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Historiography of Science, Its Aims and Methods

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HISTORIOGRAPHY OF SCIENCE, ITS AIMS AND METHODS *

I

Perhaps even more than abstract analysis, the concrete facts of the history of the development of concepts and theories show us their meaning and scope. Many people feel their demand of explanation more satisfactorily answered by a dynamical-genetical exposition than by a statical-ontological analysis. [1] History of science does not *prescribe* how science *should* be cultivated, but how this *has been* done and with what result. "It is self-deceit to study *a priori* the course of the procedures of reason"; [2] this is true, for historical investigation demonstrates that science is not always and not wholly what we would call rational or logical.

For example: Galileo describes the track of a projected body as the result of a perpendicular fall combined with a uniform movement parallel to the surface of the earth (which is circular). Circular movement is to him, not only in the heavens but also on earth, a *natural* movement (as it was for Copernicus in 1543 and for Jean Buridan in 1328). In the beginning of the 17th century Isaac Beeckman went a bit further: he formulated a law of inertia ("what once moves, will move always, if it is not hindered"), a law valid for uniform circular *and* rectilinear movements: both movements having a constant curvature. (He considered a straight line as a circle with infinite radius.) When Descartes then re-

* Lecture delivered in the University of Cambridge on April 29, 1968; in a different version before the Committee on History of Science of the Polish Academy of Sciences on October 25, 1967.

[1] "To go back to the sources is clarifying ideas and helping science instead of paralyzing it." P. Langevin, in: *Revue de Synthèse*, VI (1933), 1.

[2] L. de Broglie, *Matière et Lumière*, Paris, 1937, p. 321.

stricted the principle of inertia to the rectilinear uniform movement, the state (the mode of existence) of a body was no longer determined by velocity and *curvature,* but by velocity and *direction.* This principle of inertia of Descartes and Newton is justified *a posteriori* by the system of mechanics founded upon it, as *this* (and not the older one) enables us to make correct calculations for constructing machines. But the *logical* value of the belief that a circular movement will go on without end, is no less than that of the conception; generally accepted since Newton, that bodies must maintain a rectilinear uniform motion as long as they are not subject to any force.

Our scientific notions, then, are less "logical" than we might believe, accustomed as we are to them since we imbibed their dogmatic presentation in our textbooks. Whereas medieval man felt his need of explanation completely satisfied by reference to "substantial Forms", these seemed empty words to the 17th-century Cartesians, who explained all change by matter and local motion. Consequently, the Cartesians considered Newton's attractive forces as a return to medieval darkness. Yet, in the 18th century, the Newtonians considered matter and *force* as "clear and distinct" principles of explanation: they believed that the twilight of Descartes had been replaced by the light of the "divine Newton." Getting used to something and taking it for rational often amounts to the same. As John Donne put it (1627): "the daily doing takes off the admiration."

The Portuguese navigator D. João de Castro (1548) said that the Ancients were right in denying the existence of antipodes, for this seemed to be against Reason. But the circumnavigation of the globe showed most clearly that there are inhabitated countries in the southern hemisphere: "this experience, what further proof does it want? Today it is evident that the existence of the antipodes is the most *reasonable* thing in the world."

And when Niels Bohr in 1913 violated the Newtonian principle of the "analogy of nature" and introduced electrons moving without loss of energy, he apologized for not giving a real "explanation" of the spectrum laws in the current physical sense: he only established a numerical relation between spectra and other properties of the elements. But in 1921 he used the term "explanation" in a wider sense than in 1913, namely without requiring a concrete image. Evidently he had got accustomed very rapidly.

In quite another sense still Science is co-determined by non-rational influences. Beauty and simplicity were often referred to in relation with the sphericity of the world, the circularity of celestial movements, the law of falling bodies (Galileo), the distances of the planets (Kepler), the laws of chemical composition (Dalton) and those of crystallography (Haüy).

Of course the choice of the most simple device has often been made rather arbitrarily. Here again History teaches us that no logical or philosophical *a priori* rules can show the right way. The important and stimulating desire for simplicity, analogy and unity, which inspired so many successful scientific endeavours, may as well lead us astray.[3]

Perhaps you will wonder why these examples are given. It is done to show that the way in which one cultivates the history of science largely depends on what one thinks in science and about science itself, and also that one's ideas about Science may be clarified by the history of science, *i.e.* by the critical description of its data, methods and theories, *now* as well as in the *past.* Cultivating science is more than putting nature in our service with the help of a clever system: it includes critique of knowledge. History of Science may help the scientist to become conscious of the contents and the scope of fundamental notions like inertia, force, element, compounds, species.

