, and therefore the remote danger of their occurrence should not prevent scholars from trying to fight fragmentation and reciprocal ignorance — an actual predicament rather than a remote danger — by the establishment of an arena for interdisciplinary contact and debate.

9. The concept of [[something placed above the different ways of looking at science]] is far from new, though I am as yet unable to tell when that ‘something’ was a new super-discipline from when it was just an empty term-of-genus, a confrontation arena as we are proposing here.

The already (sect. 2) mentioned 1923 editorial by Stanislaw Michalski on “Nauka Polska” seems to lean toward the idea of an arena. The same spirit seems to inspire the activity of a group of Polish scholars debating problems of ‘Nauka o nauce’ (science of science) around the journals “Nauka Polska” and “Organon” between 1928 and the war [see Krauze et al. in Merton (ed.) *Sociology of Science*].

In the same period Bernal, the British historian of science, proposed “science of science” as a specialized discipline devoted to the “self-consciousness” of science — something possibly similar to what we would call ‘philosophy of science’; see sect. 8.

In 1963, the Polish Academy of Sciences has established a committee on science of science, chaired by Ignacy Malecki; we confess ignorance as to its orientation. Interdisciplinary groups for Wissenschaftsforschung (research on science) or Wissenschaftswissenschaft (science of science) have been created in the 1970's in several German universities (Bielefeld, Ulm) and academic centres (Max-Planck Institut in Starnberg, Erlanger Institute for Society and Science); some of them are no more active, however. Similar groups are still active in the United States, e.g. by M.I.T. In 1982 an Association of Science of Science has been established in People's Republic of China.

A final note on the term suggested to designate the above described concept. In English, three labels occur to me as eligible: ‘science studies’, ‘science of science’, and ‘metascience’. My preference goes to the latter, as being less cumbersome. Moreover, ‘science studies’ are — as far as I understand — already connoted as exclusively empirical, and mainly sociological.

The first occurrence I could find of the term ‘metascience’ is in the title of a 1950 paper by Ajdukiewicz that I could not read. Then Michael Polanyi used it in *Personal Knowledge* [1958, p. 344]. In 1968 Gerard Radnitzky

used the term in the title of his book *Contemporary Schools of Metascience*.

In 1985 the Australasian Association for the History, Philosophy, and Social Studies of Science started an annual review under the title "Metascience". In 1984 the same term was used as the title of a special issue, edited by the present author, of the Italian journal "Sociologia e Ricerca Sociale", with essays by Toulmin and several Italian scholars.