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The Methodology and Philosophy of History of Sciences : Recent Issues and Developments

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THE METHODOLOGY AND PHILOSOPHY OF HISTORY OF SCIENCES: RECENT ISSUES AND DEVELOPMENTS

1. META-HISTORICAL STUDIES

The beginnings of the history of science can be traced all the way back to ancient Greece. Diligent historians have identified a pupil of Aristotle named Eudemus of Rhodes as the first historian of science of record.[1] Though his works are no longer extant, he is said to have been the author of histories of arithmetic, geometry, and astronomy, whose use by later writers is responsible for whatever knowledge we have of the Greek origins of those sciences. Eudemus appears to have been a practitioner of the sciences whose histories he was writing, and so it is plausible to presume that his genre of history of science was essentially identical to the one that became relatively common in the eighteenth and early nineteenth centuries; example I have in mind include Joseph Priestley's histories of optics and of electricity. Now, some of you would perhaps relegate such works to the pre-history of the discipline,[2] but I prefer to categorize them as examples of science-oriented history of science; I would add that this type of work continues to exist, that it can be done either well or poorly and so is not necessarily inadequate, and that it constitutes one of several main historiographical genres.[3]

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[1] *Dictionary of Scientific Biography*, vol. IV, pp. 460–65. I owe this piece of information to the mention made by I. Bernard Cohen, "The Many Faces of the History of Science", in *The Future of History*, edited by Charles F. Delzell, Nashville, TN: Vanderbilt University Press, 1977, p. 83.

[2] Cf. Arnold Thackray, "The Pre-History of an Academic Discipline: The Study of the History of Science in the U.S., 1891–1941", *MINERVA*, 18 (1980): 448–73.

[3] Some of the theoretical issues relating to this kind of history of science were brilliantly analyzed in Amos Funkenstein, "Scientists as Historians", the paper presented in Symposium No. 14 "History of Science: Methodology and Philosophies", at the XVIIth International Congress of History of Science, University of California, Berkeley, July 31–August 8, 1985.

Be that as it may, it is well known that other types of histories of science have emerged in more recent times. Philosophy-oriented history of science, or more simply philosophical history of science, began to study the historical record with the aim of deriving general principles about the nature of scientific knowledge, its structure, scope, limits, and relations to other kinds of knowledge. At least since William Whewell this genre has had its ups and downs. What may be called history-oriented history of science also made its appearance and tried to understand the development of science as an integral part of the evolution of human civilization in general. Another genre that may be added to this list is the social history of science, if we take the latter to mean the sociological and/or historical study of the social aspects of science and of the interaction between scientific knowledge and society, that is, the study of science as a social institution.

One of the latest arrivals on this scene is what for lack of a better name may be called professionalized or self-oriented history of science. This is of special interest to us here for several reasons, not the least of which is the fact that this very Congress of which we are participants may be taken as evidence of the existence and of the vitality of the discipline.[4] At one level the self-oriented history of science is the study of scientific development for its own sake, merely because it exists, as it were. It consists of the investigations undertaken and works produced by professional historians of science. There is obviously some overlap between this genre and the previously defined ones, and I call it professionalized rather than professional in order not to imply that the other types of history need be unprofessional. I am not sure whether the birth of professionalized history of science should be dated as the year 1900, when Paul Tannery organized the first international meeting of historians of science,[5] or as the year when the first journal specializing in history of science first appeared, or when the History of Science Society was founded, or when the first university department or doctoral program was instituted. Nevertheless, it is clear that the self-oriented history of science is a twentieth century phenomenon.

These remarks on the history and the sociology of the history of science could be elaborated, but their elaboration is beyond the scope of this paper. In the present context their main purpose is to introduce one of the main themes of these remarks, which is the following: just as the history of science began to be taken seriously when a given science had undergone significant cognitive progress or institutional organization, now that the study of the history of science has experienced both, it is inevitable that this will motivate

[4] This paper was originally prepared for Symposium No. 14 "History of Science: Methodology and Philosophies", XVIIth International Congress of History of Science, University of California, Berkeley, July 31 – August 8, 1985. This published article is a much expanded version of the remarks read there.

[5] I. B. Cohen, "The Many Faces of the History of Science", p. 87.

serious inquiries on a higher plane which take these developments as their subject matter. In other words, the history of the history of science will come into being, in a way analogous to the way that the history of science originated, and for similar reasons. Now, just as the earlier types of history of science were not those that studied the phenomenon for its own sake, it can be expected that students of the history of the history of science will have ulterior motives, such as the desire to practice the object discipline in a different manner, or the need for a more critical and self-conscious understanding of their actual practice, or the philosophical curiosity of understanding how the knowledge produced in the object discipline relates to other types of human knowledge. Because of these ulterior motives, which of course are quite legitimate, and because to speak of the history of the history of science connotes a self-directed, professionalized kind of history, it is better to speak of the philosophy and the methodology of the history of science, to refer to the above mentioned inquiries which are a natural consequence of the progress and the professionalization of the discipline.