For example: the chemical law of fixed proportions may have a basis in experience, but as a general verdict it is rather a definition *a priori.*[4] Before Dalton, the French crystallographer Haüy arrived at this law, because he was of opinion that to every definite *geometrical type* (crystal form) belonged one definite *chemical type.* Cases of isomorphy ($CaCO_3$—$FeCO_3$) were dubbed as pseudo-morphoses: siderite ($FeCO_3$) was said to have been deposited in the mould of a crystal of calcite, but its microstructure ought to be totally different. This prejudice was useful: Haüy characterized many minerals by their geometrical forms and reduced several other minerals to the same species, because they had the identical geometrical form, though they differed in colour (ruby and sapphire). Nevertheless, isomorphy exists and gives us now an interpretation of indefinite compounds like (Ca, Fe)CO_3.

Another example: in biology the notion of species is still much discussed; it has been said that there exist as many definitions of species as there are taxonomists. In order to assume a stand, one would profit from an insight in the history of classification and of evolution theories. Darwinistic orthodoxy (to which Darwin himself did not wholly belong) has as one of its tenets that changes in the animal types are continuous and

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[3] Dalton's conception of simplicity led to the rejection of Avogadro's Law. To Btrzelius, the supposition of biatomic hydrogen-molecules (implied in Avogadro's hypothesis) would conflict with his dualistic theory of the chemical bond and would destroy the unity and simplicity of the prevailing doctrine. In the long run, however, it was presicely the acceptance of Avogadro's hypothesis that dispelled the chaos in the chemical theory of the first half of the 19th century.

[4] In a great many cases it has been confirmed by experiment that a chemical compound has a fixed composition, and this provides us with the coordinates which enable us to consider inudefinite compounds as "devations" from the rule. *Cf.* R. Hooykaas, "The Species Concept in 18th-century Mineralogy," *Arch. Intern. Hist. Sc.*, No. 18-19 (1952) pp. 45-55; R. Hooykaas, "The Concepts of 'Individual' and 'Species' in Chemistry," *Centaurus*, 5 (1958), 3-4, pp. 307-322.

extremely slow, whereas the adherents of saltatory evolution believe in radical, "sudden", changes. Historical investigation demonstrates that the arguments of both parties are largely of a methodological character and are often tied up with a metaphysical belief of some kind, whereas there is no dissension about the data themselves.

History of science is of great importance in those natural sciences whose typical method is not classification, experimentation or deduction, but historical reconstruction (geology, palaeontology). Thomas Carlyle said: "Only *facts* are important: King John has been here, that is a reality for which I would give any theory in the world." The physicist Poincaré retorted: "King John has been here, that is of no interest to me, for he will not come here again." History does not repeat itself, its facts are unique, but the physical experiment is repeatable. However, geological uniformitarianism or actualism is a theory or a method which tries to "physicalize" these historical sciences, and it is extremely interesting to analyse, not only philosophically, but also historically, the problems at stake between uniformitarians, evolutionists and catastrophists. Differences of a methodological and a metaphysical character often lead to opposite conclusions based on the same facts.

Thus, history of science makes the scientist aware of the fact that, as a consequence of early indoctrination in school and university, he has unwittingly taken sides in ancient methodological controversies, like those between Cartesians and Newtonians about physical method, uniformitarians and catastrophists in geology, empiricists (Cuvier) and idealists (Geoffroy Saint-Hilaire) in zoology, *etc.*

History of science shows that age-old problems are still alive. The opposition of the Natural and the Artificial, *Physis* and *Techné*, which so much occupied Plato, came back in the mediaeval controversy on alchemy: can artificial gold be identical with natural gold? It comes back in the problems of experimental geology and biology, and in that of the synthesis of organic compounds. The recent conflict among Russian chemists on Ingold's theory of mesomerism or Wheland and Pauli's theory of resonance is another manifestation of the ancient contrast between so-called mathematical hypotheses (which need not be true in nature but are useful for calculation and systematization), and so-called physical theories (which pretend to be conformable to physical reality). It played a role in the interpretation of Copernicanism in the 17th century and in the controversy between Ch. Gerhardt and Kolbe on the significance of chemical formulae in the 19th century.

