

THE DIFFERENT STRATEGIES IN HISTORIOGRAPHY OF SCIENCE. TENSIONS BETWEEN PROFESSIONAL RESEARCH AND POSTMODERN IGNORANCE

Michał KOKOWSKI

Institute for the History of Science,
Polish Academy of Sciences, POLAND
michal.kokowski@gmail.com

Abstract

History of science, as a branch of knowledge, is a discipline of two countenances. On the one hand, as a kind of history, it is one of the humanistic disciplines. As this type of discipline it interacts with other humanistic disciplines, such as general history, methodology of history, philosophy (including epistemology, methodology, rhetorics, etc.), sociology, theory of literature, theory of cognition, etc. Moreover, history of science is one of the branches of knowledge called science studies or science of science, which also include philosophy of science (with epistemology, methodology, rhetorics, ontology, ethics, theory of values), sociology of scientific knowledge, and politics of science. Hence, history of science develops in strict relationships with the branches mentioned.

On the other hand, as a kind of reflection on science, history of science interacts with science itself (or more precisely, with particular sciences themselves).

Such a dual nature of history of science lays the groundwork for various approaches to this branch of knowledge, different methods of researching history of science and writing about it, based on different combinations of the so-called internal and external factors of science development. As a consequence, we observe the existence of a wide spectrum of different interpretations of the history of science. The mentioned spectrum extends from a detailed, professional case study approach, to the ignorant postmodern approach. In this paper I would like to develop the issues sketched.

Let us introduce two terms at the very beginning: “the historiography of science” and “a historiography of science”.

By the term “the historiography of science” I mean “the study of the way history has been and is [(added by M.K.): analysed and] written — the history of historical writing” (Furay, Salevouris (1988) p. 223).

From the history of this branch of knowledge we know that there were and there are many different approaches to study the history of science. In consequence, we can show *many distinct historiographies of science*, that is, different ways, styles of studying and writing on the history of science.

Thus, we may conclude that *the historiography of science* is composed, among others, of *distinct, different historiographies of science*. Let us list them explicitly:

1. historiography of science analysed and written from the point of view of positivistic philosophy, that is positivistic historiography of science, which we may define as Montuclian, Whewellian, Tannerian, Machian, Sartorian, Duhemian, Conantian, Crombian, Neugebauerian, ...,
2. historiography of science analysed and written from the point of view of history of ideas, which we may define as Lovejoyan, Koyrian, ...,
3. historiography of science analysed and written from the point of view of contemporary philosophy of science, which we may define as Fleckian, Bachelardian, Popperian, Kuhnian, Lakatosian, Feyerabendian, Hackingian (historical epistemology), ...
4. external, social, sociological or socio-political historiography of science, which we may define as Marxian (Hessen's, Bernal's), Bloorian – Barnesian – Shapinian (*The Strong Programme in Sociology of Scientific Knowledge*), Collinian (*The Empirical Programme of Relativism*), Latourian – Woolgarian – Knorr-Cettinianian (*Ethno-methodological Approach*), Callonian – Latourian (*Actor-Network Theory*),
5. historiography of science analysed and written from the point of view of cultural studies of science), that is, cultural historiography of science or socio-cultural historiography of science (Dear (1995),
6. postmodernist historiography of science (propagated by "Postmodernists", but severely criticised by the "Friends of science" in the 1990s quarrel of the so-called "Science wars") – see: Sokal, Bricmont (1997), Sokal (2007),
7. rhetorical historiography of science, which we may define as Prellian, Grossian, Dearian, Woolgarian, and Mossian – see: Haris (ed.) (1997), Fahnestock (2008),

Then, using terminology assumed by Russell (1984), Harrison (1987) and Hughes (1997), among the above mentioned kinds of historiographies of science, we may differentiate between two groups:

1. *Whig historiographies of science*
2. and *prig historiographies of science*.

Or, using the terminology assumed by Brush (2007):

1. *Modernist historiographies of science*
2. and *contextual historiographies of science*.

