INTRODUCTORY NOTE

In this short introduction, I shall comment on the philosophical interest of present-day discussions on and results in scientific discovery. Some readers who are not familiar with contemporary philosophy of science might wonder whether something sensible may be said about discovery. For them, I should first spell out some historical facts and some recent findings.

Exactly ten years ago appeared the two volumes on scientific discovery,[1] edited by Tom Nickles. From those days on, increasing numbers of philosophers of science became convinced that theory formation is just as rational a matter as scientific justification, and that the dichotomy itself is in many respects a mistake. As a consequence, many people tend to consider a philosophical analysis of discovery as necessary for the understanding of science as a rational process.

The position defended in earlier days is well-known. According to the received view, discovery is opposed to justification, and whereas the latter is a central problem for epistemology, the former belongs to the realm of psychology. This view itself was adopted only from the nineteenth century on. It replaced the seventeenth century conviction that the method of science is induction, or rather, as some prefer to call it, the old inductive method. It was only after the actual evolution of science forced scientists to give up the old inductive method - Larry Laudan[2] wrote an excellent and instructive study of this matter - that the dichotomy itself became popular and that discovery was characterized as a non-rational process. The latter conception was well in agreement with the romantic view according to which creativity in general is a matter of genius and inspiration, and is the extreme opposite of the 'dull' activities that fall within the scope of rationality and the application of rules.

The 'friends of discovery' among the contributors to the aforementioned Nickles volumes, did not suddenly arise out of the void in 1980. In the sixties and seventies, problem solving had extensively been studied in psychology and artificial intelligence, most eminently, e.g., by Herbert Simon and his group at CMU. This study included discovery problems. Among the most important results were those on *heuristic rules*. The enterprise was not as such opposed to the received view, which actually claimed that the study of discovery belongs to psychology (and

presumably other human sciences as well). But, as Herbert Simon put it in a more recent paper, it became apparent "that there are better and worse ways of problem solving; hence that it is possible to construct normative theories of problem solving and of scientific discovery."[3] In this way the road was prepared for philosophers of science to change their minds. The Nickles volumes contained at once (i) a careful dissection and refutation, mainly by Tom Nickles himself, of the traditional arguments for the non-rational character of discovery, (ii) a diversity of rational approaches to discovery, some of which are highly effective and promising, (iii) a large amount of historical material analyzed in view of the discovery problem. No doubt, this multiplicity of results and promises, combined with the outstanding quality of most of them, is largely responsible for the effect of the volumes on the philosophical community.

The present-day view on theory formation neither constitutes a revival of the old inductive method nor does it entail subscribing to anything similar to that method. No one claims that there is a *logic* (in the narrow sense) of discovery, or a *single* logic (in the wide sense) of discovery. This should not cause any astonishment in view of the fact that even the traditional views on scientific *justification*, whether Popperian or inductivist, are hopelessly simplistic in comparison to their contemporary replacements. For one thing, there is general agreement that a multiplicity of criteria play a role in the appraising of theories and research traditions, and few philosophers of science will claim nowadays that such appraisal *always* allows for a conclusive choice in favour of one theory out of the alternatives. Given this outlook on theory appraisal, it is easy to understand why few scholars actually require that a rational methodology of discovery reduce to some simple or deterministic set of rules. Incidentally, it has gradually become evident, during the last hundred years or so, that both nature and our thinking about it are not adequately described by the simple and deterministic theories of the past. At the same time we have developed instruments that enable us to get a good grasp on complex, instable and indeterministic situations - think about Prigogine's theory of dissipative structures and about chaos theories. The new view on discovery fits into this general development: generative processes cannot be described by simple and deterministic general rules, but we have learned to handle heuristic rules, reasoning from inconsistent premises, and similar devices.

The topic of the present volume is also important from a meta-philosophical point of view. It is quite obvious that the

contemporary outlook on discovery is indebted to results from non-philosophical disciplines. Moreover, both the development of this outlook and the arguments in its favour heavily depend on empirical matters. In other words, the philosophical debate on discovery is exemplary for the increasing success of inter-disciplinary and empirical approaches in philosophy.

There is a further reason why scientific discovery is philosophically important. It is indeed a specific case of a more general phenomenon, viz. creativity. For all its prejudices, romanticism was right in assigning great importance to creativity and in seeing it as a distinctive feature of human beings. There is no reason to deny that our insights on scientific discovery will be helpful to tackle the general problem of creativity. The generation of concepts and hypotheses in science is in many respects similar to, for example, the production of works of art - information about one such similarity may be gained from Kostas Gavroglu's contribution to the present volume. Artificial intelligence offers some results on the production of several forms of art, but, in spite of the extensive literature in aesthetics, philosophers have clarified next to nothing on artistic creativity. The contributions to the present volume, though written by highly specialized authors, are very readable for the general philosophical public. It is my hope that, apart from being informative, they might be thought provoking as well.

The papers in this volume offer a nice though obviously incomplete survey of the different approaches to and aspects of creativity in science. In the first paper, Thomas Nickles, one of the most fervent and most productive present-day defenders of discovery, offers an articulation and defence of a global philosophical theory. Central to his position are the rejection of a neutral or global logic of discovery, the idea of discoverability logics, the import of our factual and theoretical results (and of their organization), and the fact that science produces not only such results, but also problem-solving *methods*. In spelling out his position, he criticizes current views on discovery and shows that arguments which apparently are opposed to logics of discovery may actually be turned in their favour.

Conceptual change is the theme of Nancy Nersessian's contribution. Comparing the weaknesses of the two major approaches to the problem, she opts for a third, cognitive-historical method. The latter is illustrated through an examination of the roles of imagistic and analogical reasoning in the creation of the concept of electromagnetic field. Nersessian argues that this imagistic reasoning is actually a form of analogical reasoning, and that the cognitive-historical approach provides an explanation for the

fact that conceptual change in science is continuous but not simply cumulative.

Starting from Hacking's proposals, Kostas Gavroglu studies the role of 'style' by comparing the work in low temperature physics performed at two different laboratories. He reviews central features of the liquefaction of helium as attempted by Dewar and as accomplished by Kamerlingh Onnes. The law of corresponding states (Van der Waals) was essential to Onnes's success. Gavroglu argues that, though Dewar knew this law, it lacked the required affinity to his type of discourse. He does not conclude, however, that some styles are superior to others; the heuristic efficiency of style is, again, local, not global.

Criteria for promise should not be totally unrelated to criteria for success and efforts at justification are no less directed to discoveries than are efforts at conceptualization. With these as a motivation, Scott Kleiner focuses on traditional scientific methods *as* heuristics, i.e. as procedures with limited applicability. From this point of view, he consecutively discusses at length hypothetico-deductive as well as retroductive methods, and compares their merits to inductive and analogical methods.

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1. Thomas Nickles (ed.), *Scientific Discovery, Logic, and Rationality,* and *Scientific Discovery: Case Studies,* Dordrecht, Reidel, 1980.
2. Larry Laudan: "Why was the logic of discovery abandoned?" in *Scientific Discovery, Logic and Rationality,* o. c.; a revised version appeared as chapter 11 of Laudan's *Science and Hypothesis,* Dordrecht, Reidel, 1981. For a criticism of the general conclusion argued for by Laudan, see the paper by Tom Nickles in this volume.
3. Herbert Simon, "Is scientific discovery a topic in the philosophy of science?" in Nicholas Rescher (ed.), *Scientific Inquiry in Philosophical Perspective,* University Press of America, 1987, pp. 1-15. The quotation is from p. 14.