Ancient modes of thought, then, survive in modern disguise: seemingly superseded prejudices often influence even the most revolutionary scientists. Galileo carried the Platonic, and Harvey the Aristotelian heritage with him; Lavoisier never got rid of the remnants of the phlogiston theory which he overthrew. It is a myth that great things in science are exclusively the result of strict logic, exact observation or infallible intuition.

Extra-scientific factors played also an important role: religion, philosophy, technological development, the economic situation, the whole of the social and spiritual climate of a period, influence the quantity and the quality of its science. However much the scientist may try to free himself from these in his method, this cannot lead to a psychical separation: the human mind cannot be divided into watertight compartments (religious, philosophical, political, aesthetical, *etc.*). Generally speaking, this is more easily to recognize for the past than for our own time.

As to the relation between *Science and Religion*, the stories of the Galileo-process, and the quarrel over Darwinism, are generally known. Without any doubt in some countries where the Counter-Reformation was particularly strong, a promising beginning of Science in the 16th century led to little or nothing: Belgium, Poland, Spain, Portugal. As to the influence of the Reformation, it seems that the tenet of the priesthood of all believers ran parallel to a similar liberation from philosophical and scientific authorities: to Palissy not only God's Book of Holy Scripture, but also God's book of Nature had been given to *all* men to know and to read it.[5] Independent and progressive minds were liable to new religious thought as well as to new scientific thought, and thus it is not so strange that in 16th-century Flemish science the Reformed had a preponderant share. Later on, however, the problem becomes different: here people *were born* in the new situation, so that the selective value of it hardly existed. The same is true, when, as in Lutheran countries, the whole people followed the religion of the prince.

At any rate, we should not confound clericalism with religion. As a matter of fact there is a crowd of witnesses to prove that for many scientists religion was a powerful inspiration for their scientific work: *e.g.*, Kepler, Boyle, Newton, Mayer, Maxwell, and even Hutton. The founders of the mechanistic world picture in the 17th century were accused by their conservative contemporaries of reviving ancient materialism, but in their own opinion they were Christianizing science, precisely by making a methodological separation from theology and by propounding a world *mechanism* instead of a world *organism*; they considered themselves "Christian *virtuosi*." One of their leading principles was, as the English Marxist professor Benjamin Farrington (1964) heads one of the chapters of his book on Bacon: "Out with Aristotle, in with the

[5] The reader may be referred to our forthcoming Gunning Lectures, delivered at Edinburgh University in February 1969, and to our article on "Science and Reformation," *J. World History*, 3 (1956), pp. 109-139, reprinted in: *The Protestant Ethic and Modernization*, ed. S. N. Eisenstadt, New York-London, 1968, pp. 211-239.

Bible." They emphasized that to sit down before fact like a little child and to accept facts even if they conflict with reason and venerable tradition, is a religious duty.

The historical relations between *Science and Philosophy*, for the better or for the worse, are so close that it seems superfluous to dwell upon them. The rise of modern science was for a large part a battle against Aristotelianism (to whom it, nevertheless, owes a great deal). Newton's philosophy had to struggle against Cartesian dogmaticism; in the early 19th century, German *Naturphilosophie* exerted great influence on biological thought. Kant underwent the influence of Newtonian physics, whereas he himself influenced the thinking of Cuvier.

The connections between *Science and the Economic and Social Circumstances* are so evident in our own time that historians now recognize that the course of events must always have been co-determined by scientific and technological factors. In the 16th and 17th centuries, Science and Technology flourished in centers of trade and industry, which, like Venice, Nuremberg, Antwerp, London and Amsterdam, were open not only to a new geographical world but also to a new intellectual world. Astronomy, cartography, magnetism were stimulated by navigation; Galileo and Huygens occupied themselves with the problem of determining longitude at sea. Not only admirals and generals enabled the small provinces Holland and Zeeland to withstand Spain, the biggest and richest power of the world: it was also their consmographers, astronomers, engineers, shipbuilders; whereas the windmills compensated for their lack of manpower. The diary of the great 17th-century scientist Isaac Beeckman demonstrates how closely these things were interwoven.

