

SCIENTIFIC DISCOVERY AND MUSICAL CREATIVITY  
WITH IMPLICATIONS FOR THE CONCEPTUAL  
FOUNDATIONS OF SYSTEMATIC INQUIRY

by

Paul Chi Lung Tang

B.Sc., University of British Columbia, 1966

A.R.C.T., Diploma of Associateship,

Royal Conservatory of Music, Toronto, 1962

A THESIS SUBMITTED IN PARTIAL FULFILMENT OF  
THE REQUIREMENTS FOR THE DEGREE OF  
MASTER OF ARTS (EDUCATION)  
in the Department  
of  
Social and Philosophical Foundations

© PAUL CHI LUNG TANG 1971

SIMON FRASER UNIVERSITY

August, 1971

APPROVAL

Name: Paul Chi Lung Tang

Degree: Master of Arts (Education)

Title of Thesis: *Scientific Discovery and Musical Creativity  
with Implications for the Conceptual Foundations  
of Systematic Inquiry*

Examining Committee:

---

Gordon R. Eastwood, Ph.D. (Minnesota)  
Senior Supervisor

---

Cornel M. Hamm, Ph.D. (London)  
Assistant Professor

---

R. Murray Schafer, L.R.S.M. (London)  
Professor

---

John Walkley, Ph.D. (Liverpool)  
External Examiner  
Professor of Chemistry,  
Simon Fraser University

Date Approved: Aug. 16, 1971.

## ABSTRACT

The thesis is concerned with an investigation of certain aspects of the conceptual foundations of scientific methodology as applied to research in the natural sciences, the social sciences, and education. However, it is essential that this general statement be supplemented by the Preface (page viii).

A general historical survey from ancient times to the modern is first undertaken, and in Chapter Two the enterprise is described in some detail, with particular reference to Popper's criterion of falsification. The discussion then turns to a philosophical analysis of selected conceptual problems, focusing specifically on the problem of discovery. The Kinetic-Molecular Theory of Gases is taken as a representative scientific theory, and an outline is appended in order to draw together the several ideas of the preceding sections (Appendix F).

The problem of scientific discovery, introduced in Chapter Two, becomes the subject of intensive examination in Chapter Three. Two approaches are employed. On an empirical level, some psychological studies are cited, as well as quotations from scientists themselves. No attempt is made to present empirical evidence in detail; its use is necessary insofar as it is recognized that an explanation of discovery cannot be established *a priori*. The remaining part of the chapter is devoted to a philosophical analysis (primary approach), reviewing first the traditional problem of the existence, or not, of a logic of discovery.

The existence of such is refuted, and alternative notions are suggested, of which the concept of model comes closest to being a deductive inference technique for the formulation of fruitful hypotheses. However, it is shown that any attempt to treat models as the logic of discovery still gives rise to difficulties. The chapter concludes with an exposition of Blackwell's recent theory of discovery. Upon analysis, the non-logical, creative aspect of discovery is again established.

Chapter Four is an examination of musical creativity in order to draw out additional features of the "creative" discovery process introduced in Chapter Three. Again, empirical and philosophical approaches are employed with emphasis on the latter. The creative-subjective phase is described in the same terms as for discovery, and the thrust of the argument then shifts to an attempt to make sense of the notion of an "objective phase." This is done through an elucidation of the concepts of music theory and testing. In addition, concepts such as explanation, prediction, and conceptual frameworks, previously discussed in Chapter Two, are shown also to pertain. Thus, the claim of a relationship between scientific discovery and musical creativity can be extended beyond the initial subjective stage. The chapter concludes with an exegesis of Richard Martin's recent attempt to expose the underlying logic of a fundamental theory of musical structure. An analogy is drawn between this theory and corresponding theories in science, such as the Atomic Theory (structure of matter) and the Cell Theory (structure of living systems). In so doing, the concept of "objective phase" is further supported.

In Chapter Five, a model is developed to relate scientific discovery and musical creativity. This is analyzed from a strictly formal point of view, but it is important that the logical formulae be interpreted in terms of the arguments of the thesis. Furthermore, the model is simplified, and revised models, representing more completely the two processes concerned, are appended (Appendix L). The thesis ends with the presentation of some conclusions and implications for the conceptual foundations of systematic inquiry.

## TABLE OF CONTENTS

Chapter	Page
1. THE DEVELOPMENT OF SCIENTIFIC INQUIRY-- AN HISTORICAL PERSPECTIVE . . . . .	1
Section I: Historical survey of the natural sciences . . . . .	1
Section II: Historical survey of the social sciences . . . . .	12
Section III: The scientific study of education . . . .	15
Summary . . . . .	20
2. THE STRUCTURE OF SCIENTIFIC RESEARCH . . . . .	24
Section I: An overview of scientific research . . . .	24
Section II: Popper's modification of the hypothetico-deductive system . . . . .	36
Section III: A detailed treatment of some key concepts of scientific research . . . . .	43
Summary . . . . .	86
3. SCIENTIFIC DISCOVERY . . . . .	90
Section I: Introduction . . . . .	90
Section II: Psychological studies . . . . .	91
Section III: Quotations from scientists . . . . .	94
Section IV: The problem of a logic of discovery . . . .	104
Section V: Alternative notions of a logic of discovery . . . . .	121
Section VI: Exegesis and analysis of Blackwell's theory of discovery . . . . .	142
Summary . . . . .	175

Chapter	Page
4. MUSICAL CREATIVITY . . . . .	182
Section I:    Introduction . . . . .	182
Section II:   Psychological studies . . . . .	183
Section III:  Quotations from musicians . . . . .	185
Section IV:   The notion of "objective phase" . . . . .	194
Section V:    Exegesis of Martin's proto-theory of musical structure . . . . .	212
Summary . . . . .	221
5. RELATIONSHIPS, CONCLUSIONS AND IMPLICATIONS . . . . .	224
Section I:    A model relating scientific discovery and musical creativity . . . . .	224
Section II:   Conclusions and implications . . . . .	231
BIBLIOGRAPHY . . . . .	243
APPENDICES . . . . .	250
A. EINSTEIN'S THEORY AND THE BRANS-DICKE THEORY . . . . .	252
B. ANALYTIC STATEMENTS AND THEORY CONSTRUCTION . . . . .	254
C. THE ONTOLOGICAL STATUS OF THE ELECTRON . . . . .	257
D. CONCEPTS AND CATEGORIES . . . . .	259
E. TELEOLOGICAL STATEMENTS IN BIOLOGY . . . . .	263
F. KINETIC-MOLECULAR THEORY: AN OUTLINE . . . . .	265
G. ALTERNATIVE MODELS AS ALTERNATIVE EXPLANATIONS . . . . .	273
H. FURTHER POINTS ON ONTOLOGICAL STATUS AND WHITEHEAD'S FALLACY OF MISPLACED CONCRETENESS . . . . .	277
I. THE LAWS OF HARMONIC PROGRESSION . . . . .	281
J. FUGUAL FORM . . . . .	284

Appendix	Page
K. SONATA FIRST-MOVEMENT FORM . . . . .	287
L. EXPANDED MODELS OF SCIENTIFIC DISCOVERY AND MUSICAL CREATIVITY . . . . .	290



## PREFACE

The thesis aims at comprehensiveness. It is addressed to philosophers, scientists, musicians, and educators alike. Because of this comprehensiveness certain sections may be omitted on a first reading, or, perhaps, omitted altogether. For example, Chapter Two, Sections II and III, contain fairly detailed discussions of Popper, and of topics peculiar to the philosophy of science. It is not assumed that this material is of interest to all groups to which the thesis is addressed, and, further, this is related to the fact that the thesis contains two arguments. These are listed below, together with the relevant portions:

Primary argument: establishing a relationship between scientific discovery and musical creativity.

Chapter Two:	Section I
Chapter Three:	All sections
Chapter Four:	All sections
Chapter Five:	Section I

Secondary argument: drawing implications from the Primary argument for the conceptual foundations of inquiry.

All of the material for the Primary argument, and, in addition:

Chapter Two:	Section II
Chapter Five:	Section II

Chapters are divided into sections. At the beginning of each section there is a summary of the material contained therein.

At this time I should like to acknowledge the assistance of many who, in various and several ways, have contributed. Space does not

allow me to list them all, but I should especially like to acknowledge the assistance of:

- (1) Professors Gordon R. Eastwood and Cornel M. Hamm, for guiding me into the exciting realms of philosophy;
- (2) Professor R. Murray Schafer, for his help especially in the preparation of Chapter Four;
- (3) Professor Dennis H. Chitty, D.Phil.(Oxon.), F.R.S.C., Professor of Zoology and of the Principles and History of Biology, University of British Columbia, who first interested me in the philosophical foundations of science;
- (4) my two faithful typists, without whose patience this thesis would never have been completed.

Inadequate though words may be, to each and all I tender this small token of deepest appreciation.

SCIENTIFIC DISCOVERY AND MUSICAL CREATIVITY  
WITH IMPLICATIONS FOR THE CONCEPTUAL  
FOUNDATIONS OF SYSTEMATIC INQUIRY

## CHAPTER ONE

### THE DEVELOPMENT OF SCIENTIFIC INQUIRY--

#### AN HISTORICAL PERSPECTIVE

##### SECTION I

*Science as a rational, intellectual, and cultural activity, is characterized by its method. The roots of western science are also grounded in heritages other than the Greek. A brief survey of ancient science: Egypt, Mesopotamia, India, China, Ancient Greek science, Medieval science. The Renaissance and early modern science. The 17th Century and the rise of the modern scientific outlook. Determinism. Modern natural science.*

If one were to identify the single, most influential factor that has pervaded the life style of western industrialized societies, especially during the last three centuries, that factor would probably be the impact of science and technology. Today, evaluation of this enterprise can be undertaken, and this can be enhanced by an understanding of, and an appreciation for, both the history and philosophy of science. This thesis is concerned with such an investigation, and is limited to a brief historical survey of what may be termed "the scientific tradition," after which the focus will shift to a specific problem central to the philosophy of science, viz., the problem of "discovery." However, before beginning the historical analysis, the main features of the scientific enterprise will be summarized.

Scientific knowledge is extremely diverse, and encompasses phenomena ranging from subatomic reactions to mental processes; from mathematical laws to thermodynamics; from the economics of educational financing to the laws of supply and demand; from the birth and death of stars to the migration of birds; from the study of extragalactic nebulae to the study of ultra-microscopic viruses; from the structure and function of human bodies to laws of thought and to investigations of student creativity. The question then arises, "What, if anything, is common to this diversity?" Formal sciences apart (i.e., logic and mathematics), the most distinguishing feature of empirical science is the striving for systematic and unbiased observation to satisfy the quest for explanation and prediction. Other features, which may be present to varying degrees, include systems of classification, from which generalizations or laws can be deduced, but this is not a claim that all laws necessarily must be associated with classificatory schemes. These laws (or "lawlike generalizations") may be applied to further observations resulting often in new laws, or, perhaps, resulting in alteration or even abolition of existing ones. However, the final appeal is to observation, often in the form of experiment.