I shall presently give some illustrations of this kind of inquiry, but before I do that I want to comment briefly on the other purpose for these introductory remarks, the one pertaining to the sociology of the history of science. It should first be noticed that my five-fold classification of science-oriented, philosophy-oriented, history-oriented, social, and self-oriented history of science reflects in large measure the present socio-institutional structure of the field. That is, allowing for some overlap, each genre tends to be practiced by individuals who have a corresponding institutional background as defined by the academic degree they hold, and whose pedagogic activities consist of teaching corresponding groups of students. It would be possible to undertake a more epistemological characterization of these five genres, but this will not be done here. And it is obvious that it is equally possible to classify historiographic types in other ways. For example, there is the internal-external distinction, which is widely discussed, and, as we shall see later, there is the trichotomy of inductivist, conventionalist, and critical historiography, popularized by Joseph Agassi's work entitled "Towards an Historiography of Science".[6] Now, the main purpose of my sociological classification is to introduce us to this point of view, since it so happens that Professor Agassi's paper[7] discusses issues that are best interpreted from this viewpoint. That will be seen in due course.

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[6] J. Agassi, "Towards an Historiography of Science", in *History and Theory*, Beiheft 2, The Hague: Morton, 1963.

[7] Cf. Joseh Agassi, "Twenty Years After", the paper presented in Symposium No. 14 "History of Science: Methodology and Philosophies", XVIIth International Congress of History of Science, University of California, Berkeley, July 31—August 8, 1985; now published in this volume.

2. HISTORICAL PHILOSOPHY AND PHILOSOPHICAL HISTORY

Rather than surveying the 2300 years that separate us from Eudemus, it is much better to focus on the last twenty three years.[8] In fact, at the beginning of the 1960s two works appeared which are perhaps best described as epoch-making for the issues at hand. Written by a historian and published in 1962, the first one began with the following words: "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed."[9] After a brief chapter of methodological discussion, the author went on to sketch the details of his new image of science. They are so well known that there would be no point in even summarizing them. Suffice it to say that, according to this image, change is essential to science, more exactly that type of discontinuous change which is called revolutionary and which presupposes periods of normality as the points of departure and of arrival. The image was thus doubly historical: methodologically, insofar as it derived from and was tested by history, and—substantively, inasmuch as it attributed to science an historical nature. And there were at least two ways in which the image was philosophical: first because of its generality, and second because of the self-consciousness with which it was constructed; for the author was well aware that to some extent his project involved some historiographical innovations. From the viewpoint of the analysis of the nature of science, we may say that what we had here was a historical philosophy of science; whereas, from the perspective of the present context, the work amounted to a sketch of a philosophy of history of science. It should be no news to anybody that the author I have been speaking of is Thomas Kuhn, and the book is *The Structure of Scientific Revolutions.*

A year later, in 1963, a philosopher published the second one of the two above mentioned works, in which the undertaking is something like the converse of Kuhn's. This author aimed to formulate and illustrate a new way of studying and writing the history of science deriving from philosophy of science, that is, to sketch a new historiography of science based on philosophy; we may say he was advocating a philosophical history of science or outlining a philosophy of the historiography of science. I do not know how many of you remember the words with which he introduced his project. They too are eloquent and provocative enough to deserve repetition:

> The history of science is a most rational and fascinating story; yet the study of the history of science is in a lamentable state [...] The faults which have given rise to this situation [...] stem from the uncritical acceptance, on the part of historians of science, of two incorrect philosophies

[8] Notice that these remarks were written and uttered in 1985.

[9] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (first edition). Chicago: University of Chicago Press, 1962, p. 1.

of science [...] the inductive philosophy [...] and [...] the conventionalist philosophy [...] A third, contemporary theory of science, Popper's critical philosophy of science, provides a possible remedy.[10]