More recently, the First World War stimulated the artifical production of nitrogen compounds which caused an economic crisis in the republic Chile. Thus history of science is an indispensable part of historiography in general.

II

Having considered *what* History of Science is doing, we have to consider *how* it does it: from the problems and the aims we have to go to the *method*. Of course this is the ***historical*** method: as natural sciences are based on positive facts (a "natural history"), so the *history* of these sciences has to be based on facts and testimonies of facts.

But, as Francis Bacon pointed out, one may handle the facts like the spiders, the ants or the bees. The spiders spin their webs out of themselves, they deduce their "facts" on *a priori* grounds; the ants just collect facts, without systematizing them, but the bees collect facts and digest them and put them into an orderly system.

As to the spiders: as in Science itself the seemingly most logical theory is not always the best one, but rather the theory which is most adapted and adaptable to the facts, so in historiography we have to follow our data and not to *interfere* by our clever constructions. Especially in the search for economic, theological, social and other extrascientific causes, it is difficult to avoid the pitfalls of "rationalizing" constructions. For instance, the geocentric system fits in very well with the anthropocentric standpoint of the biblical authors. Theological opposition to Copernicus is often rationally "explained" as due to religious aversion to abandoning the central position of Man for whom all things have been created, according to Genesis 2 and Psalm 8. It would, however, seem to be as rational (though this is never contended!), if the *reverse* had been warmly *applauded* for religious reasons: it would have been an edifying thought that the Incarnation in a humble carpenter's son, living not even in the capital of an insignificant country, would also have taken place, not in the centre of the world, but on an insignificant planet. In fact, however, neither the one nor the other of these opposite plausible explanations turns out to be true: the theological opposition was not based on dogmatical but on exegetical arguments about certain biblical texts; the change of cosmographic position did not influence religious feeling very profoundly: Kepler remained of the opinion that: "all things have been made for Man" and John Donne (1618) pointed out that Copernicus' innovations had not affected anybody's faith.

It seems to be possible to "prove" anything by arranging and selecting facts. In a recent general survey of the history of sciences the "idealism" of science in Germany about 1820 is "explained" by German nationalism; a few pages below the "empiricism" of science in Germany about 1840 is attributed to this same nationalism. [6] Similarly, it does not strengthen our trust in the consistency of sociological explanations when another British author declares Newton's atomism as fitting in with the free economy of the capitalistic system, but the same author explains the *opposition* of 19th-century positivists against atomism (which is now considered as a thoroughly subversive system) by their conformistic philosophy and bourgeois ideology. [7]

Of course, this does not mean that we should not have any working hypothesis, but we have to apply it tentatively, well aware of the fact that human beings are inconsistent in their thoughts and dealings. Sometimes, a working hypothesis leads to good results in historiography: in the 18th century Torbern Bergman put forward a crystal theory which was far from satisfactory. Haüy spoke in a deprecatory way about Bergman's efforts, and pretended to have heard about his theory

[6] S. F. Mason, *Main Currents is Scientific Thought*, New York, 1954, pp. 469 and 474-475.

[7] J. D. Bernal, *Science and History*, London, 1954, p. 595; *Science and Industry in the 19th Century*, London, 1953, p. 162.

only after his own, successful doctrine had been constructed. Internal evidence, provided by Haüy's earliest publications, led me to the supposition that the real situation was different, and that he had started from Bergman's assumptions. Research in the archives of the French Académie des Sciences fully confirmed this hypothesis: in contrast to the published articles, the original text of his communications clearly showed his debt to Bergman. [8]

Quite a different example: the first and convincing demonstration of the fallibility of the Ancients in scientific matters was the discovery by Portuguese mariners that the tropics are inhabitable. This shocking discovery came at the same time that humanism penetrated into that country, that is when an almost superstitious belief in the omniscience of the Ancients was reigning. Which loyalty would prevail? That to the unlearned Portuguese mariners or that to the learned Greek and Roman philosophers? An inner tension must have been the result. Subsequent perusal of 16th century Portuguese litterature gave abundant proof that this was indeed the case. [9]

On the other hand, things do not always run so smoothly: a study of Petrus Ramus' scientific attitude made it a plausible supposition that Ramus' mathematics should lead to an empiristic approach of the teaching of mathematics. But a very careful investigation undertaken by one of my pupils showed that for Ramus himself this appeal to practical mathematics of the unlearned was not made with a view to renovation in method, but for having a guide in selecting topics of practical usefulness. [10]

In this connection it is important that even the accounts the discoverers themselves give of the way they arrived at their results should not always be taken too seriously. Every scientist tries to present his results in a logical demonstration, which is by far not always the same as the historical genesis of his research.

