Ravetz, Jerome R.

[We can all agree...]

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intéresse tout particulièrement les professeurs d'histoire des sciences, et surtout les auteurs de traités.

En fait, il me semble qu'aucun des systèmes de périodisation qui ont été employés jusqu'au présent n'est pleinement satisfaisant et que l'utilisateur doit choisir la solution la moins mauvaise, compte-tenu du but qu'il se propose. Les coupures à introduire sont de deux types, l'un interne, lié au développement même de la science, l'autre externe, lié à des facteurs extérieurs agissant sur le progrès scientifique. Une découverte, une innovation méthodologique, une synthèse originale peuvent correspondre à une coupure du premier type.

Mais il ne faut pas oublier que la date d'une découverte ou d'une création est difficile à fixer et qu'il faut distinguer entre sa préparation, son éclosion, sa publication, sa diffusion et son adoption définitive. La découverte des rayons X, l'enseignement de la géométrie descriptive par Monge, les expériences de Laue sur la diffraction des rayons X par les cristaux marquent des coupures importantes et relativement bien précisées; par contre l'introduction de la gravitation universelle, de la théorie des groupes, de la théorie des ensembles, de la génétique, des quanta, etc. sont des coupures très importantes, mais qui s'étalent sur une longue période.

La Révolution française, sur le plan mondial, par les changements, qu'elle a introduits dans les domaines de l'enseignement et de la recherche scientifique, la Révolution russe sur le plan national, sont des exemples de coupures externes aussi bien précisées. Par contre, si le concept de Renaissance est utile et commode, les limites chronologiques de cette période sont difficiles à fixer.

En conclusion, je pense que la périodisation en histoire des sciences doit utiliser ces deux types de critères, mais en insistant sur le fait que les coupures introduites sont en partie artificielles, qu'elles s'étendent souvent sur une assez longue période et varient suivant les branches de sciences considérées. Les coupures adoptées pour l'ensemble des sciences, comme celles que j'ai introduites dans mon *Histoire générale des sciences*, ne doivent donc être considérées que comme des points de repère moyens, dont la commodité et l'utilité ne doivent pas cacher l'imprécision et le caractère très relatif.

J. R. Ravetz

We can all agree that periodization must not be a dogma, but rather a guide to action. How is it to be used? First we have the deep philosophical problem of describing complex qualitative changes. I think we

do not have the conceptual tools to do this properly, and from this follow many of our difficulties. It seems to me also that there is a difference between many of us and our Marxist colleagues. We are surely concerned with the same basic problem: understanding the complex causes responsible for the progress of science. But we seem to pursue this in different ways.

As Professor Taton says: *"Je suis plus empiriste"*, I do not try to identify "general laws" of change and study their interaction; rather I try to illuminate the complex fabric of development where I can, using certain general principles as tools for this work. Professor Price has helped me to see the difference between this approach and that of our Marxist colleagues. Let us understand this difference clearly, or else we may find ourselves having no communication.

I shall give an example of my "empirical" approach to this problem, but first may I suggest a refined terminology for dividing between periods in the history of science. Instead of trying to fix the starting point of a new development, we may speak of *"anticipation"*, *"penetration"* and *"dominance"*.

My example may be called "Social Aspects of Modern European Natural Science". I shall consider the natural periodization of different aspects of this activity, and see what is revealed by the intersection of the different intervals.

First let us consider science as a basic means of production. Here we find *"anticipation"* in Bacon, and in the early Royal Society. *"Penetration"* begins slowly, becoming effective on a mass scale only towards the end of the XIXth century. *"Dominance"* dates from 1945. Corresponding to this aspect, we have the position of the community of science. Up to the last war it was on the margin of society, away from the centres of power. Since then, with science as the basic means of production, the community of science has been at the centre.

But the prehistory of science does not end in 1945; let us consider the aspect of "ideology". I take, for simplicity, the attitude that the study of inanimate nature is the key to truth. For this, *"anticipations"* are in Bacon, and continuous traditions start with the schools of Descartes and Galileo.

For the organization of the community of science, the XVIIth century "anticipations" are realized only at the end of the XVIIIth century, with the foundation of the École Polytechnique.

Considering all these aspects together on a time-scale, we notice an interesting feature of the period between the French Revolution and the Second World War. Then, the community of science existed, self-contained, on the margin of society, with an effective ideology. Perhaps out of such a social situation came the attitude of "pure science" which has conditioned teaching and evaluation of science until so very recently.

The above sketch is dangerously oversimplified. Not only are my categories crude, but I have ignored national differences of tradition and other approaches to the understanding of nature. In particular, the "hylozoistic" tradition of the Renaissance, reappearing as German *"Naturphilosophie"*, whose significance has been seriously underestimated.

A. P. Youchkevitch

Professor Olszewski correctly raises a series of problems concerning the periodization of the history of science. The attempt to periodize the general history of natural and mathematical sciences, as suggested by Professor Kedrov, deserves much attention. Many principles recommended by him are correct. The concrete characteristic of particular periods, however, gives rise to objections.

This applies, first of all, to the period A, defined by Professor Kedrov as a natural-philosophic period — when the rudiments of nature's knowledge are part of a single, undifferentiated philosophical science. Kedrov extrapolates the peculiarities of the brief developmental epoch of Greek thought over the whole of the Antiquity. Was, however, the astronomy of Babylonians natural-philosophic? Was the entire mathematics of Greeks, their statics and hydrostatics, their astronomy and so forth a part of philosophy and was the natural-philosophic method of thinking really peculiar to them? But the mentioned sciences did play a leading role in the Greco-Roman world. If I am allowed to delineate this period A with two or three words, I should say that it was a period of formation of natural and mathematical sciences in the true sense of this word.

The period *a* was — according to Professor Kedrov — a scholastic one, in which the false sciences astrology, magic, alchemy and others were prevailing. It is true that this was the period of scholasticism dominating in philosophy and that scholasticism exerted influence on science. On the other hand, there developed mathematics, astronomy, mechanics (Oxford and Paris) and son on. The development of cognition was not so simple and so one-sided a process. There is, besides, no reason to oppose — as Professor Kedrov does — Europe to Mussulman countries. The philosophic and theological scholasticism, hampering the progress of science, and the struggle of the progressive scientific thought against the former existed both in Christian and Moslem countries.

At last, not quite fortunate was the denomination of the period B as empirical and collecting. This may be attributed to the cycle of biological sciences, but not to science as a whole. In that period, the leading role was being already played by the complex of mechanical--mathematical and in part physical, that is theoretical sciences.