I am of course, referring to Joseph Agassi's "Towards an Historiography of Science". Its details are worth summarizing. First, the book contains both analytical descriptions and critical evaluations. Analytically speaking, he calls inductivist that historiography which presupposes the epistemological principle that scientific theories emerge from facts, which evaluates past theories and observations by the criterion of the latest textbook, and which is best exemplified by Whewell. Conventionalist historiography is the one whose epistemological presupposition is the principle that scientific theories are mathematical instruments for classifying facts, best exemplified by Pierre Duhem; it adopts the nonevaluative stance or relativist criterion whereby past scientific theories and observations can only be judged in the light of the conditions of the time, and so all past dominant theories are equally correct, and none is any better than any other. Popperian historiography is defined in terms of the philosophy of science according to which scientific theories are explanations of known facts and potentially refutable by new facts; it is allegedly best exemplified by none less than Alexandre Koyré; and its evaluations are neither textbook absolute nor historically relative, but dynamic and susceptible of degrees. Now, in my view, Agassi's objections are not directed against inductivist or conventionalist historiographies as such, but against their uncritical use and acceptance, and what he really advocates is not a simple Popperian explanationist historiography of conjectures and refutations, but rather a critical historiography, be it inductivist, conventionalist, Popperian, or whatever.

The last two decades bear witness to the fact that both works accomplished their central aims. The historical approach to studying the nature of science has now become the dominant one in the philosophy of science, while in the history of science the inductivist school has all but ceased to exist and the critical approach predominates in one form or another. This is not to say, of course, that the same applies to their subsidiary aims and secondary points. For example, I am not sure that anyone today would hold that revolutions are as ubiquitous, or different paradigms as incommensurable, or normal science as monistic as the first edition of *The Structure of Scientific Revolutions* makes it sound. However, the extent of the influence here is apparent from the fact that, even when Kuhn's specific theses are denied, normally critics will formulate their own favorite claims by using the conceptual framework invented by him. Analogously, one of Agassi's peripheral critiques concerned the continuist historiography which presupposes that every idea has a predecessor that more or less resembles it, and which

[10] J. Agassi, "Towards an Historiography of Science", p. V.

thus tries to connect the two by means of as many other developments as possible, so as to make the transition between them as smooth as possible.[11] I am not sure that such criticism retains its viability today, when, to cite an example that I happen to be acquainted with, new documentary discoveries about Galileo's early career (primarily on the past of William A. Wallace) have lifted onto a new plane the question of the continuity between Medieval and early modern science.[12] Another one of Agassi's secondary theses was his positive proposal about the technique of error analysis, adapted in part from Koyré's actual practice, according to which the identification of past errors is one of the most fruitful enterprises. Whatever one may think of the philosophical justification of this technique, in terms of the Popperian asymmetry between truth and falsehood, and the epistemological primacy of falsehood, it seems to me that here the pendulum has swung to the opposite extreme, and error-mongering has become a favorite sport on the part of too many historians. This is especially true in areas like Galilean or Newtonian scholarship. Of course, past errors should not be concealed when they are demonstrably present. But my point is, first, that as a matter of socio-historical fact, in fields of my special interest, errors are frequently attributed on the basis of superficial understanding.[13] Moreover, and this is a methological point, there is indeed an asymmetry between truth and falsehood, but it is such that the textual and documentary evidence supporting falsehood-attributions needs to be much stronger than that supporting truth-attributions. Despite these and many other questionable minor issues, the major goals of historical philosophy of science and of critical-philosophical history of science have been essentially realized, and I would add that in this particular instance we may agree with Hegel that the real is rational.

Now, it would be both naive and unhistorical to believe or for me to give the impression that these two individuals and these two works were solely responsible for these developments. On the contrary, there can be no question that in each case there were both more external socio-economic causes and also more internal disciplinary reasons for these two revolutions, respectively in the philosophy of science and in the history of science. Restricting ourselves to the latter reasons, it is well known that the historical philosophy of science did not emerge from nothing, but rather involved the displacement of logical empiricism as the dominant approach, and that the latter had developed into a state of crisis: its difficulties ranged all the way from the so-called paradoxes of confirmation to the apparent inconsistency

[11] *Ibid.*, p. 32.

[12] William A. Wallace, *Galileo's Early Notebooks*, Notre Dame: University of Notre Dame, 1977; *idem, Prelude to Galileo*, Dordrecht: Reidel, 1981; and *idem, Galileo and His Sources*, Princeton: Princeton University Press, 1984. About the last one, see for example my "Wallace on Galileo's Sources", *The Review of Metaphysics*, 39 (December 1985): 335–44.

[13] For a critique of some Galilean scholarship, see my *Galileo and the Art of Reasoning*, Dordrecht: Reidel, 1980.

of elaborating an empiricist interpretation of scientific method while practicing an apriorist methodology in theorizing about science.[14] On the other hand, as Agassi himself admits in his book, the historiographical revolution he advocated was also what corresponded in large measure to the historical practice of Koyré and his followers. In fact, one would expect that Kuhn's and Agassi's contributions would be mere foci of larger processes, given that both were outsiders to the fields they were trying to revolutionize: an historian advocating a revolution in philosophy, and a philosopher advocating a revolution in historiography. Whether the consequences of this fact were as described by Agassi for his own case in his paper,[15] is something I am not sure about.