From this brief overview one may catch a glimmer of what is meant when it is said that science is a dynamic enterprise, for the term "science" is now less associated with "knowledge" *per se* (as its etymology implies) than with the making of new knowledge. That is, science is more nearly equated with the notion of "research," and this is to say that the distinguishing characteristic of science is not its content,

but its method. This does not imply that the method is flawless; nor that it cannot be changed or improved; nor that there are no conceptual problems involved; nor that the method cannot be philosophically analyzed as part of, for example, the quest for greater understanding of this important enterprise. Moreover, if this latter is at least one of the primary concerns, then this treatment of the contemporary context can be superficial and, perhaps, misleading, for the scientific tradition briefly sketched above, did not just "happen." Rather, it is the product of a long, multifarious history.

The purpose of this chapter is to develop an historical perspective preparatory to an analysis of one specific conceptual problem, that of discovery. This is part of a program that hopefully will lead to an evaluation of science as a rational, intellectual activity which has exerted a profound effect on the development of western culture as a whole.

The present interest in the history of science has revealed that science can no longer be considered solely a product of Greek civilization. For example, recent investigations of ancient Mesopotamian empires, such as those of Babylonia and Assyria have shown that there had been a far more extensive development and accumulation of information in astronomy and mathematics than was heretofore suspected. Furthermore, one may also claim that the development of geometry, for example, began in ancient Egypt, for it was here that the annual flooding of the Nile resulted in the need to develop surveying techniques. Another feature of ancient science was the cross-fertilization of ideas of eastern

cultures, again in greater degree than was, until recently, suspected. For example, Indian science influenced, and was influenced by, the traditions of western Asia and Greece. The chief areas of investigation were astronomy, physiology, psychology and plant science, the latter with particular reference to food supply and medicine. It also had a store of chemical knowledge that more closely resembled alchemy or *materia medica*. Its texts were devoid of reference to superstition and magic, and showed genuine attempts to seek natural explanations for observed phenomena. Furthermore, it possessed a theoretical logic against which ideas were tested.

However, despite the fact that Indian science speculated rationally, sought detailed knowledge from observations, and formed explanatory hypotheses, rarely did it experiment, except in psychology. Moreover, although it did have what we now call theories (which, presumably, gave mentally satisfying explanations), these were, in reality, founded upon insufficient facts.

The contemporary study of scientific history has also revealed that Chinese science has influenced Greek- and Latin-dominated western thought. Under the Han Dynasty (206 B.C.-A.D. 24) techniques in bronze-work, lacquering, dyeing, and fermentation stimulated the growth of science. Later, the development of industry and commerce, under the Western Tsin (A.D. 265-A.D. 317) and Eastern Tsin (A.D. 317-A.D. 420), favoured the expansion of science and the arts. It was in this period that Tsu Chung-chih (A.D. 429-A.D. 500) calculated the relationship of the circumference of a circle to its diameter, and placed it between

3.1415926 and 3.1415927, thus marking a brilliant stage in the history of Chinese mathematics. During the T'ang Dynasty (A.D. 618-A.D. 907), (and after the printing press had been invented and established), there appeared an encyclopaedia, *Ch'i-min yao shu* ("*Treatise on Agriculture*"), which dealt with agriculture and gave useful advice on fermentation. During the Sung Dynasty (A.D. 960-A.D. 1279), the first monograph on beriberi, entitled *Chio-ch'i chih-fa tsung-yao* ("*Notes on the Treatment of Beriberi*") was produced by Tung Chi. First hand observations of various medicines were recorded in *Pen t'sao* ("*Materia Medica*"), with mention of over 893 animal, vegetable, or mineral products which may be used in the medicinal treatment of various illnesses. Under the Ch'ing Dynasty (1644-1912), western science was introduced by missionaries, and such scholars as Wang Si-chan (1628-82) and Mei Wen-ting (1633-1721) began to study this European tradition. Still later, Tai Chen (1723-77) edited ten mathematical works under the title *Suan ching* ("*Classics of Mathematics*").

So much for a brief overview of the development of non-Grecian based science. Much more could be said, and much research remains to be done, but it is time to turn to the Greek influence itself. Traditionally, the beginning of Greek science is usually associated with Thales of Miletus who made some geometrical discoveries in the earlier part of the 6th century B.C. However, it was not until Pythagorus and his followers appeared that there was a rise of Greek intellectualism. This "school" believed in the independent existence of numbers, and also of the correspondence between the workings of mind and nature. By the



middle of the 5th century B.C., the so-called "Athenian" school had separated mathematics from medicine, thus marking the beginning of specialization that has characterized science ever since.

The two dominant figures in Greek science during the 4th century B.C. were, of course, Plato (428-348 B.C.) and Aristotle (384-322 B.C.). Platonism held mathematics as the paradigm of thought, with a resultant de-emphasis on observational activity. Aristotle attempted to systematize all knowledge, his most successful endeavours being in biology. For five hundred years after Aristotle, the scientific centre of the world shifted to Alexandria, Egypt, where mathematics, under Euclid, again assumed a predominant position. Some other prominent men of the Alexandrian period were Aristarchus of Samos (fl. c. 270 B.C.) and Hipparchus (fl. 146-127 B.C.) both in astronomy; Theophrastus (c. 287-212 B.C.) (biology, botany); Archimedes (c. 287-212 B.C.) (physics and mathematics); Apollonius of Perga (fl. 220 B.C.) (mathematics, with particular reference to conic sections); Eratosthenes (c. 276 to c. 194 B.C.) (measurement of the globe of the earth). Of all these men, particular mention should be made of Archimedes, generally regarded as the greatest of the Alexandrian school. The mathematical reconstruction of the Archimedian spiral, the mechanical construction of the Archimedian screw for raising water, and the exposition of the doctrine of levers, as well as his work in geometry, are all well known. Also notable during this period of science was the rise of anatomy and physiology as well as the exposition of the doctrine of atoms.

Although from the period 30 B.C.-A.D. 200 Alexandria was under

the political dominance of Rome, science remained Greek in both spirit and language. This was the age of Ptolemy (fl. c. A.D. 140) and the geocentric theory of the universe, and of Galen whose investigations into human anatomy, although in many instances erroneous, were nevertheless to have considerable influence for several centuries to come.

During medieval times, progress in science was negligible until versions of the Greek scientific works occurred in Arabic in the 10th and 11th centuries, and in Latin (from the Arabic) in the 13th and 14th centuries. The Islamic contribution to science, other than the presentation of Greek science in Arabic, centered around alchemy, mathematics, astronomy, and medicine, and some of their contributions in these areas had some effect on the rise of modern chemistry, especially in the introduction of new drugs, many of which are still in use. Astronomy and astrology (there was, at this time, no clear-cut distinction) were constant preoccupations of the Arabs, who, in addition, developed considerably the geometrical and optical works of the Greeks.

During the 11th century, science in the Latin west came into contact with that of the Latin east through Latin translations of Arabic works. Subsequently, during the 13th century, the revival of the works of Aristotle proved to be a major factor in the development of what may be called "intellectual coherence," and, furthermore, gave to scholastic science its essential character. Traditionally, the so-called "forerunners" of science, especially Roger Bacon (c. 1226-92) belong to this period, although the actual contributions, scattered over two or three centuries, are somewhat minute when compared to those of the best Greek

and Arabic centuries--so much so that these contributions of the "fore-runners" do not justify a revision of the standard view that modern science has its main roots in the 15th and 16th centuries. However, the better empirical technology of the new science was the product of the medieval centuries; and its method of exposition may be said to be rooted in scholastic thought.

Early modern science may be associated with the recovery of the scientific heritage of the Greeks, and this was accomplished largely by 1550. There appeared to be a renewal of interest in experimentation and observation, and the German cardinal Nicholas of Cusa (1401-64) recorded a careful experiment on growing plants, proving that they absorb some weight from the air. This was, in fact, the first modern, formal biological experiment, and the first experimental proof that air has weight. This was also the time of Copernicus and his heliocentric theory of the universe, which theory, despite its revolutionary nature, was still Ptolmaic in many ways. Indeed, these Ptolmaic vestiges support the claim that Copernicus was, in fact, more a conservative scholar than a scientific investigator. For example, his theory retained some of the Ptolmaic epicycles, the sphericity of the universe, and the notion of the uniform circular motion of celestial bodies.

This was also the era of Leonardo da Vinci (1452-1519) whose contributions in many fields, both in art and science, are well known. It was also the era of Andreas Vesalius (1514-64) whose *De humanis corporis fabrica* (1543) was the first great scientific monograph in the modern manner, albeit founded solidly on the (mistaken) tradition of Galen.

As Medieval science was rooted in Aristotelian thought, Renaissance science witnessed a revival of Platonic thought. Simon Stevin (1548-1620) stated that science could not well advance until its mathematical framework was improved. He was also one of the earliest to detect flaws in Aristotle's system.

The great exponents of scientific method in the earlier part of the 17th century included Galileo, Kepler, and Descartes, each of whom made valuable contributions to both mathematics and physics. By the middle of this century, Aristotle's cosmology had fallen, and his biology was being rebuilt. In a sense, science had reached a maturity that was ripe for formal organization in the period which followed. Most of this newly found maturity was exemplified in the brilliant work and personality of Galileo.

The beginning of the modern scientific outlook did not occur until the first half of the 17th century, and featured a de-emphasis on scholastic rationation concomitant with a new reliance on experience, especially through experiment, as a means of acquiring knowledge. In cosmology, the heliocentric theory had replaced the geocentric, and chemistry had replaced alchemy. Physical experiments were expressed as far as possible in mathematical terms, and physiology set physics as its ideal. In the biological sciences, Harvey had worked out the circulation of the blood, and a new classification of both animals and plants (albeit by basically Aristotelian methods) was begun.

From the latter half of the 17th century to the end of the 19th century, it is safe to claim that western civilization was most

influenced by the deterministic framework of Newton (1642-1727). Within this conceptual scheme all material events would be grounded in mathematical and/or logical rules. Indeed, this "ideal" became the main task of his "philosophy" and, for that matter, of science in general. Moreover, scientific determinism also influenced the approach to the study of mental phenomena. Charles Darwin's *Origin of Species* contained the germ of biological determinism, and Karl Marx's *Zur Kritik Der Politischen Ökonomie* contained the germ of psychological determinism; taken together, both works may be said to form the basis of social determinism.

