CHAPTER 5.2

SCIENTIFIC DISCOVERY AND THE PSYCHOLOGY OF PROBLEM SOLVING† *

The very fact that the totality of our sense experiences is such that by means of thinking (operations with concepts, and the creation and use of definite functional relations between them, and the coordination of sense experiences to these concepts) it can be put in order, this fact is one which leaves us in awe, but which we shall never understand. One may say “the eternal mystery of the world is its comprehensibility.” It is one of the great realizations of Immanuel Kant that the setting up of a real external world would be senseless without this comprehensibility.

Albert Einstein, Out of My Later Years

In the previous chapter a theory of human problem solving was put forward with references to some of the evidence for its validity. The theory has been formalized and tested by incorporating it in programs for digital computers and studying the behavior of these programs when they are confronted with problem-solving tasks.

The thesis of the present chapter is that scientific discovery is a form of problem solving, and that the processes whereby science is carried on can be explained in the terms that have been used to explain the processes of problem solving. In particular, I shall undertake to show how the theory of problem solving described in the previous chapter can account for some of the principal reported phenomena of scientific discovery.

For a description of these phenomena, the analysis will draw heavily upon previous published accounts. Discussions of scientific discovery have always been highly anecdotal, most of our specific information on the subject deriving from reports of specific examples, recorded in some instances by historians and philosophers of science, in some instances by psychologists, but often by the discoverers themselves. The classics in the latter category are Henri Poincaré’s celebrated lecture, translated as ‘Mathematical Creation’ (New York: The Science Press, 1913), and the delightful essay by Jacques


H. A. Simon, Models of Discovery

The literature on the topic produced by philosophers of science is substantial, but has been for purposes of this analysis, on the whole, less useful. (I will mention two important exceptions in a moment.) The reason is that philosophers of science tend to address themselves to the normative more than to the descriptive aspects of scientific methodology. They are more concerned with how scientists *ought to* proceed, in order to conform with certain conceptions of logic, than with how they *do* proceed. Notions of how they ought to proceed focus primarily on the problem of induction: on how generalizations might validly arise from data on particulars and on the degree to which a corpus of data logically confirms a generalization. These are interesting questions of philosophy, but they turn out to have relatively little relation to the actual behavior of scientists—and perhaps less normative value than has been supposed.

In the past few years, two philosopher-historians of science, both originally trained in physics, have made particularly significant contributions to the psychology and sociology of scientific discovery. Both have been quite explicit in distinguishing the processes of discovery from the traditional canons of 'sound' scientific method. I shall make considerable use of their work and ideas. One of these men, Norwood Russell Hanson, has set forth his views most extensively in *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958). The other, Thomas S. Kuhn, has produced an original and stimulating account of *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

To explain scientific discovery is to describe a set of processes that is sufficient—and *just* sufficient—to account for the amounts and directions of scientific progress that have actually occurred. For a variety of reasons, perhaps best understood by psychoanalysis, when we talk or write about scientific discovery, we tend to dwell lovingly on the great names and the great events—Galileo and uniform acceleration, Newton and universal gravitation, Einstein and relativity, and so on.\(^1\) We insist that a theory of discovery postulate processes sufficiently powerful to produce these events. It is right to so insist, but we must not forget how rare such events are, and we must not postulate processes so powerful that they predict a discovery of first magnitude as a daily matter.