However, the point about being an outsider raises an issue that deserves further discussion. To appreciate this issue requires a translation of the somewhat psychological and sociological insider-outsider terminology into more methodological language. I do not know if I can do it satisfactorily in the present context. Let me try.

Professor Agassi interprets an outsider to a given discipline as someone who is not a member of the community of its practitioners, but whose work is significantly relevant, though not explicitly endorsed by the leadership. The question I should like to ask is, How does one identify membership in a given scholarly-scientific community? I believe Professor Agassi does have an answer, namely, by determining whether you are one of its leaders or followers, and by conceiving the community as consisting entirely of these two subgroups. But this only pushes the question further, for how do we determine who is a leader and who is a follower, and which is the community where one is either? It seems to me that, even if one should resort to citation indexing data, ultimately what is needed is an examination of one's actual works. So it is the character of the works one actually produces that determine one's disciplinary location, whether or not, and if so with respect to what, one is an outsider or an insider. Now, it has been widely remarked that neither Kuhn nor Agassi have followed up their original intuitions by practicing what they had preached in their classic works; generally speaking and as a first approximation, this seems to me undeniable. For example, consider Agassi's major subsequent work, entitled *Faraday as a Natural Philosopher.*[16] Viewed in a favorable light, this is straightforward philosophy, partly history of philosophy of science, and partly a case study illustrating an epistemological thesis; unfavorably considered, the work has been criticized as a violation of some of the best advice contained in "Towards an Historiography of Science", namely as an illustra-

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[14] For a superb account of these developments, see Harold I. Brown, *Perception, Theory and Commitment*, Chicago: University of Chicago Press, 1979.

[15] J. Agassi, "Twenty Years After". in this volume.

[16] J. Agassi, *Faraday as a Natural Philosopher*, Chicago: University of Chicago Press, 1971.

tion of uncritical historiography characteristic of inductivism, the only difference being that Popperian anti-inductivism replaces Baconian inductivism. Or consider Kuhn's book on *Black-Body Theory and the Quantum Discontinuity*;[17] again, at best this is straightforward history of science, attempting to narrate the details of the origins of quantum theory, but it was also widely criticized as disregarding some of the best insights of *The Structure of Scientific Revolutions.*[18] Thus in the one case the revolutionary historical philosopher went back to history, in the other case the revolutionary philosophical historian went back to philosophy.

Perhaps this sort of thing is inevitable. After all, this is what one would expect in the light of Kuhn's own insight that the initial discoverer of an idea is normally not the one who can most effectively pursue it. Moreover, both original works do contain a mixture of good and bad. In the case of the Kuhnian project we have an oversimplified theory of scientific change as well as a promising approach to scientific rationality oriented toward value-judgment and sociology; and in the case of the Agassian project, we have an historiography oriented toward dogmatic Popperianism mixed with the sketch of a genuinely critical historiography open to all epistemological ideas. Therefore, on the one hand, there is some consolation in the fact that the better instincts of these scholars have refrained them from engaging in the various versions of rational reconstructions of history to which may others have succumbed. On the other hand, it must be somewhat disconcerting for the self-styled Kuhnian sociologists of science, for example, to be dismissed or not appreciated by the one they regard as the originator of their research program.[19] Similarly, it is somewhat disappointing to see Agassi ignore efforts at a constructive elaboration of his original "Historiography" into a genuinely critical historiography, efforts undertaken by the present author and found in a book which modesty prevents me from naming.[20]

3. THE BERKELEY SYMPOSIUM

With these clarifications as a background, we are now ready to examine some concrete issues, as they emerge from some recent papers by Professors

[17] T. S. Kuhn, *Black-Body Theory and the Quantum Discontinuity 1894–1912*, New York: Oxford University Press, 1978.

[18] For example, T. J. Pinch, "Review Essay on Kuhn's Black-Body Theory", *Isis*, 70 (1979): 436–40. A reply to this kind of criticism and others may be found in T. S. Kuhn, "Planck Revisited", *Historical Studies in the Physical Sciences*, 14 (1984): 230–52.

[19] I am referring to such works as Barry Barnes, *T. S. Kuhn and Social Science*, New York: Columbia University Press, 1982; and S. B. Barnes and R. G. A. Dolby, "The Scientific Ethos: A Daviant Viewpoint", *Archives Europeennes de Sociologie*, 11 (1970): 3–25. Kuhn's criticism is explicit in his "Preface", to his *The Essential Tension*, Chicago: University of Chicago Press, 1977, p. xxi.