Contemporary science has laid less emphasis on determinism, and the formulation of one basic conceptual scheme. Indeed, it has sought to question much more closely the conceptual foundations, such as the ontological and epistemological bases of its assumptions, conclusions, and methodology. This kind of philosophic inquiry has exposed, for example, the danger of transferring concepts from their original fields of reference, such that one becomes more careful in using such terms as "evolution," "race," "heredity" and "value" as an attempt to give a scientific status to what, in fact, may be nonscientific judgments. As well, this inquiry has sharpened understanding and cleared away some of the confusion surrounding such terms as "substance," "theory," "idea," "cause," "purpose," and "function" as they are construed in scientific, theological, or philosophical fields of study. Moreover, it has pointed up some of what may perhaps appear to the layman as conceptual and/or methodological "weaknesses" of science. For example, the bases for observations often require a presupposed theoretical framework, and this

raises the problem of limitation (or closure) in the generation of hypotheses. A further problem is that of the intrinsic limitation of all experiments, and this includes not only the problem of the ontological status of theoretical constructs, such as the electron, but also includes the intrinsic limitation of all experimentation *vis-à-vis*, for example, the *Heisenberg Uncertainty Principle* which principle states that it is impossible to specify or determine precisely both the position and the velocity of a particle. This can be expressed as the inequality:

$$\Delta x \Delta v \geq \frac{h}{4 \pi m}$$

or, as usually expressed, in terms of momentum  $p$

$$\Delta x \Delta p \geq \frac{h}{4 \pi m}$$

Now, when one is discussing macroscopic quantities, the uncertainty is of little consequence, because  $h$  is very small ( $6.6 \times 10^{-27}$  ergs/sec.), and where  $m$  is relatively large; but uncertainty is of obvious importance when one is considering electrons ( $m = 9.0 \times 10^{-28}$  gms.) and other small particles. As mentioned earlier, this raises extremely interesting philosophical problems, for it implies intrinsic limits to experimentation. Relatedly, then, a further philosophical problem emerges concerning the status of scientific conclusions, for even the most general laws may not be (and probably cannot be) "ultimate" goals; they may have only provisional status in the service of, say, explanation and prediction,

or they may be only the starting points for further investigations.

Many of these problems will be encountered again as the thesis develops.

Hence, today there is an expanded interest in the philosophy of science. This emphasis on conceptual investigation is itself a part of one of the main features--and secrets of success--in science, viz., that of specialization, and of subsequent application of knowledge gained in one area to other areas, resulting in a strong interdisciplinary approach. Science never considers (and cannot consider) the world as a whole. Each science, within its own concepts and language, describes only a little bit of the universe.

## SECTION II

*A brief historical survey of the social sciences. Social science and normative philosophy. Rise of scientism and the schism of social science from normative philosophy. Contributions of Comte and Marx, and a brief reference as to why their theories failed. Social sciences today. The use of the term "social science" as classification.*

The scientific approach to the study of social phenomena did not begin until the nineteenth century. Previous to that time the rules of social organization were oriented towards a normative, philosophical basis which emphasized the rules such as they ought to be, rather than seeking to discover rules of social organization as they exist. However, this does not necessarily imply that these normative problems are easily solved, and indeed, according to some philosophers and social scientists, solution may be impossible.

Duverger (1961) states that:

. . . in its original form, social science was embedded in a mass of normative considerations of a moral or philosophical nature. . . . The proportion of scientific observation relative to the normative considerations varied depending on the author.<sup>1</sup>

However, he goes on to say that "the metaphysical and normative context hindered neither careful analysis of the real world nor the development of the comparative method: in this respect Aristotle remains a model."<sup>2</sup> Duverger is claiming then, that some essential features of scientific attitude were present, for careful observation and an intellectual temperament disposed to reflection on information already available and, moreover, prepared to seek new information, are important features of scientific methodology.

As the scientific approach became increasingly predominant, a decisive separation between science and this type of normative philosophy occurred. Concomitantly there appeared the notion that social phenomena may have a regular character such as to allow their being subsumed under natural laws more or less analogous to those which govern physical phenomena in the universe. Unfortunately, the eighteenth-century authors neither clearly delimited the field of social science, nor precisely defined its purpose. Indeed, it is not until the contributions of Auguste Comte and Karl Marx that one arrives at a program for the establishment of the objective and scientific character of social science.

From the perspective of the discussion so far, probably the main contribution of Comte was, as Duverger suggests, "the assertion of the



positive character of the social sciences and their definitive separation from morals and metaphysics."<sup>3</sup> Unfortunately, for future development, Comte thought that "the whole social mechanism rests on opinions"<sup>4</sup> and he thus imparted a subjective character to the sociology he developed. Marx, however, went further in an attempt to secure social science an objective base by his claim that juridical relations, political forms, and the whole social structure rested on the economic infrastructure, and on the relations of the forces of production. In fact, Marxism was the first complete system which attempted to explain all social phenomena. Despite its inadequacies and errors, Marxism has exerted considerable influence as a basic framework and reference system, but that:

The comprehensiveness of the *doctrine* (italics added) has encouraged Marxists to use deductive reason rather than experimental research, thus developing a new scholasticism within a dogmatic framework; the disciplines of one of the greatest among the founding fathers have thus, paradoxically, returned to the primitive confusion between science and philosophy.<sup>5</sup>

The development of particular social sciences is not wholly a twentieth century occurrence for even when social science was still dominated by metaphysical and moral considerations, some specialist disciplines were founded centered around the criterion of experimental method. An example of this is political economy, which was established as an autonomous science clearly separated from philosophy by the Physiocrats and Adam Smith.

With the increasing amount and complexity of subject matter, it was almost inevitable that social science would divide into a number of

separate disciplines. Today, the scientific enterprise is often viewed, *in toto*, as comprising three broad divisions: the physical sciences, the biological sciences, and the social sciences. However, the boundaries are often neither distinct nor static. For example, psychology and anthropology may be considered in part social and in part biological, sciences, whereas geography would, as a discipline, cut across all three major divisions. Also, history may be classed by some as a science, but others would include it as a discipline within the humanities along with literature and the fine arts. Social psychology is variously classed as a branch of psychology, as a branch of sociology and as a science in its own right. Any classification then, is only for convenience in description and study, and need not exclude all doubtful cases in order to be useful. What is important is that all these disciplines strive to employ the basic, methodological principles of the natural sciences which have been surveyed in the previous section.

### SECTION III

*Herbart and the beginnings of the scientific study of education. G. Stanley Hall and the "Child Study Movement." The "Cultural Epochs Theory" (Recapitulation Theory), J. M. Rice, Maxwell and the modern scientific movement. Statistics in educational research. Development of standards. Binet and the I.Q. metric. Laboratory studies. School surveys and the establishment of bureaus of research.*

The scientific approach to the study of education may be said to have begun with Johann Friedrich Hebart (1776-1841) who did most of his

work between 1800 and 1841. He held that education should be based on two fundamental sciences, psychology and ethics. Indeed, the development of psychology during the last decade can be traced, in very large measure, to the influence of Herbart; and in Germany, Herbart's "movement" was so effective as to lead to the establishment of laboratories for the study of mental processes of both adults and children.

The work of G. Stanley Hall (1846-1924) and his "Child Study Movement" was also to prove extremely influential. The emphasis was again on the psychology of children, and the chief means of investigation was the questionnaire. Different aspects of children's mental development were formulated into questions which were sent to parents and teachers all over the United States. The results were recorded but, unfortunately, conclusions were subsequently drawn without very serious regard to the validity of the answers themselves. In short, the movement failed because of an unsystematic methodology of investigation, and all that was obtained was a collection of interesting material about the development of children which, in general, was not beneficial to educational practice.

Another attempt at a scientific approach to education culminated in the "cultural epoch theory" (recapitulation theory) which held that the development followed by children closely parallels the line of development held by the human race. For example, parallelisms were drawn between children's drawings and the art of primitive peoples. To the followers of Herbart, the theory was viewed as a method by which a course of study appropriate for the schools could be laid out. They had only to find, by a careful study of the history of the race, the different

epochs (stages) through which civilization had passed. The theory proved unsatisfactory, mainly as a result of its crude generalization. However, it did have one beneficial effect, and that was to create a great deal of criticism and skepticism of traditional teaching methods, and to suggest the possibility of new methods. For example, at this time the Herbart Society, which set up experimental schools for the study of teaching methods, was formed.

The so-called "measurement movement" was founded by J. M. Rice (1857-1934) whose investigation of spelling and arithmetic, by means of tests, spread to almost all other subjects of the curriculum. Rice's tests, together with a close examination of the results and implications of these tests, were carried out in the United States in 1894 to 1895. The influence of this movement is still felt today.

The modern scientific approach in education may be said to have begun with Superintendent Maxwell's Reports of the New York school system. Basically, he called attention to the fact that numerous assumptions with regard to the progress of children through the elementary school grades were without support. Also, studies in biology and anthropology revealed that mental characteristics, like physical ones, seem to follow the same law of distributions. For instance, in a given population, there always seemed to be a few highly gifted individuals (if not geniuses) as well as a few extremely dull individuals (if not idiots) but the individuals of the whole sample tended to group around a centre or average. This, of course, is the normal distribution the application of which is still in use. The point of mentioning it here, however, is

to indicate the rise of interest in statistics, which interest subsequently gave rise to studies in acceleration, retardation and elimination. Some of the most productive scientific studies in the early 1900's were done on these latter two categories.

The publication of L. P. Ayres' (1879-1946) book, *Laggards in Our Public Schools* (1909) had much influence. The "age-grade tables" called attention to retardation in schools, and stimulated teachers and administrators to make attempts at improvements. The results have been positive, for now the proportion of students who achieve grade eight status is over twice the number before these scientific investigations were undertaken. The way was now prepared for the development of standards in school work, which standards can perhaps be first attributed to E. L. Thorndike's (1874-1949) handwriting scale (1909) based on numerical terms. Teachers no longer had to make the assumption that children in different grades can write perfectly. Furthermore, such standardizations spread to other subjects of the curriculum, but it should be understood that standardization was not meant to be the reduction of all school work to an absolute uniformity. It can be argued that this could never be so. The advantage is that now the degree of excellence that can be obtained can be specified, and the teacher can thus adjust the methods of instruction.

Together with the advent of measurement, statistics, testing and standardization came the use of intelligence tests, which, in America, were vigorously promoted by James McKeen Cattell. Binet's tests of intelligence soon followed but no attempt was made to predict a child's

future mental age, and this had important ramifications for the study of retardation. Early in the history of intelligence tests, therefore, psychologists adopted the practice of stating the relationship between mental age (M.A.) and chronological age (C.A.) as a ratio, the intelligence quotient (I.Q.). The concept of I.Q. was used in the development of intelligence tests by L. M. Terman. Today, these and other versions of the tests are well known and will not be dealt with here, suffice to say that they are always a measurement of relative intelligence expressed as a single numerical term. The Stanford Revisions of this test led to extremist views in education, such as the grouping of all students at one level of I.Q. with each other, regardless of other factors such as age. Again, all the problems that such a misuse of the scale gives rise to will not be dealt with in this thesis. It only needs to be said that these tests are now used to support the results of other evaluations, and teachers' judgments, in classifying students in all grades in the schools. This is perhaps their most legitimate use, for it appears that they cannot be used to the exclusion of all other evidence with regard to student achievement.