Furthermore, it is also worth applying in this context the terminology applied by Daston (1989), Hacking (1992), (2002), Sturm, Feest (2008), Daston, Peter Galison (2007), Kusch (2009), Pisano, Gaudiello (2009), in their analyses of historical epistemology and to distinguish three kinds of historiography of science:

1. The objective historiography of science (recorded in text-books as a list of data along with the corresponding mathematical laws, and taught through its techniques and objective concepts).
2. The subjective historiography of science (that is, the history of thoughts and experiences of scientists).
3. The effective historiography of science (that is, historiography (a) of epistemic concepts, (b) of the objects of scientific inquiry, and (c) of the dynamics of scientific developments, as they can be extracted from an analysis of scientific texts or practices).

Then, irrespective of the differences between distinct historiographies of science, we can show a simple schema for all such historiographies. Namely, every historiography of science is always written from a specific point of view determined by a certain choice of a set of more or less clear assumptions, concepts and ideas. Let us call this point of view, *the interpretative core* of a historiography of science.¹

We can explain what the role of the *interpretative core* of a historiography of science is using the following comparison taken from the theory and practice of photography, spectroscopy and spectrometry. Namely, the interpretative

¹ I go in this point on footnotes of several scholars such as Józef Tischner, Imre Lakatos, and Gerald Holton. See Kokowski [1997].

core is a kind of a colour filter that transmits a certain limited range of frequency of light. Hence, like a photograph can take different photos of the same objects using different colour filters, we can provide different pictures of the history of science using different *interpretative cores*, that is, using *the different historiographies of science*.

The interpretative core a historiography of science is composed of two strata:

- the purely declarative stratum (it is based on rational argumentations)
- and the non-declarative stratum of a specific climate of thoughts, ethos, spirit or vision (it permeates and supplements the argumentation).

Let us note that the diversity of historiographies of science is caused by two reasons:

- In each historical epoch, there is a variety of possibilities of writing on history of science. This variety of possibilities is caused by the fact that we want to analyse the different combinations of internal and external factors of development in science. To achieve this task, we need to pick out concepts, ideas, etc., stemming from, among others, related branches of history of science, such as philosophy of science, sociology of scientific knowledge, etc. In other words, we can choose different possible interpretative cores.
- Science itself changes its meaning along history (this is reflected, among others, in the different classification of science from ancient times to nowadays).

Furthermore, the interpretative core of each historiography of science is always determined by a certain understanding of science assumed by its authors. Namely, it expresses the answers for such fundamental questions as follows:

- What is science?
- Whether does science differ in something from other branches of culture or not?
- Whether can one identify science with its results only?
- Whether can one identify science with methods and results?
- Whether can one identify science with a certain social institution?
- Whether can one identify science with a certain kind of argumentation and rhetoric?

Hence, each interpreter of the history of science always assumes more or less consciously a certain philosophy of science and ... a certain sociology of scientific knowledge.

Let us notice that according to the statement of Imre Lakatos:

“Philosophy of science without history of science is empty; history of science without philosophy of science is blind” (Lakatos (1971), p. 91).²

Then, according to a degree of the granularity or the level of detail of the interpretative core, we can say on:

- macro-historiographies of science (when we are able to describe science in general aspects)
- and micro-historiographies of science (when we are able to describe science in details).

To better understand the problem of variety of historiographies of science, I postulate to introduce another term – “The Perfect Historiography of Science”. It is an abstract, ideal notion. “The Perfect Historiography of Science” is written by a perfect subject that has a perfect, absolute knowledge on the history of science. (In the margin, let us notice, that this subject may be called “God”).

However, we all know people, not being perfect beings, have limited cognition of the world. This simple remark regards also to the results of our ways of interpretation of the history of science. Hence, we always create limited models of the history of science. In other words, every historiography of science is always only one of many possible cuts of *The*

² Lakatos Imre (1971) *History of Science and Its Rational Reconstructions*, [in:] P. S. A. 1970 , R. C. Buck, R. S. Cohen (red.), Reidel, p. 91-135.

Perfect Historiography of Science. We can illustrate it, by saying that *The Perfect Historiography of Science* can be represented by a sphere, and a *certain historiography of science* – by a circle, one of its cuts.

In this context, the fundamental questions arise:

- Whether are we allowed everything (Feyerabend's "anything goes") in making our historiographies of science?
- Whether are we entirely free in creating the interpretative core of a historiography of science?
- Whether all possible interpretations are equally good?

I have the same answer for these three questions – "Unequivocally No!".