[20] Maurice A. Finocchiaro, *History of Science as Explanation*, Detroit: Wayne State University Press, 1973.

Agassi, Hollender and Olszewski, and Pera.[21] These were originally presented at a symposium on "History of Science: Methodology and Philosophies", at the XVIIth International Congress of History of Science, held at the University of California, Berkeley, July 31 – August 8, 1985. We will see that, in Professor Agassi's paper, the most striking things are the introduction of some sociological notions to analyze what might be called the social structure of the community of historians of science, and his adoption of Kuhnian terminology to describe other aspects of the field. The central problem in Professor Hollender and Olszewski's paper turns out to be the classification of various sciences, and their most interesting and relevant points seems to me to be that their principles of classification are historical criteria. Finally, Professor Pera appears to criticize a fundamental presupposition of the historical philosophy of science; though he does restrict the discussion to methodology of science, if what he seems to emphasize were correct, then what I called the recent revolution in the philosophy of science would be essentially misconceived. I shall, of course, criticize this thesis.

4. AGASSI'S HISTORIOGRAPHY TWENTY YEARS LATER

Turning to the details of Professor Agassi's remarks, I take them to be primarily an attempt to describe and to justify some changes or lack of changes in his views since the publication of his classic monograph. From this viewpoint, one novelty I find in his use of sociological terminology such as rank-and-file, leaders, and outsiders. These do strike me as potentially fruitful concepts for the sociological analysis of scientific communities in general; in particular, the distinction between leaders and rank-and-file is reminiscent of the distinction between ruling class and ruled class used by political sociologists of the so-called elite school (such as Gaetano Mosca, Vilfredo Pareto, and Robert Michels) to analyze political communities and institutions and forms of government. However, I am not sure how committed Professor Agassi really is to this type of analysis; nor do I see how this reflects any logical, methodological, or epistemological difference *vis-à-vis* twenty years ago. To be more specific, does this mean, for example, that whereas in his "Historiography" he felt that one great advantage of the Popperian approach was its ability to explain the broad outline of the history of science in terms of metaphysical commitments, without recourse to socio-economic causes, he is now inclined to take the latter course? I cannot tell. If so, did he change his mind because he discovered some error in his previous arguments, or for some other reason?

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[21] J. Agassi, "Twenty Years After"; H. Hollender and E. Olszewski, "Regularities in the Evolution of Particular Sciences"; and M. Pera, "Narcissus at the Pool: Scientific Method and the History of Science"; all presented at the XVIIth International Congress of History of Science, now published in this volume.

The other main novelty I find in his paper is his application of Kuhnian terminology to the field. For example, though he is not the only one to use such terminology,[22] he does speak of the Koyré paradigm and of the Popper-Koyré paradigm, whereas earlier he spoke of schools. He also speaks of the community of historians of science, as if the field was one of the normal sciences theorized about in *The Structure of Scientific Revolutions*. And most significantly, in the course of his discussion of the problem of the outsider, he even redescribes some of his own earlier descriptions accordingly. This occurs when, in connection with the puzzle-solving of normal science, he first refers to the fact that "Kuhn says the role of the rank-and-file physicist is to solve certain technological problems which serve the community at large; the theoretical investigations of the leading physicists, then, serve the rank-and-file in their discharge of their recognized task."[23] Then he adds, "I have argued in my 'Historiography' that the self-selected role of the historian of physics is to orchestrate the process of hero worship which the community at large is supposed to perform with the scientist as the hero."[24] At the same time there are implicit and explicit indications that he rejects the Kuhnian account. For example, I do not know what critical point the lengthy preface of his paper is supposed to make other than that Talmudic interpretation and Kuhnian normal science are essentially identical, and hence presumably the latter cannot be taken seriously. At one point he expressly asserts, "I also recognize the fact that communities are often identified by shared prejudices, by paradigms, but this fact is one I dislike and claim that it is no longer true even as a fact in any pluralist society."[25] So the question naturally arises: is Professor Agassi adopting an instrumentalist attitude toward a presumably rival theory? Or is something more significant going on? And if he is adopting such an attitude, does that perhaps have its own significance, such as a re-evaluation of instrumentalism? And would instrumentalism, then, no longer imply, as it did in the old days, a softening of critical standards?

Finally, there are two other points which hardly receive any discussion in his remarks, but which are much more important. One concerns the plausibility of the Popper-Koyré combination. Professor Agassi is as explicit now as he was earlier about the presence of these two components in his proposal. Having had occasion to reflect on this issue which I did not raise in my published critique of his book,[26] I should like to ask, Where are really the similarities between Popper and Koyré? On the contrary, just to give two

[22] Cf. A. Thackray, "The Pre-History of an Academic Discipline: The Study of the History of Science in the U.S., 1891–1941", *op. cit.*

[23] J. Agassi, "Twenty Years After", this volume, p. 59.