A further scientific approach is based on laboratory studies centered on the physical maturing of the child and the relationship to learning. This has led to detailed knowledge of, for example, the pre-school child, studies in reaction measurements, and studies in reading and eye movements. In the latter case, photographs have been made of the movements of the eye which have led to discoveries of difficulties which arise in training the child to read.<sup>6</sup>

Finally, school surveys have been used to obtain general measures of the efficiency of an educational system. The New York Survey in 1912 was received with severe criticism especially with respect to methodology. The Portland (Oregon) survey of 1913 was more successful, methodologically, and was notable for the very full statement it made of the theory of administrative organization for a city like Portland. The Springfield (Illinois) survey, conducted by L. P. Ayres gave special attention to the buildings. It also employed the methods of measurement and tests with more completeness than did any of the previous surveys. The report was much less open to question and much more significant as an illustration of scientific methodology in the evaluation of school organization. The Cleveland Survey, in twenty-five volumes, covered in detail every aspect of school organization and classroom work. Since these early times, many other surveys have been carried out, and Bureaus of Educational Research have been established to provide continuous studies of school systems and school operations for the maximization of the educational experience for all concerned.

#### SUMMARY

After a brief sketch of the scientific enterprise as it is known today, a survey of the history of this enterprise has been undertaken. It was shown that contemporary interest in the history of science has revealed that many other cultures have contributed to its origins. The civilizations of ancient Egypt, Mesopotamia, India, and China have all

influenced the Greek-based origins of Western scientific thought. Some of the main "schools," such as the "Athenian" and the "Alexandrian" have been noted, in addition to some of the most significant people: Plato, Aristotle, Archimedes and Ptolemy in ancient times; Roger Bacon, Nicholas of Causa, Leonardo da Vinci, and Andreas Vesalius in medieval times; Galileo, Kepler, and Descartes in renaissance times. Also noted was the fact that the origins of modern science did not begin until the first half of the 17th century, and this new outlook featured a reliance on experiment. Mention was also made of the Newtonian deterministic framework that held sway from the 17th century to the end of the 19th century. Finally, a few examples were given of the types of conceptual problems that are being investigated today, such investigations being one of the chief characteristics of the modern scientific enterprise (i.e., rigorous questioning of its own conceptual and methodological foundations in addition to a greater interest in the historical foundations) and to which types of problems the remaining chapters of the thesis will be devoted.

Sections II and III have been included to show how scientific methodology has been employed in the behavioural disciplines. In section II, a brief historical survey of the development of the social sciences, with particular reference to Comte and Marx, was undertaken, and in section III a similar survey treatment was applied to the scientific movement in education. The contributions of such men as J. F. Herbart, G. Stanley Hall, J. M. Rice, and A. Binet were briefly mentioned.



This chapter has been included specifically to develop an historical perspective preparatory to a detailed examination of the modern enterprise. For example, it is easy to overlook the fact that the origins of scientific methodology are grounded not only in Greek culture, but also in Chinese, Indian, and Mesopotamian cultures as well. Moreover, despite the fact that scientific inquiry today may be regarded as systematic, controlled, critical, and objective, this has not always been the case. Historical analysis, by elucidating these and similar points can contribute much to a deeper understanding of the conceptual foundations of research, not only in the natural sciences, but also in the social sciences and in education.

This concludes the historical survey for "background" purposes of the thesis. The investigation will now be limited to an examination of the conceptual foundations, focusing specifically on the nature of scientific discovery. The emphasis in Chapter Two through Chapter Four will be on the natural sciences, and in Chapter Five the implications of the conclusions will be extended to include also the social sciences and education.

## FOOTNOTES

<sup>1</sup>M. Duverger, *Introduction to the Social Sciences* (London: George Allen and Unwin Ltd., 1961), p. 12.

<sup>2</sup>Duverger, p. 13.

<sup>3</sup>Duverger, p. 16.

<sup>4</sup>Duverger, p. 16.

<sup>5</sup>Duverger, p. 19.

<sup>6</sup>Many other psychologists have made important contributions to educational psychology. Perhaps the most famous is the Swiss psychologist, Jean Piaget (1896-19-- ) who has been a leading investigator of thought processes among children. The most distinctive feature of Piaget's contribution to experimental psychology is his developmental or genetic approach. His system sees cognitive development as a gradual advance toward more thorough and intelligent adaptation to the environment, marked by more complete equilibrium among psychological processes. Some psychologists who have influenced education include J. S. Bruner, S. S. Colvin, L. J. Cronbach, John Dewey, J. P. Guilford, William James, B. F. Skinner, Charles Spearman, Lewis Terman, and many others.

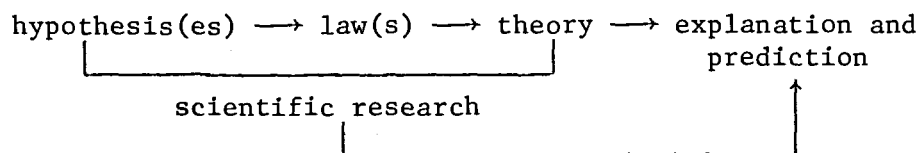
## CHAPTER TWO

### THE STRUCTURE OF SCIENTIFIC RESEARCH

#### SECTION I

*Brief overview of scientific research ("SR Model A"). The distinction between "pure" and "applied" research. The relationship of theory to research. Theory construction (to be examined in more detail in Section III) and the hypothetico-deductive system ("SR Model B and C"). The Popperian modification of the traditional hypothetico-deductive system.*

For introductory purposes, the scientific research enterprise can be schematically represented as follows:



*SR Model A*

It should be noted that any random trial of something new does not necessarily qualify as research. Scientific research is a systematic, controlled, empirical, critical and objective investigation concerning hypothetical propositions about assumed relationships among natural phenomena. (The latter characteristic of "objectivity" is a particularly thorny problem, and will be taken up in more detail later in the thesis.)

In short, the basic purpose of research is the formulation of theory (or theories) which, in turn, is aimed at the production of satisfactory explanations (rather than mere descriptions) in order that one may arrive at a better understanding of the phenomenon in question. Scientific knowledge begins with the notice of regularities in the course of events, and these regularities are analyzed for their value not only in explanation, but also in prediction and control. Often, however, control is not direct, for while on the one hand an astronomer can reliably predict eclipses, he cannot in fact control their occurrences; on the other hand, a meteorologist might predict rains, and although he exerts no direct control, *per se* over them, adjustments such as erection of dams, emergency measures, evacuations, and so on can be made. In these cases, although the control is indirect, some type of control is possible on the basis of the scientist's ability to predict. As Hospers says:

The scientific enterprise could be described as the search for genuine invariants in nature for regularities without exception, so that we are enabled to say, "Whenever such-and-such conditions are fulfilled, this kind of thing *always* happens."<sup>1</sup>

But it should be emphasized that these regularities which we do observe are very seldom strict invariants. Moreover, the explanations with which scientific research is concerned are those which are stable, that fit in with other existing theories, that have been tested, and that have been treated, insofar as possible, "objectively" (that is, free from any preconceived notions). Still further, theory and explanation have unique and specific purposes, namely to provide knowledge

through which practice can be rendered more effective.

It is generally recognized that there are two facets to the research process: "basic" or "pure" research and "applied" research. The former is designed so that an organized body of knowledge can be extended; it does not necessarily result in any immediate practical value. The latter type of research is undertaken to solve an immediate practical problem, and is thus concerned with the application of knowledge, gained from basic research, to practice. As such, any contribution of applied research to the organized fund of knowledge is secondary.

As has already been mentioned above, the immediate aim of scientific research is theory construction. When a science first begins, it is almost entirely empirical in nature, concerned directly and almost completely with observable phenomena, and with relationships among them. To put it another way, science begins with observation, such as the falling of an apple or the observation that combustion is impossible when air is removed, or that stones of different sizes and weights fall a given distance in the same time interval. These events are, without doubt, in the realm of observations, but, as such, cannot explain these events. Until one begins to understand why the observable phenomena occur, the science is crude and primitive and eminently incapable of explanation.

What does it mean "to explain" a phenomenon  $x$ ? It may be assumed that to explain  $x$  is to understand  $x$  in such a way as to bring someone else to understand it. If this assumption is granted, then two levels of explanation may be distinguished. At the primitive level is

"explaining how" which is associated with the concepts "understanding how" and "learning how." One can speak then, of explaining how to play the piano, and this would involve the student's learning how to sit, how to position the fingers, hands and arms, how to use the pedals, and so on. But, if one were to take the example of the interpretation of a Bach fugue, one is at once concerned with a higher level of explanation, for here one is dealing with a conceptual framework, namely the Baroque musical style. In this case, the teacher's task is that of explaining why and is associated not only with understanding why and learning why, on the part of the student, but also with the giving, by the teacher to the student, of valid reasons. In contrast to "explaining how," which is concerned with the acquisition of some perceptual motor-skill or habit, "explaining why" is concerned with theoretical or conceptual justifications, and this involves the formation of concepts and the employment of some form of inference.

It is this second type of explanation ("explanation why") which is of concern to the scientist. In his attempt to "understand why" he invents or discovers such abstract concepts as "gravity," "energy," "mass," "heat," and "velocity," all of which cannot be observed directly, and hence are in the realm of theory. The philosophical problems arising from the ontological status of these theoretical constructs are legion, and will not be taken up here, suffice to say that it is through these abstract concepts, and the relationships that are hypothesized to exist between them that the scientist does come to explain, understand, predict, and often, to some extent at least, to control phenomena which may be directly observed.

As sciences mature, they become increasingly concerned with theory and proportionately less concerned (though not absolutely) with observables. Indeed, it can be quite easily argued that the maturity of a science may be ascertained by using as a measure the development and sophistication of its theories which, in addition to being explanatory and predictive, should also be comprehensive, systematic, empirically demonstrable, and capable of exerting some type of control over observable events. According to these criteria, the sciences of physics and chemistry would seem to be the most advanced, whereas the behavioural sciences (of which educational research may be considered one) are considerably far behind. In these disciplines, many of the theories are questionable not only on conceptual and methodological grounds, but also because of their isolated and fragmented status.

It should be pointed out that scientific research both follows and precedes theory. After theory construction, experimental research provides the means whereby theory is tested, as well as the means for obtaining information on which theory is based, and also by which theory is modified and improved. In short, research cannot exist at an advanced level without theory, although "theory" can exist without research, in which case the scientific value of the theory is dubious. (This notion of "test" is central to Popper's approach which will be discussed shortly.) At this time it will be sufficient to say that unless a theory is subjected to rigorous empirical test through the research process, there is no way to judge its validity, and so the theory will be no more than mere opinion. It can be stated, then, that the first requirement

of a scientifically sound theory is that it be amenable to test, and that this testing be performed. Research cannot exist without theory, and valid and useful theory cannot exist without research.