In 1532 Pedro Nunes pointed out that it would be desirable that mathematical authors describe their discoveries in the way they had found them: "In every art the discovery differs much from the tradition: do not believe that the theorems of Euclid and Archimedes have been found in the way they are delivered to us." That is true, but it is doubtful whether it would always be a great help if they had given their own

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[8] R. Hooykaas, "Torbern Bergman's Crystal Theory," *Lychnos,* 1952, pp. 21-54, and "Les débuts de la théorie cristallographique de R. J. Haüy, d'après les documents originaux," *Rev. Hist. Sc.*, 8 (1955), 4, pp. 319-337.

[9] R. Hooykaas, "The Portuguese Discoveries and the Rise of Modern Science," *Boletim Academia da Cultura Portuguesa,* 2 (1966), pp. 87-108, and our forthcoming study on "The Impact of the Voyages of Discovery on Portuguese Humanist Literature," *First International Symposium on the History of the Maritime Sciences,* Coimbra, 1968.

[10] J. J. Verdonk, *Petrus Ramus en de Wiskunde, thesis,* Amsterdam V. U., 1966, pp. 351 ff.

story: the habit of presenting results in a logical exposition easily leads to present also a logical reconstruction of their genesis, instead of a true historical account. Dalton left three different versions of the genesis of his atomic theory, but his laboratory registers show that the authentic story is a fourth one. The autobiographies of scientists, then, bear resemblance to those of politicians.

Whereas the spiders still find favour with many people, the ants are nowadays low in the market. Chronological registration of facts is indispensable, but in itself it leads but to chronicle-writing, and it makes no real historiography. The latter implies selection and evaluation of materials, that is: in history of science too, we have to follow the method of the *bees*. In order to judge fairly, the historian has to approach the thinking, observing and experimenting of the forebears with a sympathetis understanding: he must possess a power of imagination sufficient ly great to "forget" what became known *after* the period he is studying. At the same time, he must be able to confront earlier views with the actual ones, in order to be understood by the modern reader and in order to make history something really alive, of a more than purely antiquarian interest.

In order to be able to do so, the historian of science ought to have some knowledge of modern science as well as of the culture of the period he studies; without knowledge of the religious, philosophical, social and political preconceptions and situations with which our ancestors did grow up, it is impossible to enter, as it were, into their skin, to live, to think, and to act with them, to *become* them, as far as possible.

This is very difficult for a scientist: he is always under temptation of regarding the predecessors as more primitive than the present generation. Few human activities show so much continual progress as Science. No student of philosophy or fine arts, when going back into the past, has a feeling of meeting with childlike or self-deluding minds; no 20th century philosopher would consider Plato a beginner in philosophy. But Aristotle's explanation of the fall of bodies, or his doctrine of chemical composition, are so far away from modern conceptions that it requires exercise and imagination to understand them, let alone to take them seriously, or even to appreciate them.

Therefore the History of Science has its peculiar pitfalls. At first sight it gives the impression of one continuous development from lower to higher. [11] Consequently, history of science easily gives in to an Evolutionism in which each generation only serves to prepare the next one;

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[11] It is evident hat in political history this is different. After the political revolution of 1789, a restoration of the *ancien régime* in France was possible to a certain extent, and outside France the old situation even lingered on for a long time. But after the chemical revolution by Lavoisier, it became impossible (always and everywhere) to return to the old situation in chemistry.

it is only as "precursors" of the heroes of science that the earlier scholars are appreciated. This Evolutionism tends to construct a rectilinear development towards the present situation.