[24] *Ibid.*, p. 59.

[25] *Ibid.*, p. 58.

[26] *History of Science as Explanation*, *op. cit.*

examples, it seems to me that whereas Koyre is an epistemological apriorist, Popper is an empiricist, though admittedly a sophisticated rather than naive empiricist; and whereas Koyré holds that scientific problems are derivative from metaphysics, Popper thinks the reverse. Secondly, am I right in having interpreted Professor Agassi's "Historiography" as committed primarily to a critical historiography, rather than to a Popperian oriented one, with the consequence that even today it is possible to practice inductivist or conventionalist historiography in a critical manner?[27]

5. A HISTORICIST CLASSIFICATION OF THE SCIENCE

Focusing now on Professor Hollender and Olszewski's paper, one of their stated aims is "searching for more general models of the development of science",[28] more general, that is, than models which take physics as representative of all disciplines, or which consider other sciences but aim to reduce all developmental patterns to one. What we have here, of course, is the old program of a philosophy of history of science, and the particular sensitivity to interdisciplinary differences constitutes a novel and promising twist. It is obvious that their paper contains no concrete historical investigation designed to support or to illustrate their generalizations, but I presume that they would agree about the necessity of this sooner or later. So we may take their present effort to represent another legitimate and indeed necessary stage of this sort of inquiry, namely that of concept formation, clarification of issues, and formulation of hypotheses and generalizations for later testing and guidance in data collection.

With this ultimate goal, the more direct problem to which they address themselves is that of the classification of various sciences. Although in some of their own summaries they speak of four criteria, my own reading and analysis of their text reveals six. What they are proposing is that sciences can be classified according to (1) the degree to which their development is influenced by internal or by external factors, (2) the degree to which they are definable in terms of method or of subject matter, (3) the degree of changeability of their subject matter, (4) the degree to which the distinction between past and future is relevant, (5) the degree of abstractness of their assertions, and (6) what the authors call "the range of correspondence between diachronically successive assertions".[29] Each of these criteria obviously generates a continuum, rather than two discrete and separate classes; moreover, the various criteria are not completely isomorphic or completely distinct from each other. Nevertheless, we may agree with the authors that a rough

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[27] *Ibid.*

[28] H. Hollender and E. Olszewski, "Regularities in the Evolution of Particular Sciences", this volume, p. 71.

[29] *Ibid.*, p. 74.

combined spectrum results, and that it probably has the mathematical disciplines and the historical ones at opposite ends.

The interesting thing about all these principles of classification, with the exception of the one referring to the degree of abstraction, is that they are all historical criteria. The gap in sophistication and complexity, as well as the family resemblance, *vis-à-vis* what one finds in *The Structure of Scientific Revolutions*, are obvious. I should also add that the last one of their criteria is especially obscure; despite some intense reflection on their text I am not sure I grasp their meaning when they speak of "the range of correspondence between diachronically successive assertions".

Next the authors suggest plausibly that the synchronic interrelations among the various disciplines play a major role in scientific change. How do we study this process? Their answer is: "By the combination: asking or answering (borrowing or lending) either about methods or the subject matter content."[30] What they seem to have in mind here is that the crucial interdisciplinary connections stem from the following considerations: which discipline is asking for methodological advice or for substantive information to which other discipline, and which is giving answers to which; or which is borrowing substantive information or methodological techniques from which, and which is lending these things to which. Thus, they explain, the lending of methodology and of answers to substantive questions tends to occur in the direction from the mathematical to the historical end; hence, the historical disciplines tend to be influenced by methods and knowledge from all the other sciences and by questions posed from outside the cognitive disciplines by social practice, whereas the mathematical sciences tend to develop mainly internalistically but also under the pressure of methodological and substantive requests from all other sciences.

Given these so-called synchronic interdisciplinary connections, it is obvious that a revolution in one field may lead to revolutions in others. The extent to which this happens is a measure of how basic is the science from which revolution spreads. This phenomenon complicates the usual division between internal and external factors, since once a given field has reached scientific maturity, the influence from another discipline could plausibly be viewed as external. The most interesting idea here seems to me to be the formulation of a historical criterion for the "basicness" of a science.

