ABSTRACTS

B5.4 Historical Aspects in the Philosophy of Science

The reception of Ludwik Fleck's theory of thought styles and thought collectives in English.

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The story of H. Reichenbach’s footnote (1938) and the fact that T.S. Kuhn mentioned Fleck’s German book (“Entstehung ung Entwicklung einer wissenschaftliche Tatsache”) in the foreword to “The Structure of Scientific Revolutions” (1962) is quite well known. What happened after the publication of American translation of Fleck’s book (1979) hasn’t yet been described. And the bibliography of the reception of Ludwik Fleck’s theory in English consists of over 300 entries. Surprisingly only around 30% of authors who write on Fleck in English come from English speaking countries (around 10% - Poland, around 25% - German speaking countries, around 35% - others). Although more or less a half of Fleck’s theoretical legacy is written in Polish, those who come from English speaking countries and write on Fleck in English in 70% cite only the American translation of Fleck's German book (and English translations of Fleck’s Polish papers are available). The question why Fleck’s theory of thought styles and thought collectives was recognized so late was raised by many authors. I would like to raise the question how it is recognized nowadays in English language with special attention to the problems of translation and trace few examples of Fleck's original expressions, it’s English translation and the influence of these translations on the reception in English.

Reinvigorating Hanson’s patterns of discovery

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Matthew D. Lund (2010) has recently written a book on N. R. Hanson’s history and philosophy of science. Hanson is often treated as an important precursor of many important ideas in the late 20th century philosophy of science; like the theory-ladenness of observation, discussions on logic of discovery, or a close relationship between history and philosophy of science. Still, as Lund points out, Hanson’s more elaborate reading has not received philosophical attention it deserves. One way of seeing Hanson has been through the Kuhnian framework, that is, as a basis of what Kuhn developed further. This has left the analysis of Hanson’s ideas incomplete.

In my presentation I analyze critically Lund’s interpretation on Hanson's philosophy. One central notion is that of intelligibility. Scientists are creating new "patterns" while struggling to make sense of the object of their research. As Lund points out both Hanson and Kuhn were modeling the creation of new
conceptual frameworks. But unlike Kuhn, Hanson thought that this creation is a rationally appraisable activity.

I maintain that in his many ways excellent treatment, Lund is missing one central aspect of Hanson’s philosophy. Lund discusses abductive inference only in passing, and he is not seeing the meaning and the potential of abduction for making sense of intelligibility. Hanson’s ideas on abduction as a logic of discovery can be defended and developed further. Methodology still often emphasizes the testing of hypotheses and test implications while abduction gives means of reasoning “backwards”, from consequences to hypothetical causes. This bias can be seen in Lund’s analysis also. There are also interesting parallels between abduction and Kuhn’s description of paradigm shifts which gives means of analyzing Kuhn’s implicit “logic of discovery”.


Lakatos, Rational Reconstruction and Comparative Historiography

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In this talk my aim is to show that (1) an aspect of Imre Lakatos’s philosophy has been largely ignored in previous discussions and that (2) this omitted aspect has great potential to contribute to the philosophy of the historiography of science. More specifically, it may provide an answer to the question of whether and how historiographical data can be used to support and compare different ‘philosophies of science’. In other words, I will outline a valuable core of Lakatos’s philosophy of historiography and then update it to meet the requirements of the contemporary history and philosophy of science.

The plan is as follows. First I explain the positive features of Lakatos’s philosophy of historiography, which are: (i) highlighting of hierarchies of historical interpretation, (ii) non-realist and (iii) comparative historiography of science using an epistemic value (of rationality) for comparisons. In the second section of this essay I discuss potential problems in Lakatos. They are: (i) Lakatos’s reference to ‘actual histories’ that seemingly contradicts non-realism, (ii) utopianism due to exaggerating the rationality of history and (iii) distortion of the history of science because of Lakatos’s normative ambitions. The last part is devoted for updating Lakatos’s programme to answer the needs of contemporary history and philosophy of science. First, (i) it is necessary to bring new ‘methodologies’ or ‘philosophies of science’ into consideration, such as scientific realism and Latourian actor-network analysis. (ii) Another issue that needs updating is criteria to be used in comparative evaluation. I consider what other values beside rationality could be used in comparisons. Finally, (iii) I will mention briefly an example of how Lakatosian comparative historiography of science works in its updated mode.
Thomas Kuhn and the rationality of theory choice.

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In this communication, I try to articulate and clarify the role of the epistemic authority of experts in Kuhn’s explanation for the transition process between rival paradigms in the scientific revolutionary period. If science progresses, that process should contribute to the attainment of the cognitive aim of science, namely, the articulation of paradigms increasingly successful at the resolution of problems. In virtue of the semantic and methodological incommensurability between rival paradigms, it is not easy to sustain that the winner paradigm is superior in relation to the aim of science. Furthermore, according to Kuhn, scientists choose one paradigm instead of another based on subjective reasons. If the debate between rival paradigms ends that way, then it seems that science moves irrationally. Against this conclusion, we could say that the individual choice can be irrational if it doesn't affect the epistemic rationality of the process of changing from one paradigm to another. Given that a paradigm needs supporters in order to be developed, it is good that some scientists give support to a new paradigm based on subjective reasons. Otherwise, scientific revolutions would never happen. Nevertheless, even when we have two well developed incommensurable paradigms, the scientist’s comparative judgment that one paradigm is better than the other would not be based on common evidence, according to Kuhn. So, it seems that the threat of irrationality comes back. It is hard to see this process as rational and attaining the cognitive aim of science. In order to avoid this conclusion, I will argue that we should appreciate the kind of epistemic authority that is granted to the scientist by our society in the revolutionary period. The mistake of Kuhn was to emphasize and clarify insufficiently the role of the epistemic authority of experts; his critics failed for ignoring it altogether.