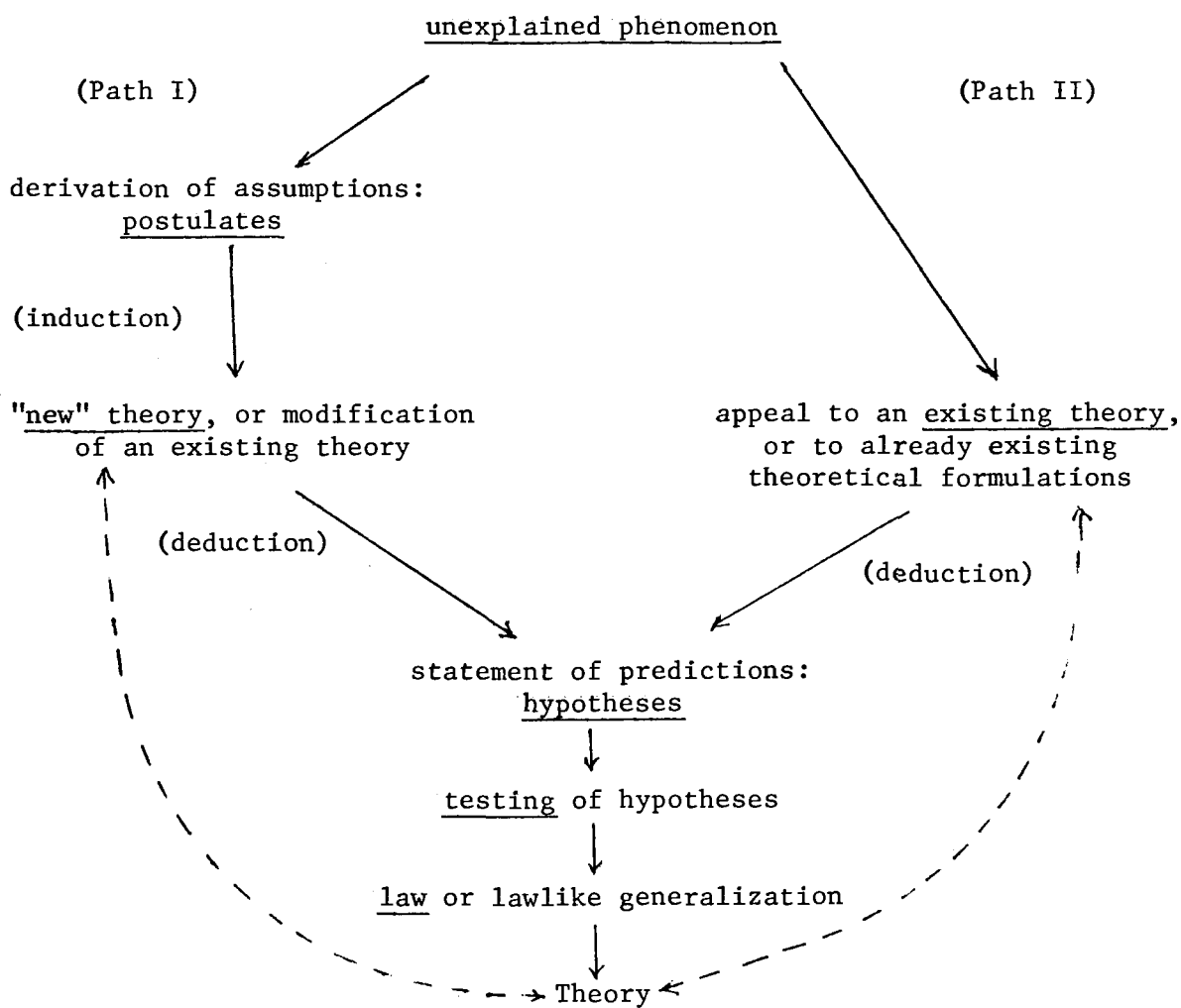
It is time now to focus briefly on the nature of "theory." The term is often used very loosely, but to a scientist it may be generally defined as a deductive system of abstract, hypothetical (or theoretical) constructs, and of relationships among them, which is intended to explain particular aspects of nature, or of human behaviour, and which can generate testable predictions.

Theory construction is initiated by an unexplained phenomenon. One may then attempt to build a "new" theory by derivation from the observations, assumptions or postulates. These postulates are statements of the principle assumed despite the lack of direct evidence, after which, by inductive inference, a "new" theory, or a modification of an existing theory is constructed. From this tentative theoretical system, a statement (or statements) of predictions, called an hypothesis is deduced which is then subjected to experimental testing, and, if validated, the hypothesis can then be reformulated as a law or lawlike generalization. It may then be combined with further laws (arrived at in the same manner) or with already existing laws, to constitute a comprehensive, systematic, explanatory theory. Alternatively, the initial, unexplained phenomenon, instead of requiring the formulation of postulates, may require only an appeal to an already existing theory, or to extant theoretical formulations from which the hypothesis(es) may be deduced. This process of theory construction, known traditionally as



the hypothetico-deductive system, may be represented schematically as:

(SR Model B)



SR Model B

It should be noted that in Path I, the theory produced will be of course the "vindication," so to speak, of what was earlier referred to as the "new" theory, and it must also be tested for its "fit" with already established theories. So also with Path II, where the new theory produced must also be tested for "fit" with other theories. Moreover, the

"new" theory here (as also in Path I) may require modification, or even abandonment of other theories, including, as in the case of Path II, the theory which was appealed to in order to arrive at an hypothesis. This would result in what T. S. Kuhn would call a "scientific revolution."

The above is the traditional account of the hypothetico-deductive system, and as the thesis proceeds, modifications will be introduced. A symbolized scheme will now be presented (*SR Model C*) in which the following abbreviations will be used:

UPh = unexplained phenomenon

P = postulates (assumptions)

NTh = new theory

L = law

ETh = existing theory

Th = theory

H = hypothesis

(I) = induction

TH = testing of hypothesis

(D) = deduction

In addition, the following terms will be used:

I

SD sp = scientific discovery, special sense, path I

I

SD sp = scientific discovery, special sense, path II

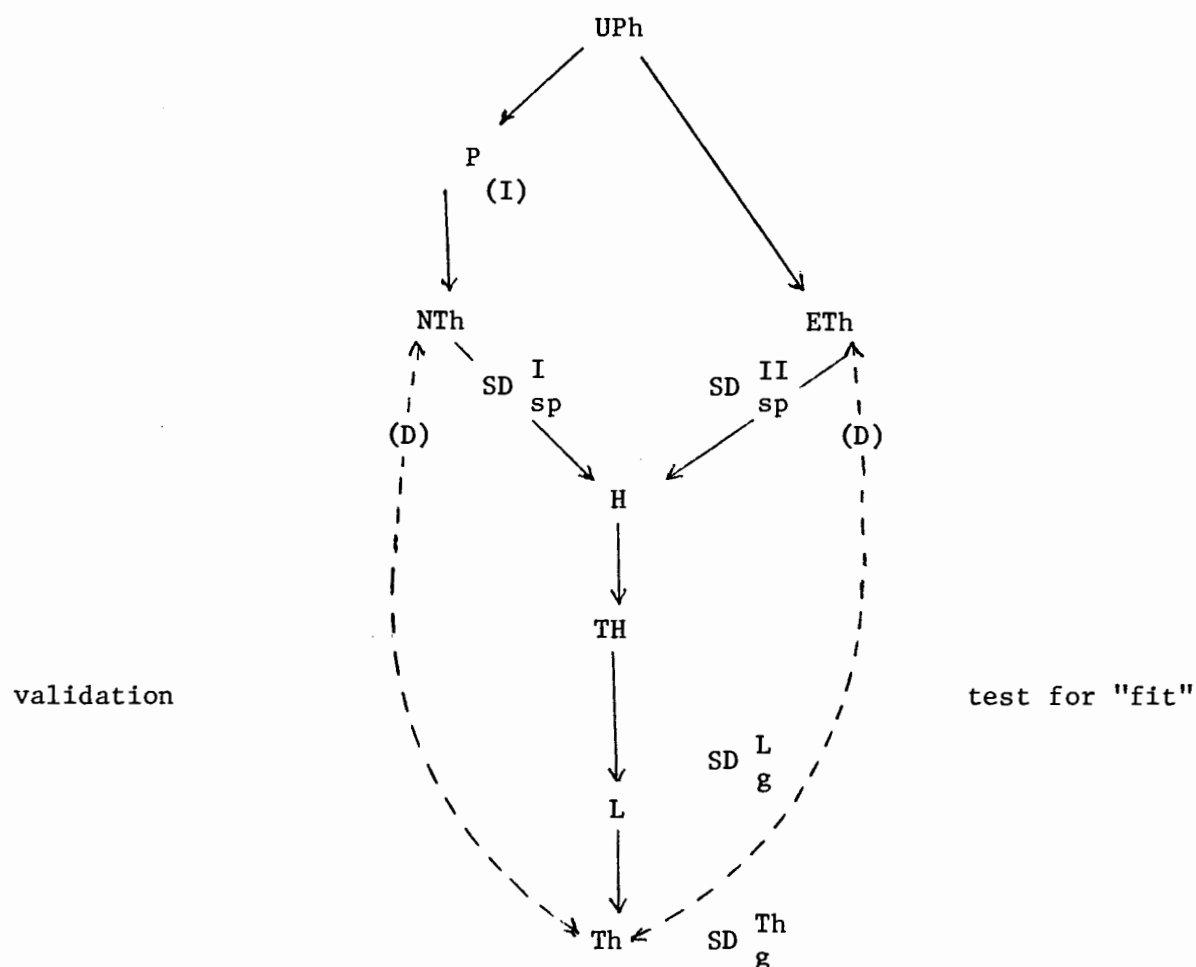
L

SD g = scientific discovery, general sense, level of  
formulation of laws

Th

SD g = scientific discovery, general sense, level of  
formulation of theory

The symbolized scheme will then be:



*SR Model C*

This scheme locates the concept of "scientific discovery" which is of primary concern in this thesis. However, before the subject is treated in more depth (Chapter Three), it will be necessary, in order to understand fully the content of Chapter Five, to investigate in some depth a number of additional preliminaries.

In the outline of the hypothetico-deductive method just undertaken,

it was pointed out that if an hypothesis is tested and confirmed it is then admitted, as it were, into the status of "law" or "lawlike generalization." According to hypothetico-deductive theorists, each additional confirmation lends greater support to the theory, and  $n$  confirmations (where  $n$  approaches infinity), it may be taken that the theory will then be verified. The status of a scientific theory, then, will vary directly with the number of confirmations that aim at verification of the theory. Presumably, this would also provide a measure for demarcating a scientific theory from a pseudo-scientific one, for in the case of the latter, very few, if any, confirmations would be possible.