The authors end by distinguishing several types of scientific revolutions. A local revolution would be one affecting a single science; a global revolution would be one involving most of the sciences. They follow Kedrov in distinguishing two types of global revolutions and four types of local ones, but here I again wish they would give us some clarifications. For example, I do not understand the general difference between the two types of global revolutions; they do give Darwinism as an example of the second type, but I

[30] *Ibid.*, p. 75.

wish they had also given an example of the first type. I am puzzled by the fact that they mention the Copernican revolution as an example of local revolution, whereas I would have though it to be a paradigm example of global. At any rate their four types of local revolutions seem to me more like particular instances rather than general kinds.

To conclude, Professors Hollender and Olszewski have very suggestive, if programmatic, things to say about the development of different types of sciences, and they advance a sophisticated classification which historians and historically minded philosophers should find very exciting on account of its historicity.

6. HISTORY AND ANTI-HISTORY IN METHODOLOGY

Let us now examine Professor Pera's paper. Though he comes to us from the land of Giambattista Vico and Benedetto Croce, he has chosen to defend what may be called an anti-historical theme. But he has also the good fortune of teaching at a university which numbers Galileo Galilei among its former associates. So perhaps Professor Pera has been influenced by this former colleague, whose anti-Peripatetic polemic in the dialogue on the two chief world systems contains memorable passages that easily lend themselves to anti-historical interpretation. For example:

> What is more revolting in a public dispute, when someone is dealing with demonstrable conclusions, than to hear him interrupted by a text [...] thrown into his teeth by an opponent? If, indeed, you wish to continue in this method of studying, then put aside the name of philosophers and call yourselves historians, or memory experts; for it is not proper that those who never philosophize should usurp the honorable title of philosopher. [31]

Actually the trained ear can hear mostly echoes of Kant in Professor Pera's paper. But such matters need not detain us any further.

In proposing "an amicable separation" [32] as the metaphor for the relationship between history and philosophy of science, or at least the methodology of science, Professor Pera is well aware that there are other models, such as intimate relationship and marriage of convenience. He also knows that I favor intimate relationship, and that therefore I am going to fault his argument from that perspective. However, it will turn out that, underlying the differences suggested by such pleasing metaphors, we do agree about what may be the essential point. Of course, it may be that we will not agree that we agree, but that is another story.

What is undeniable is that he explicitly states the crux of the matter at one point, toward the end of his argument. Referring to the choice of

[31] G. Galilei, *Dialogue Concerning the Two Chief World Systems* (Translated by S. Drake). Berkeley: University of California Press, 1953 and 1967, p. 113.

[32] M. Pera, "Narcissus at the Pool: Scientific Method and the History of Science", section 1, this volume, p. 82.

methodology, he asserts: "The history of science may guide us, but only in the same way in which the objects on show in a window, or bought by previous customers, are a guide for a purchaser. In the well-filled but also messy window of the history of science, we can only find suggestions or instantiations, not justifications."[33] He repeats the point when he says that "the choice [...] is a free one: it must be argued, of course, but in this argumentation the history of science plays, at the very most, a suggestive, not a probatory role".[34] This distinction between instantiation and justification, or between suggestive role and probatory role, seems to me to be equivalent to what in other contexts one calls the difference between induction and deduction, probable and necessary inference, the method of retroduction and the method of apodictic proof, or indeed between argumentation and demonstration. So the claim is that the theory of scientific method is an inductive science, that reaches merely probable conclusions, and has to engage in those techniques of trial and error and explanation-hunting technically known as the retroductive method. In short, the methodology of science is an empirical discipline. When so formulated, this is indeed a key presupposition of the historical philosophy of science. Professor Pera's critical, destructive arguments may be taken to demolish other less cautious interpretations of the epistemological status of the discipline. Since he spends most of his time with these critique, it is easy to get the impression that he is rejecting any kind of historical dependence. Similarly, in his own positive account of the so-called axiological vindication of method, he frequently appears to be proposing an apriorist conception of methodology. I believe this is an unfortunate and misleading choice of terminology for what is in reality a retroductivist conception. As I reflect more about this essential agreement between myself and Professor Pera, I come across further evidence that I am correct in this interpretation. For example, in his list of the necessary conditions for a justification of scientific method, the first one he writes down is that it should provide an intelligible explanation of "past or present scientific practice at least partly".[35] Elsewhere, he expresses the point in the following very telling and eloquent statement: "Two extreme positions are, of course, to be avoided here. We should not stipulate a normative but arbitrary definition of scientific method which exludes all or most of what has been called science; nor should we simply draw up a list of the uses of the term 'science' or 'scientific method'."[36]

The actual details of Professor Pera's paper are too rich to be exhaustively discussed here. Let me restrict myself to what I feel is the most crucial detail.[37] This is the critical argument whereby he tries to show that