However, we should not put our forefathers in the dock before a court of justice of 20th-century science. Theories that seem absurd from a modern point of view were quite rational in their own time: fitting in with the prevailing philosophy and giving an adequate interpretation of facts then known. In 19th-century historiography of chemistry the alchemists have been reproved because they tried to synthesize substances which 19th-century chemists considered as elementary; in the 20th century, however, they have been praised because, "with deep intuition," they had seen that the metals are not simple. But we should investigate whether facts known in the Middle Ages made the transmutation of metals probable and whether a rational theory that fitted in with the generally received philosophy of the time could be given about it. In fact, this could be done then. Their theory, though completely wrong, bears a truly scientific character: it was based on one series of facts (the properties of the metals) and it was able to give an explanation of an independent series of facts (the affinities of the metals). The quality of the thoughts of our forefathers, then, is not inferior to ours: even today it is sometimes an intellectual pleasure to follow the reasoning of scholastic philosophers about now obsolete problems, and to enter into arguments for theories which have been superseded since long. The medieval *calculatores* mathematized many things that afterwards turned out not to be liable to mathematical treatment, but it was worth while trying it and at any rate in kinematics they paved the way for posterity.

Therefore, we will not, like Lucretius, sit in the well-built temple of wisdom and look down in self-congratulation upon those who „are erring and seeking for the way of truth," but we will join in the struggle: no laughing, no weeping, but understanding is needed. We will enter the labyrinth; we will discover then that our forebears stood at the cross-roads: that they erected the signals of warning against blind alleys. We will see that each generation has a value in itself and did not serve only to produce our excellence. We will recognize that theories appearing absurd now, must have seemed right in their own time and that the same procedures of thought which lead *now* to the *right* theory, led to a *wrong* one in the past, and that the latter gave as much intellectual satisfaction to our ancestors as the right one now gives to us. Sometimes, the "wrong" theories were intellectually superior to the "right" ones: the scholastic doctrine of chemical composition was certainly more profound, though less successful, than that of Dalton; the scholastic philosophers saw difficulties which the moderns happily overlooked: otherwise, they too would have arrived nowhere.

The historian of science has fulfilled an important task when he restores, albeit on another level, the connection with the past, which the pure scientist (as a matter of method), has to cut off again and again, that is, when he makes evident that Ptolemy was a great astronomer, that Stahl's phlogiston theory offered a clever classification of phenomena, that Darwin's opponents were not the silly people they seem after Darwin's canonization by the Church Scientific, that the scientists of the past were as adult, as human, and also as fallible as we are.

"Historiography implies selection and evaluation of facts." Our attention, then, is inevitably drawn towards those facts and theories of the past which led to our actual conceptions. We are, in general, more interested in the Copernicans of the 16th and 17th centuries than in the multitude of their opponents and we are inclined to pass by the weaknesses of Copernicus' arguments. We have a tendency to pass milder judgment on the errors of Darwin's precursors than on those of the protagonists of constancy of species; consequently Lamarck and Geoffroy Saint-Hilaire are much applauded and their phantastic and sometimes crazy opinions are covered up, but Linnaeus and Cuvier are severely condemned as if they were rigid conservatives. Whereas the "general" historian will show a keen interest in civilisations that came to nothing (*e.g.*, that of the Manichees in Turkestan), most scientists studying history of science will have to conquer a certain revulsion when tackling topics like *Naturphilosophie*, which tries to unveil the mysteries of nature by a method we have learned to consider as unscientific.

Another pitfall for the historian of science is hero-worship. Especially in popular writings great scientists (Galileo, Newton, Darwin) are depicted as more than human. They are isolated from the rest of mankind and clothed with *robes de pédant*; it is as if they issued their pontifical proclamations under some supernatural inspiration. They owed, however, a great deal to their predecessors, even to those they opposed. Darwin was not only indebted to Lyell, who became his supporter, but also to progressionists, like Sedgwick, who were against the theory of evolution. In some stories, however, when Newton saw the apple falling, all was light; when Haüy dropped a crystal of calcspar, "a moment's hesitating inspection, and [under the excitement of an illuminating] suspicion... [which] became a conviction, the formative theory of *molécules intégrantes* was born." Lavoisier, " a pair of scales in his hand," chased away the shadows of phlogiston, and there was chemistry: "La chimie jaillit comme jadis Minèrve, toute équippé du cerveau d'un savant bien éminemment français, nommé Lavoisier." However, he was not a chemical Melchisedech, "without father, without mother, without descent;" he was possessed by the very ideas he fought against; his "caloric" is phlogiston in disguise; his theory of acids is essentially "qualitative;" his overwhelming authority is one of the causes of the chaos in chemical theory up to 1860.