My further *thesis is: Every historiography of science has its advantages and shortcomings*. The great diversity of approaches to the study of the history of science brings about both positive and negative consequences to research in this branch of knowledge – both profits and losses, so to speak. On the one hand, plurality of attitudes can create new promising perspectives in research, if, for example, such approaches are complementary to each other. On the other hand, however, it can be very destructive to research, because it allows for instances of relativism, overspecialization, or for ignorance by authors and propagators of falsely based assumptions (that, nevertheless, have great influence on the broad public).

One problem noticed Edward Harrison in his article *Whigs, prigs and historians of science*, published in "Nature" vol. 329, p. 213–214 (17 September 1987).

"The whig interpretation of the history of science, practised by most scientists according to historians, commits the crime of reconstructing past science in the context of today's science. The prig interpretation, practised by many historians, adopts a superior attitude to historical work by scientist, and from fear of being unhistorical commits what it supposes to be the lesser crime of being unscientific" (quoted from Hughes [1997] p. 21).

"By communicating the history of science only to historians, and deigning not to communicate it to scientists, the historian lives neither in the past nor the present, in a never-never land where ignorance is bliss, unable to evaluate (and necessary to discount) the effect of modern science on modern styles of thought" (quoted from Hughes [1997] p. 22).

In contrast to Harrison (1987), I would like to emphasize that the problem mentioned is much more complicated.

1. We can cultivate the history of science in two fundamental different ways. On the one hand, the history of a particular branch of knowledge can be researched as a part of this branch of knowledge. For example, the history of physics can be researched as a part physics. On the other hand, the history of a particular branch of knowledge can be researched as a part of general history. These are two distinct research perspectives that show different pictures of science.
In the margin, it is worth noticing that Sarton's problem of two cultures –Snow (that is, the culture of Science and the culture of Humanities), and Sarton's third culture –Snow (that is New Humanism) manifest here.
2. The historian of science has every right to write on the history of science in a different manner than the scientist, if the former writes on science in which held true the other classification of science than modern one's.
3. Writing on the history of science, the interpreter (either scientist or historian of science, philosopher of science, sociologist of scientific knowledge, ...) are not allowed to be an ignorant in this branch of knowledge and related branches of knowledge.

Furthermore, we can classify all works on history of science according to the following schema. There are:

- very weak works (treating the subject on a very low level of competence; such works have often a great "impact factor"³),
- mediocre works (treating the subject on a weak level of competence – the phase of "normal science"),
- ordinary works (treating the subject in a normal level of competence – the phase of "normal science"),

³ Nevertheless, I am very skeptical in regards to the application of the idea of "impact factor" and its use to evaluate the real importance and quality of these works. I agree in this point with the stance of the scientific editors, presented in two texts: The European Association of Science Editors (2008) and Andersen (*et al.*) (2009).

- mature works (developing the accepted axioms and treating the subject in a very competitive way),
- premature works (treating the subject in a fresh way, however with insufficient justification of the proclaimed theses),
- innovative works with a low “impact factor” (treating the subject in a very fresh and exciting way, however without a greater impact),
- innovative works with a high “impact factor” (treating the subject in a very fresh and exciting way, with a greater impact – the phase of “revolutionary science”).

Then, on the hand, it is easy to show many examples of great competence in the field of history of science.

Among the mature works, we can mention, for example, the works of Jean-Étienne Montuc – влович Юшкевич (1906–1993) on history of mathematics, the works of Alistair Cameron Crombie (1915–1996) on history of science, etc. Let us notice that these works represent the positivistic historiography of science.

Among the innovative works with a low Impact Factor, we can mention, for example, *The Genesis and Development of a Scientific Fact* by Ludwik Fleck published in 1935, that became popular among philosophers only when *The Structure of Scientific Revolution* by Thomas S. Kuhn was published in 1962.

The latter mentioned work is a good example of the innovative works with a high Impact Factor.

On the other hand, unfortunately, it is not difficult to show many examples of real ignorance in the field of history of science. And both scientists and historians of science, philosophers of science, sociologists of scientific knowledge, etc. are the authors of such wrong works. I give here only two extreme examples.

- The most common mistake proclaimed by scientists is the anachronistic myth that mature exact sciences were born only in modern times.
- One of many important errors made by humanists (mainly experts of literature theory) in the interpretation of the history of science is the creation of the so-called postmodernist historiography of science.

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