But in fact, such a probabalistic approach is unsatisfactory, for one is immediately thrown up against Hume's problem of induction. In the consideration of confirming instances of a theory, there still remains the brutal fact that no matter how many confirmation are obtained ( $n$ ), there is no logical guarantee that the next instance ( $n+1$ ) will also be a confirming one.

It was this very problem that led Karl Popper to modify the hypothetico-deductive method. He maintains that scientific research should not attempt to produce highly probable hypotheses (and, hence, laws and theories) or hypotheses that are highly confirmed by evidence. Popper's position will be examined in more detail in the immediately following section, but for the time being it will be sufficient to say that his modification of the traditional hypothetico-deductive account centers on the criterion of falsifiability. He holds that scientific theories should make bold assertions about the world, and also be both

risky and testable. One must make every attempt to falsify one's own theory, and only if it has withstood such rigour, can it then be identified as, not confirmed, but corroborated. Thus, such a theory cannot be regarded as final truth; in the long run it is as strong as its initial hypothesis, and the status of that hypothesis will be, in the end, nothing more than a tentative conjecture.

In logical terms, Popper is arguing against *Modus Ponens*:

$$([p \supset q] \cdot p) \supset q$$

which is the logical structure of the traditional hypothetico-deductive account. It can be easily seen that Popper's method of falsification and corroboration is logically captured by *Modus Tollens*:

$$([p \supset q] \cdot \sim q) \supset \sim p$$

It should also be noticed that while falsification can be directly associated with *Modus Tollens*, Popper's notion of corroboration is not associated with *Modus Ponens* in the same way. That is to say, there is an asymmetry revealed here, for hypotheses are not regarded as true (by *Modus Ponens*) simply because they are highly corroborated. That is, *Modus Ponens* is a necessary truth, but according to Popper's criterion, if *Modus Ponens* "occurs" in the test of an hypothesis, the result is a high corroboration, even in spite of the fact that logically it remains a necessary truth. This is because necessary truths are *a priori* and analytic, and have no necessary existential claim in the universe of experience. When highly corroborated, such hypotheses must be regarded

as still possessing the status of tentative conjecture, and must still be subjected to the severest possible criticism. Moreover, falsification, in itself, cannot establish a scientific hypothesis, for if an hypothesis is falsified, many alternative hypotheses remain unfalsified. Similarly, there is nothing exceptional about an hypothesis that survives without having been falsified, for there would remain many other unfalsified hypotheses to explain the same phenomenon.

But hypotheses do differ with respect to the ease with which they can be falsified, and this entails a criterion for choosing one hypothesis over another. As Salmon writes:

Popper directs us to seek hypotheses that are as highly falsifiable as possible. Science . . . is interested in bold conjectures. These conjectures must be consistent with the known facts, but they must run as great a risk as possible of being controverted by the facts still to be accumulated. Furthermore, the search for additional facts should be guided by the effort to find facts that will falsify the hypothesis.<sup>2</sup>

Furthermore, it may be stated that high probability means low falsifiability, for the greater the logical probability of an hypothesis, the fewer the potential falsifiers. In addition, high content means high falsifiability, for, as Salmon writes:

The greater the number of possible states of affairs excluded by a statement, the greater its content, for the more it does to pin down our actual world by ruling out possible but nonactual states of affairs. At the same time, the greater the range of facts excluded by a statement--the greater the number of situations with which the statement is incompatible--the greater the risk it runs of being false. A statement with high content has more *potential falsifiers* than a statement with low content. For this reason, high content means high falsifiability.<sup>3</sup>

As Popper's approach will become important to the main argument of the thesis, a closer examination of his position will now be undertaken.

## SECTION II

*Popper's problem concerning the status of scientific theories (reference to Marx and Freud) and the attempt to arrive at a criterion for demarcating a scientific theory from a non-scientific one. The approach of the Logical Positivists--verification by confirmation (also problems of Induction). Popper's criterion of falsification as preparation for a closer investigation of scientific theories. A second look at the theories of Marx and Freud in terms of Popper's criterion.*

The problem that led Popper into examining the scientific status of theories stemmed from the following question: "When should a theory be ranked as scientific?" or "Is there a criterion for the scientific character or status of a theory?" Traditionally, science is distinguished from pseudo-science by its empirical method, which is essentially inductive, proceeding from observation or experiment. However, for Popper, this claim was unsatisfactory, for he was apparently struck by some factor or factors that made Marx's theory of history and society and Freud's psychoanalysis theory of personality different from, for example, Einstein's theory of relativity. Popper himself says:

It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories--the Marxist theory of history, psychoanalysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps took the simple form, "What is wrong with Marxism, psychoanalysis,

and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity."<sup>4</sup>

He goes on to state that, at that stage, he was not concerned with the truth of any of these theories, nor with the problems of exactness or measurability. His perplexity arose from the apparent explanatory power of these theories, and of the characteristic, incessant flow of confirmation, of observations which "verified" the theories in question. To quote Popper again:

Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it . . . A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation--which revealed the class bias of the paper--and especially of course in what the paper did *not* say. The Freudian analysts emphasized that their theories were constantly verified by their "clinical observations" . . . What, I asked myself, did it confirm? No more than that a case could be interpreted in the light of the theory . . . It was precisely this fact--that they always fitted, that they were always confirmed--which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness.<sup>5</sup>

Such was the traditional account of experimental method as conceived by empiricists, including the logical positivists. Before continuing with Popper's problem, it may be helpful to recall the program of the logical positivists.

The verifiability principle stands historically in a line of direct descent from the empiricism of Hume, J. S. Mill, and Ernst Mach. From it, the most distinctive doctrine of logical positivism was founded, namely, that in order for a sentence to be cognitively meaningful it



must express a statement that is either analytic or empirically verifiable. This was not meant to imply that sentences may not have "emotive," "imperative" or other sorts of meaning (for example, "What a lovely day!" or "Open the window!"). However, these sentences have no cognitive meaning, for they do not express anything that could be true or false, or a possible subject of knowledge. Apart from sentences expressing analytic statements, the logical positivists claimed that for a sentence to have "cognitive," "factual," "descriptive," or "literal" meaning (for example, "The sun is ninety-three million miles from the earth") it must express a statement that could, at least in principle, be shown to be true or false, or to some degree probable, by reference to empirical observations. Thus, it would be that such sentences as "God is love," and "Beauty is significant form," must be cognitively meaningless. The verifiability principle, it was claimed, demonstrates the impossibility of metaphysics, and from this it was concluded that empirical science is the only method by which one can have knowledge concerning the world.

However, this very principle, together with the traditional sorts of problems raised against induction, were attacked. Indeed, the well-known difficulties of induction still lack any generally accepted solution. For convenience they may be described as:

- (1) The general problem of justification: Why, if at all, is it reasonable to accept the conclusions of certain inductive arguments as true--or at least probably true?

- (2) The comparative problem: Why is one inductive conclusion preferable to another as better supported? Why is one rule of inductive inference preferable to another as more reliable or more deserving of rational trust?
- (3) The analytical problem: What are the criteria for deciding that one rule of inductive inference is superior to another?

Although these problems will not be discussed in any detail here, they are listed to give an indication of the scope of the problem. For the context of this thesis, it will be sufficient to repeat that, traditionally, the distinguishing feature of science has been its empirical method, which is essentially inductive, proceeding from observation or experiment. But it was this very process that bothered Hume, for he argued that induction cannot be logically justified (this is problem (1) listed above). He claimed that there can be no valid logical arguments allowing us to establish "that those instances, of which we have had no experience, resemble those, of which we have had experience."<sup>6</sup> Thus, "even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience."<sup>7</sup> In carrying Hume's problem to its logical conclusion, it is found that any attempt to justify the practice of induction by an appeal to experience must lead to an infinite regress. Consequently, it may be claimed that theories can never be inferred from observation statements, or rationally justified by them. In terms of the verifiability principle, the problem of

induction has deeper implications, for if one were to attempt to justify (verify) a theory by an appeal to induction, then the problem becomes one of deciding how many confirmation instances are required to confirm the theory. This is an infinite class, and the problem (problem (1) preceding) remains. At best, it seems, one can only speak of degree of verifiability, and this will be probabilistic, not exact.

In the light of the problems raised concerning the theories of Marx and Freud, induction, and the Verifiability Principle, Popper was led to the problem of demarcation of science from non-science, and thence of determining a criterion for the scientific status of a theory. Verifiability based upon induction (i.e., inference based on many observations) was inadequate. Now, in considering Einstein's theory, Popper noticed a striking difference when it was compared to those of Marx and Freud, especially with respect to the prediction which was confirmed by the findings of Eddington's expedition. This was based on Einstein's gravitational theory, which theory led to the result that light must be attracted by heavy bodies, such as the sun, precisely as material bodies are attracted. Eddington's findings are well documented, and it will not be necessary to go into detail here (see Appendix A). The important feature, according to Popper, is the risk involved in a prediction of this kind. He writes:

If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible* results of observation--in fact with results which everybody before Einstein would have expected. This is quite different from the situation I have previously described, when it turned out that the theories in question were compatible with the most

divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories.<sup>8</sup>

Thus, to Popper, the criterion of the scientific status of a theory is its falsifiability (or refutability, or testability). It can be seen that Einstein's theory of gravitation clearly satisfies this criterion, for, even if our measuring instruments (then and now) were incapable of allowing us to arrive at a definite result, there was (is), nevertheless, a clear possibility of refuting the theory. However, if one now reconsiders the Marxist theory of history, especially in the perspective of, as mentioned before, the constant stream of daily confirmations, the following questions must seriously be considered: "Is historical materialism the statement of an established historical or sociological law? or is it simply an extremely wide-ranging and complex hypothesis liable to refutation as research advances?" Considering the whole perspective of Marx's works, it seems safe to say that historical materialism was a view that Marx was constantly trying to support but never to refute. For example, Marx's analysis of the character of the "coming social revolution" led to predictions that were testable and, in fact, falsified. However, in order to "save" the theory, various *ad hoc* modifications were required. As Popper himself says:

Yet instead of accepting the refutations the followers of Marx re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a "conventionalist twist" to the theory; and by this strategem they destroyed its much advertised claim to scientific status.<sup>9</sup>

However, in the case of Freud, the theory was simply not testable, and hence irrefutable. No conceivable human behaviour could contradict it. Again, to quote Popper (who also refers to Adler's individual psychology theory):

This does not mean that Freud and Adler were not seeing things correctly: I personally do not doubt that much of what they say is of considerable importance, and may well play its part one day in a psychological science which is testable. But it does not mean that these "clinical observations" which analysts naively believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice . . . These theories describe some facts, but in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form.<sup>10</sup>

Popper's approach can best be summarized in the following quotation from *Conjectures and Refutations*:

- "1) It is easy to obtain confirmations, or verifications, for nearly every theory--if one looks for confirmations.
- 2) Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, one should have expected an event which was incompatible with the theory--an event which would have refuted the theory.
- 3) Every "good" scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.
- 4) A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.
- 5) Every genuine *test* of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability; some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.
- 6) Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it

can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of "corroborating evidence.")

