

Scientific Discovery and the End-of-History Fallacy

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Strong conceptions of scientific discovery (in the broad sense of creative work at the frontiers of research) are linked to strong conceptions of historical change. Contrariwise, impoverished conceptions of both scientific discovery and the history of science yield conservative accounts of scientific work. One form of historical impoverishment, of which even strong historicists can be guilty, is a truncated conception of history that fails to include future history. The difficulty, nay impossibility, of concretely visualizing future historical change leads even sophisticated thinkers to commit what I term "the end-of-history fallacy," analogous to the mistake made by deeply historical thinkers such as Hegel, Marx, and Fukuyama. Traditional history of science made us realize that the development of the sciences until now has been a highly dynamic enterprise. But traditional history ends at the present, and we need means to make the unrealized future come more alive for its creative, hence dynamical possibilities. A better appreciation for the nature of creative work at the frontiers of research – a better understanding of what we might call frontier epistemology – suggests that even the supposedly mature sciences may experience a long-term, highly dynamic future. Such a view has implications for the scientific realism debate as well as for science policy and the public understanding of science.

Section 1 of the paper is plea to take history of science seriously once again, indeed, even more seriously than in the 1960s and '70s, when 'history of science' usually meant 'the past of science' rather than considering that past as only the possibly raw beginnings of time series of developments that may last for many millennia beyond the present. Although it sounds oxymoronic, I shall include the history of the future as well. Following original work on the history of mechanics by Koyre', Butterfield famously contended that each modern science began with a founding revolution. In *The Structure of Scientific Revolutions* Kuhn went further to argue that there have been later revolutions as well, re-foundings in a sense, and that in the mature, hard sciences, later revolutions without end are almost inevitable. (Others have since pointed out other kinds of transformative spurts than the Kuhnian variety.) Kuhn is one of the few analysts to project such a dramatic future dynamic of science. Given the expansion of scientific domains, the tightening of linkages, and the nonlinearity of the internal dynamics of science (that even a seemingly normal result can eventuate in a transformation), future Kuhnian revolutions might even become larger rather than smaller.

Section 2 distinguishes several different concepts of mature science and points out crucial tensions between retrospective and prospective accounts of maturity. By contrast with Kuhn some strong realists hold that mature sciences are both highly creative yet not likely to undergo significant transformation, or at most a series of ever-smaller ones that converge on the truth. To argue, as some realists do, that today's sophistication can easily handle the research problems of past frontiers, overlooks the fact that living sciences constantly generate new frontiers that are at least as difficult as the old ones. At these frontiers the big questions usually involve decision-making under extreme uncertainty rather than merely under risk.

Section 3 relaxes the assumption that a significant future dynamic must be revolutionary. Kuhnian revolutions and other sorts of rapid spurts are not necessary to imagine that future mature science may well transform itself almost beyond recognition. After all, given enough time, gradual evolution can achieve transformations as radical as you please. Further, as in the case of biological evolution, it is arguable that, over a plausible range of conditions, the future evolution of science is inevitable – and will

be much faster. The usual historical-cultural time-scale begins to look rather arbitrary (even presentist in a sense) when we consider the future as extending out to, say, 40,000 years of creative scientific research, as compared with the 400 years since the beginning of the Scientific Revolution. On the strong realist view (which also cannot be proven wrong), these first few centuries will, centuries hence, be known as The Age of Scientific Discovery, a project essentially completed. Section 4 briefly sums up my "deep history" and "deep discovery" positions in terms of a set of interpretations of Mary Hesse's "principle of no historical privilege" and some reminders about changing human interests, goals, and human creativity.

In Section 5 I claim that many analysts, including philosophers of science, commit an "end-of-history fallacy," deriving from the difficulty, nay impossibility, of envisioning a distant future of science. The fallacy often involves a cluster of questionable assumptions, including a conflation of different senses of 'mature science' and an insufficiently prospective analysis deriving from our limited horizons of imagination. Insofar as maturity implies that the main period of discovery is over, it would seem that maturity claims announce the end of the History of science ('History' meaning the universal sense of 'history'). The fallacy is committed by people who assume, without adequate argument, that the future will be relatively "flat," i.e., not dynamically interesting, not highly nonlinear, that the future expansion of mature science will consist mostly of routine specialization and "translational" work, a sort of normal science "flatline." Often this assumption is a default assumption that remains implicit, by an author's simply failing to consider seriously the possibility that the future may be interestingly creative and dynamic.

Section 6 briefly rejects basic objections to the above, namely, that I am a global antirealist whose use of the end-of-history fallacy marks me as a global skeptic in matters scientific and that my own views on heuristic appraisal undermines my position. Heuristic appraisal is evaluation of the future fertility of anything, and sometimes can legitimately judge a given specialty area of be essential finished and hence sterile of further significant discoveries. Hence the objection.

Section 7 concludes the paper by briefly pointing out some implications for public understanding of science and for policy, including the way granting agencies are run. Highly optimistic philosophies of science claiming that mature science has nearly reached its ultimate goal (whether strongly realist or not) can discourage investment in long-term, potentially transformative projects. End-of-history fallacies may contribute to the conservative granting policies that currently plague institutions such as the U.S. National Science Foundation. The overall message of the paper is that, despite our limited horizons regarding the future, we philosophers must be more prospective in our thinking.