Historiography of Science, then, has to de-canonize the great and to show that they were as human as we are, that they had their inconsistencies and their weak moments. Their greatness is in that they were not *wholly* children of their age; but nobody would have listened to them if they han *not at all* been children of their time. Historiography of Science has to be a *history* of man, instead of a mixture of a paleontology of amusing oddities plus a hagiography of supermen.

What method do we want then? An *objective* one. But objectivity is impossible! Without any doubt, it is impossible, as historiography is not a mere compilation of facts: the choice of material already implies an element of subjectivity and amounts to an evaluation. The fact that the historian of science is a scientist himself, influences his judgment on what is important or not. But in spite of this unavoidable influence of the historian's own political, educational, social, national, religious background and his personal character, we maintain the ideal of objectivity. Like all ideals it is unattainable, but, nevertheless, it should keep us in a holy dissatisfaction with ourselves.

As a Dutchman I may mention the names of three historians of science who probable would have made more impact internationally, if they had not belonged to a small nation: J. A. Vollgraff (editor of Huygens's works), C. de Waard (editor of Beeckman's *Diary* and Mersenne's *Correspondence*) and E. J. Dijksterhuis (known for his work on Archimedes, Euclid, Stevin and early mechanics). They made painstaking researches and they were up to the standards one might reasonably set to a "complete historian of science:" a wide scope of interest, great erudition, sufficient knowledge of languages, sound method. Therefore, it is rather arrogant when a beginning worker in this field writes a monograph under the title "*Towards* an historiography of science," saying that Koyré and his school almost alone "are truly advancing the study of the history of science."[12] Another author "sees science as the investigation of problems, rather than the discovery of facts or truths," and he hopes that along the lines he indicates "we will *at last* be able to think historically about the history of science."[13] Fortunately, history of science had not to wait for the 20th century in order to start thinking in a historical fashion: many years ago men like Cassirer, Meyerson, Duhem, Lasswitz, *etc.*, and not to forget the great Cambridge historian William Whewell, have shown us the way. Moreover, we should acknowledge that those who want a wide scope of history of science can only get it with the help of specialists in paleography and bibliography and thanks to those professional scientists who take to historical research in

[12] J. Agassi, *Towards an Historiography of Science*, s'Gravenhage, 1963, pp. V and 57.

[13] J. R. Ravetz, in: *Acta Historiae Rerum Naturalium*, Prague, 1967, special issue, No. 3, p. 64.

their leisure time. Their works may be in some respect unsatisfactory, but they performed the drudgery for us. But as soon as we get people trained as historians of science, there is the danger that we get perhaps clever and ingenious "explanations" of the development of science, but that inside knowledge will be lacking. Recently, the geologist V. A. Eyles raised a complaint about the "inadequate and cursory treatment the history of geology sometimes receives" at the hands of professional historians of science. Similarly, the decrease of humanistic training (knowledge of the classics, the Bible and theology) will make future historians of science more dependent on specialists from the other side.

To sum up: history of science provides material for a critical self--examination of science: it increases the appreciation of what we possess now, when we recognize the difficulties it cost to acquire it. It bridges the gap between science and the humanities, demonstrating how natural sciences are part of the humanism of our age. There will always be scientists who are not satisfied with knowing the contents of theories, but who want to know their *genesis* and who will find this an intellectual and aesthetical pleasure. For the reasoning and demonstrations of our predecessors sometimes are of an incomparable beauty, as *e.g.* Pascal's use of analogical reasoning in his two treatises on the equilibrium of liquids and on the weight of the air, or the alternating use of induction and deduction by which Haüy's magic evokes from the chaos of the phenomenal world of *crystals*, the cosmos of the ideal world of *crystallography.* Even a purely literary pleasure we will find there, *e.g.*, the elegant prose of Lavoisier's "Preface" to his *Traité de Chimie,* or Pascal's sarcastic *Traité du Vide*, or Kepler's lyrical outburst when he thinks to have discovered the plan of the world.

And, finally, history of science has a peculiar charm because of its inner tension: it is the history of disciplines which are progressing as human experience increases, whereas, on the other hand, it is the history of sciences constructed by the human mind, which in the course of written history stuck to similar patterns. So it reveals that in Science, too, we see farther than our ancestors not because we are greater than they, but because we are standing on their shoulders.