- 7) Some genuinely testable theories, when found to be false, are still upheld by their admirers--for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (I later described such a rescuing operation as a "conventionalist twist" or a "conventionalist stratagem.")

One can sum up all this by saying that *the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*"<sup>11</sup>

Finally, in keeping with Popper's notion of falsification, scientific explanation is not (as is customarily held) the reduction of the unknown to the known. In pure science, Popper holds, explanation is always the logical reduction of hypotheses to others which are of greater universality, incorporated in a higher level, riskier theory which is still to be tested. In this sense, scientific explanation is the reduction of the known to the unknown.

SECTION III (Note: This section is not essential for the main argument, and so may be omitted on a first reading. Please refer to Prefatory Note.)

*This section is a detailed investigation of the scientific research enterprise with special reference to Popper and to the concept of "discovery" (both of which have been introduced earlier). This is preparatory to the detailed treatment of Scientific Discovery to be taken up in Chapter Three. The full import of the analysis given in this section will be made more explicit in Chapter Five.*

What is a scientific hypothesis? Factual hypotheses and heuristic hypotheses. Scientific laws are descriptive rather than prescriptive. The logical structure of laws is that of empirical hypothetical universal affirmative statements.

Types of scientific laws:

- (a) numerical laws;
- (b) physical laws;
- (c) biological and behavioural laws;
- (d) historical laws.

Scientific laws have no time constraints and hence may be used for prediction. Laws are systematized within theories. The dilemma of the truth-value of lawlike statements, and the distinction between "law of nature" (Ln) and "law of science" (Ls). The realist position: a law of science is a postulate, and an object of warranted rational belief if and only if the corresponding law of nature is true. The nominalist position: universals do not exist; laws are only names denoting classes of invariances. The conceptualist position: laws are conceptualized in the mind as universals brought out by the order of things. Justification for support of the conceptualist position. The concept of discovery vis-à-vis hypotheses, laws of nature, and laws of science.

Theories always contain some unobserved entities which may be justified on certain criteria. Theories are deductive systems. The "inner structure" (abstract calculus) of theories. Models as logical extensions of theories. The "outer structure" of theories and experimental testing. Scientific theories and another look at the concept of discovery and Popper's criterion of falsifiability. Scientific theories and the ontological status of theoretical constructs. The realist position: theoretical constructs are warranted approximations to what is "really real." The constructivist position: theoretical entities are mental constructs that are justified on functional grounds. The position of this thesis: the pragmatist position: theoretical entities are "creatively" formulated but must be subject to rigorous test (reference to Popper) and are justified on "pragmatic" criteria. The value of a good theory. A return to Popper and the concept of discovery.

An examination of scientific explanation. The distinction between "giving reasons" and "giving explanations." Explanation of laws, and the explanatory and predictive power of theories. A return to the problem of ontological status of theoretical entities and the acceptability of theories. Newtonian

*mechanism as a model of scientific explanation. Critique of Newtonian mechanism. The Covering-Law Model and the Probabilistic Model of explanation. Teleological explanation: goals and functions. Functionalism. Teleological language can be translated to the nonteleological and can be subsumed under theories, such as Darwin's Theory of Evolution.*

A scientific hypothesis (as opposed to one which is common-sensical or metaphysical) is a proposition which, if true, can attain the status of a law. Furthermore, when it has attained lawlike status, it can, with other laws, form a theory, which theory can aid the quest for explanation, and prediction. Often an hypothesis may seem at first outlandish, but in fact, the degree of plausibility is only one consideration. More important (as will be discussed below) is its use within an explanatory system.

To emphasize the point that an hypothesis is a proposal to accept some statement  $x$  as true (thus bestowing upon the statement a tentativeness) compare the following sentences:

- (a) What if  $x$  were the case?
- (b)  $x$  is the case;
- (c) It can be demonstrated that  $x$  is the case.

Clearly, only sentence (a) is the expression of an hypothesis.

A distinction too can be made between two types of hypotheses both of which are found within the framework just delimited. There are those hypotheses which concern matters of fact ("factual hypotheses," such as the heliocentric hypothesis) which are testable, or at least,



testable in principle. Over a period of time, during which further experimental support is adduced, hypotheses tend to elaborate into full-fledged explanatory systems in themselves. That is to say, they become treated as theories, and this treatment supports the claim that these theories themselves can be reduced to nothing more than very general hypotheses. But this treatment of theory as hypothesis in some ways at least, is unsatisfactory, because scientists themselves do not treat such a system (e.g., heliocentric system) as a simple hypothesis but, indeed as a (tested) theory. Moreover, treatment of theory as hypothesis is circular, and it would be difficult to work in the notion of explanation if a propositional statement, of uncertain truth status, simply generated, in the end, nothing more than a subsequent, more general statement also of uncertain truth status. If one wishes to point out the uncertainty of a final truth status of a theory (which will be taken up later), then this can still be accomplished within the hypothetico-deductive system and Popperian notion of corroboration in which theories can always be modified, up-dated, drastically altered, or completely abandoned. In this account, the related notions of "explanation" and "search for truth" can be accommodated, and the researcher's purpose becomes one of focusing as closely as possible (at any one moment) on the search for truth. In the first account, the search for truth would be impossible, since, even after testing, one would have to accord the general statements (theoretical statements) by definition, as only a "yet-to-be-tested" truth, such that, logically speaking, one could not ever use, for example, the kinetic-molecular theory of gases

to explain the behaviour of gases. Thus strictly (and logically) speaking, one can only test this theory--and this is not only what is done, for indeed such a theory is used for explanation. In short, to say that a theory is like an hypothesis is meaningful, but to say that a theory is an hypothesis is not. Moreover, the non-treatment of theory as simply hypothesis is in no way inconsistent with the requirement that a good theory should be able to generate further hypotheses. Thus, the "factual" approach appears inadequate for a conception of the term "hypothesis."

The second approach to the treatment of hypotheses is to consider them as statements which simply express, in a heuristic manner, how the facts might profitably be conceived. Their status now would not be that of theory. But, in any case, no matter how hypotheses are treated, it is still not enough to give insight on how they originate. This is the problem of scientific discovery (special sense) which will be taken up in detail in Chapter Three.

A scientific law differs from a social or political law in that the former is descriptive rather than prescriptive. Although there are norms about how one ought to observe, as well as norms of valid inference, it would be odd to say that a law of science prescribes how nature ought to happen. For example, Kepler's laws of planetary motion describe rather than prescribe how the planets actually do move. Moreover, the laws of science are often descriptions of the behaviour of entities under ideal conditions. Charles' Law regarding the volume of a gas does not hold at extreme temperatures; similarly, Galileo's Law of Falling Bodies describes the velocity at which bodies fall in a vacuum. The logical

form of a scientific law (except numerical ones) is that of an empirical and hypothetical universal affirmative. Moreover, if such a statement "All  $A$  is  $B$ " is to count as a law,  $A$  and  $B$  must express not only an invariance, but also a contingent (not *a priori* or necessarily true) relationship. Thus, the universal proposition: "All iron rusts when exposed to oxygen," when stated as a scientific law, becomes: "If iron is exposed to oxygen, then it will rust."

A further problem arises concerning the structure of scientific laws. Consider the sentence: "If there are unicorns, then they are white," and although this, too, may be symbolized as a conditional universal affirmative  $(x) (Ux \supset Wx)$ , it is logically true even though both the antecedent and the consequent are false. Now reconsider the statement: "If (all) iron is exposed to oxygen, then it will rust." Here too, the compound proposition will be logically true even though both component propositions could be false. That is  $(x) (Ix \supset Rx)$ , where " $I$  = iron" and " $R$  = will rust" will be true even if  $Ix$  and  $Rx$  are both false.

What, then makes  $(x) (Ix \supset Rx)$  a scientific law, while  $(x) (Ux \supset Wx)$  is not? The difference lies in the fact, that, in the case of scientific laws, there is evidence from other laws that these laws are true. However, the proposition about unicorns is connected with no other laws whatsoever, and can, moreover, be easily reduced to an analytic statement (see Appendix B).

Several types of scientific laws may be distinguished. A numerical law is a universal statement of invariant relationship among numerals, and which consists of individual variables, constants, and

some arithmetical connectives (such as "+," "-", and so on). In a purely formal uninterpreted system, such laws are factually empty, although it is always possible to interpret the relationship.

Consider the law represented by the formula:

$$y = \frac{x^2 - x}{2}$$

which is an explicit expression of the general functional relationship (mathematical law):

$$y = f(x).$$

Substitution of ordinals  $x=1$ ,  $x=2$ , etc. yields the following values of  $y$ :

$$y = 0$$

$$y = 1$$

$$y = 3$$

$$y = 6$$

$$y = 10, \text{ etc.}$$

where the difference between each successive term and the one before it also yields a series, namely:

$$y' = 1$$

$$y' = 2$$

$$y' = 3$$

$$y' = 4, \text{ etc.}$$

Now (following Wartofsky's example)<sup>12</sup> one or both of the series may be interpreted. For example, in the "y" series, the value of "x" may designate the number of points, and the associated "y" value would designate the number of lines connecting the points given in ordinal sequence, assuming that there is one and only one line between any two points.

Then,

if $x = 1$	then $y = 0$	yielding co-ordinates (1,0)
$= 2$	$= 1$	(2,1)
$= 3$	$= 3$	(3,3)
$= 4$	$= 6$	(4,6)

and these may be interpreted geometrically as:



The numerical law can be considered as a means of "prediction" of the number of lines to be associated with a given number of points. Similarly, the number of lines may be said to have been "explained" by the generator formula. But the most important fact is that there is a complete isomorphism between the algebraic and the geometric systems; the one is the transform of the other.

As already touched upon earlier, the form of such laws is always such that for every value of some independent variable (x) there is associated some value of a dependent variable (y) according to an invariant relationship as expressed by the law. Such laws may be

interpreted into another formal system (as shown above: from algebra to geometry) or they may be given a physical interpretation in which the use of numbers is connected with the number properties of some physical magnitude. This could then lead to some physically interpretable hypotheses which may then be tested to see whether the deductive consequences agree with empirical measurements. It should be noted that now the concept of "physical law" is being treated. Moreover, whereas it is awkward to talk of "explanation" and "prediction" within a purely formal, uninterpreted calculus, these terms do accord with their ordinary significance when applied to an empirically interpreted calculus (i.e., physical law).