[33] *Ibid.*, section 4, this volume, p. 94.
[34] *Ibid.*, section 4, this volume, p. 94.
[35] *Ibid.*, section 4, this volume, p. 90.
[36] *Ibid.*, section 4, this volume, p. 91.
[37] *Ibid.*, section 3.

methodological principles cannot be deduced from historical facts; for example, he argues that we cannot validly infer that it is rational to accept well-confirmed theories from the premise that in 1905 Einstein considered it rational to accept special relativity. Professor Pera points out quite correctly that in order to have a valid inference we would have to assume or state at least one other claim, namely that in 1905 Einstein was right in considering it rational to accept special relativity. He also shows, again correctly, that this would still be insufficient, for we would need to know also that Einstein's motivation was the relevant one; in other words, we would have to add the premise that in 1905 Einstein considered it rational to accept special relativity because he regarded it as well-confirmed. Next, sooner or later someone would question the inference insofar as the conclusion is a generalization, while the premises refer to a single instance; therefore, we would have to add other historical cases, such as that in 1615 Galileo considered it rational to accept Copernicanism because he regarded it as well-confirmed. Professor Pera also considers the possibility of inferring the same methodological principle from historical generalizations, such as that in 1911 it was rational to accept special relativity because it was well-confirmed, or that in 1687 it was rational to accept Copernicanism because it was well-confirmed. He is again on the right track when he points out that such arguments would be problematic since the historical generalizations would have to be themselves inferred from the individual historical claims, and that the latter inferences would also be problematic. Therefore, the individual historical claims are indispensable. So the upshot of all these critical considerations is the following. In order to derive methodological principles (like the rationality of accepting well-confirmed theories) from historical facts, we need three types of individual claims: first, we need what might be called historical descriptions, for example, that in 1905 Einstein accepted special relativity as rational; second, we need historical evaluations, namely claims of the form: in 1905 Einstein was right (or wrong as the case may be) to accept special relativity as rational and thirdly, we need claims of the form of historical explanations, for example, in 1905 Einstein accepted special relativity as rational because he thought it well-confirmed. So the issue reduces to the question of the logical and epistemological nature of historical explanations and historical evaluations of the type the historian of science is interested in; we need to determine how we can arrive at them, what is presupposed by them, in particular whether general, or philosophical, or a priori principles or concepts are needed. I have dealt elsewhere with the epistemology of historical explanations of this type,[38] and the conclusion I reached was that they are possible and do not presuppose principles, philosophical or otherwise, which are inaccessible to the practitioner of historical inquiry. They might require philosophical-sounding special skills such as conceptual clarity

[38] *History of Science as Explanation, op. cit.*

or acuity of logical analysis, but it is the concrete rather than abstract side of these activities that is most relevant; so the relative autonomy of historiography remains, as does its relevance to the theory of scientific method. I believe an analogous argument could be made for the case of evaluation in history of science, though I am not aware that this has been studied in any great detail.[39] I have to conclude, therefore, that, despite their large measure of truth and insight, Professor Pera's critiques do not succeed in establishing the illegitimacy or hopelessness of inferring methodological principles from individual historical claims.

If I am right on this particular but crucial logical issue, then it is not surprising that, as I pointed out earlier, much of what Professor Pera says on the general essential issue turns out to be acceptable. It would be a case where one's better judgment perceived the ultimate erroneousness of one's logical critique.

7. CONCLUSION

I have argued that the meta-history of science consists of a variety of higher level studies of a historical, methodological, philosophical, or sociological sort, all of which take as their subject matter the history of science, either in the sense of the historiography of science or of the historical process of scientific development. The meta-history of science may thus be regarded as the latest member of that family of genres which make up the historiography of science. This sub-field already possesses two classic works which help to define its character: in Kuhn's *The Structure of Scientific Revolutions* we find the sketch of a historical philosophy of science, and in Agassi's "Towards an Historiography of Science" we have the sketch of a philosophical history of science. More recent contributions may be taken to have advanced the debate somewhat: Hollender and Olszewski have put forth an interesting proposal for a historical approach to the problem of the classification of the sciences; and Pera's analysis of the limitations of historicist methodology may be interpreted as containing the constructive suggestion that history has an important role to play in methodology, if methodology is not subjected to more stringent requirements than science itself, and if the recent move from demonstration to argumentation is extended from science to methodology itself.

Thus we may say that the philosophical history of science and the historical philosophy of science show no signs of losing either their practical momentum or their theoretical potential. If properly conceived they will not be mistaken for substitute-history or for substitute-philosophy, nor will they be confused with each other.

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[39] But see Mary Hesse, "Reasons and Evaluation in the History of Science", in *Revolutions and Reconstructions in the Philosophy of Science*, Bloomington: Indiana University Press, 1980, pp. 1–28.