The form of numerical laws yields the form of physical laws, except in the latter the numerals are taken to represent number properties of physical magnitudes. Such properties would be, for example, length, charge, mass, and weight. Physical laws, then, will be given the immense powers of deductive inference as well as that of mathematical computation. Consider Galileo's Law of Free Falling Bodies. The hypothetico-deductive system generates the numerical Law:

$$s = \frac{1}{2} gt^2$$

where s = distance

t = time

g = gravitational constant.

This numerical law is an interpretation of the physical magnitudes distance and time, and the law can then be tested, as a physical law, by further experimentation.

In general, biological and behavioural laws are not as readily expressed in quantifiable forms, although in some areas, such as genetics and ecology, numerical formulations are becoming increasingly frequent. Indeed, such quantification may give an indication of the nature of a "complete" biology, sociology, or psychology (to select a few examples).

Many explanations in biology are answers to questions such as "What is X for?" or "Why does X function as it does?" It appears then, that such explanations will be teleological in nature, despite the fact that biologists and philosophers hold that biology is fundamentally non-teleological (more will be said covering the nature of teleological explanation later in this section). However, for present purposes it will suffice to say that, in terms of the formulation of laws, such a statement as: "The function of the heart is to pump blood throughout the organism" can be reduced to: "If anything is a heart, then its function is to pump blood throughout the organism." This is again the form of an hypothetical universal affirmative, and can be supported by experimental evidence. That is, one could formulate an hypothesis that such is the function of the heart and then try to falsify it by, for example, trying to show that its function is that of producing insulin. After numerous other attempts it may be corroborated (in Popperian language) that pumping of blood is indeed the function of the heart. In this way, then, the fundamental logical structure of laws is maintained, but whether such laws can be incorporated into a systematic theory, and hence justify in more important terms its being called a law, is a further question that shall not be investigated at this time.

Finally, there are those social scientists who maintain that history also exemplifies laws. In its strongest form this position holds that historical laws are exactly like those of nature. Thus, a scientific study of history is no different, in principle, from the scientific study of natural phenomena. Some of the arguments adduced both for and against this claim will be briefly surveyed, but it must be admitted that, unlike the forms previously discussed, the present status of historical laws seems open to much greater critical discussion.

Consider the proposition expressed as the aphorism: "History never repeats itself." This can be reformulated in standard language as: "If this is an event in history, then it will never be repeated." But is this reformulation enough to confer lawlike status? Those who would judge negatively would do so on the grounds of the uniqueness of human events as opposed to the non-uniqueness of natural ones. To this argument the proponents may concede that, although individual agents may be unique, it can be claimed that the relationships among them display lawlike invariances. An example often used in that of Marx's "stages" of historical development, in which it is held that in Western industrial societies, at least, there will always be the following pattern exhibited: slavery, feudalism, and capitalism. (The prediction is that the latter will give rise to socialism.) This position, emphasizing the lawlike nature of relationships, is often attacked as commonsensical, vague, and untestable, to which the reply is that all scientific laws, to some extent at least, are also liable to this charge. This seems valid, at least in part, for such a law as Boyle's Law (volume of a gas varies indirectly with pressure) does, *prima facie* seem obvious, does contain



"vague" terms, such as "molecular speed," "collision," and at extreme pressures cannot be tested, or if so, cannot be accepted without modification. That is, the law holds strictly true only within a certain range of pressures.

Further arguments raised against the existence of historical laws concern conceptual frameworks for, it is claimed, historical facts are always interpreted from some point of view so that facts can always be made to "fit" the framework. But it should be realized that laws in physical science also are viewed within conceptual frameworks. Finally, it is claimed that as human history concerns the action of free-willed agents, such actions, if free, will then be unpredictable and hence unexpressible in lawlike terms. (The notion of the predictability of laws follows immediately.) If no predictive powers are available, then no claim to lawlike status can be made--and this in spite of much statistical support. The counterclaim, quite true, is that, at the quantum level, events, though unpredictable, nevertheless do yield statistical "trends," and, for large "samples" do produce probability functions for a "run" of unique events. If such a lack of precise predictability is legitimate in physics, why not in history? These further problems will not be dealt with here, but will be touched upon again in Chapter Five. For the scope of this thesis, so ends the background material on the forms of scientific laws, or of those laws which aspire to scientific status.

The notion of predictability has already been raised concerning laws. As laws of nature have no time restriction, the formulation of

such laws (of science) implies a claim that the law (of nature) extends into the future. This may be considered the most important single feature of laws, for it enables them to be made the basis for prediction. Moreover, a proposition is not accorded the status of law unless there is some direct evidence for it. Laws are not viewed isolated from each other; they are, in fact, ordered into a system (theory) or systems such that each will mutually reinforce the other. The laws that scientists most dislike to abandon are those which are central to the system, for abandonment of any central law will usually require the abandonment, or at least alteration, of a large number of other laws in the system. Thus, in a "strong" system, an observation that directly corroborates one law indirectly corroborates a group of other laws because of this systemic, interrelational structure. On the other hand, a true hypothetical universal affirmative proposition not supported by evidence would have little fundamental value (or perhaps no value at all) in science, for it could be easily abandoned without any effect on the rest of the system. But consider a counter-instance of "All metals are good heat conductors" (reformulated as "If this is a metal, then it is a good heat conductor"). Such a counter-instance would have far reaching consequences for all of physics and chemistry.

So far, the detailed discussion of the concept of "scientific law" has not led to any major conceptual problems, but such problems arise when one tries to distinguish amongst three alternative philosophical positions on the nature of scientific laws. These difficulties will also have some implications for the concept of scientific discovery.

It has already been said that, for a statement  $S$  to be considered a law, it must exemplify the following form:

$$S = (x) (Fx \supset Gx).$$

Now, it is not possible to know if  $S$  is a law unless its truth-value is known, and this cannot be known unless every circumstance to which  $S$  applies is considered, and its truth-value ascertained. In very restricted cases, this is possible, but for scientific laws it is very often not possible. One way out would be to consider  $S$  as the foundation of an hypothesis which if true in all possible worlds, would be a law;  $S$  would then be best described as a "lawlike statement."

But, if  $S$  is "lawlike," it is, in the last analysis, not a law;  $S$ 's being like a law is not equivalent to its being a law. The problem, then, appears unresolved, for it is expected that a law can go beyond the presently available evidence in order to be used in prediction. Yet, in this way, one is forced to sacrifice the ability to ascertain its truth-value, for at any one moment it is not possible to determine the future truth-value of the statement. In short, if a law is true, then it cannot be known, and if a hypothetical universal affirmative proposition is true for all of its instances then it is not a law.

However, if a distinction is made between a "law of nature" ( $Ln$ ) and a "law of science" ( $Ls$ ) then the problem can be at least partially solved. Consider the case of  $s = \frac{1}{2} gt^2$ . This may be considered a formulated law of science which stands for a law of nature. It did not become a law of nature because Galileo formulated it, for it is true now,

was true in the past, and presumably will be true in the future. Using this distinction allows sense to be made of the notion of scientific discovery, for the invariance would then hold true whether it is known or not.

But what about the time before there were any human beings? The statement can be formulated as  $S'$  below:

$S' =$  If anyone were to observe a falling body prior to the appearance of human beings, then the body would have accelerated according to the relationship as expressed in  $s = \frac{1}{2} gt^2$ .

This statement  $S'$  is a contrary-to-fact conditional and epistemologically impossible. This may be solved by transforming the epistemological assertion  $S$  into the ontological assertion  $S''$ :

$S'' =$  If there were a falling body, then it would have accelerated according to the relationship as expressed in  $s = \frac{1}{2} gt^2$ .

This is a subjunctive-conditional sentence.

Now that the distinction between a law of nature ( $Ln$ ) and a law of science ( $LS$ ) has been drawn, the realist position on laws may be presented. Briefly, this doctrine holds that a law of science is an hypothesis (or postulate) which is an object of rational belief on the basis of sound evidence, such that if a law of science is true, then it states a law of nature. In short:

$LS$  is true if and only if  $Ln$  is true.

Herein lies the fallability of laws, for  $L_n$  is "objectively" true, whereas  $L_s$  is the expression of a proposition which constantly approximates  $L_n$  as alternative hypotheses are falsified. On the criterion of truth-values, then, the realist holds that only laws of nature are the "really real." This position harkens back to Plato's universals--his theory of Forms and "two-world view" of the universe.

Unlike realism, nominalism is not so much concerned with truth-values. Laws have only the status of names which do not have reference to some universal "real." Laws, then, refer to classes of invariances or collections of particular instances. In this way they may also be seen as pragmatic "instruments" more or less adequate as a means for dealing with nature, especially in terms of economy and/or efficiency. Formulating the law  $s = \frac{1}{2}gt^2$  frees one from having to test the acceleration of a body individually, or from remembering what the individual accelerations for each body is. This would be impossible. But expressed as a law of science, one need only remember the formula, and the acceleration is simple to compute each time as required. Although the problem of the truth of the  $L_s$  is not solved, its use is justified on pragmatic grounds.

The third position, called the conceptualist view, holds that if one is to talk of universals at all, then they must be conceived of as universals constructed or conceptualized in the mind as the order brought out by inquiry; universals do not have an independent existence as ideal forms. Laws of science are the forms (or formulation) in which laws of nature become objects of reason, or of conceptual judgment, and their truth-values are determined by the manner (or to the degree) which

they do or do not adequately represent the lawful invariants in nature. Unlike nominalism, conceptualism does not hold that laws of science are just mere conventions for efficiency, economy, and so on. Consider the "alternative" laws of science:

$$Ls \quad s = \frac{1}{2} gt^2$$

$$Ls' \quad (AD|AE) = (HM)^2 | (HL)^2$$

$Ls$  and  $Ls'$  represent the same  $Ln$ ; they are alternative conventions of expressing the same proposition; but the proposition itself is not just a convention.

The thesis has presented but the briefest sketch of these three doctrines, and has not even attempted to give the various "subdivisions" within each. Much discussion is still being carried out, especially between the first two stands. For the purposes of the present thesis, the view supported will be the conceptualist on the following grounds:

- (a) it seems to be the manner in which scientists themselves conceive of laws. Scientists are concerned with objects of belief that are supported by evidence, and are not normally concerned with the more ephemeral ascription of ontological status incorporated in the realist position;
- (b) the example shown above ( $Ls$  and  $Ls'$ ) tends to put the nominalist view in dimmer light. The neat distinction is "laws as conventions" and "alternate conventions of laws." No one would deny

that laws of science are formulated to make one's life easier. But one should not conflate "laws as conventions" with "conventions of expressing laws"; in this way one avoids confusing the two conventions ( $Ls$  and  $Ls'$ ) as two separate laws;

- (c) the conceptualist view most closely accords with scientific research methodology, for the scientist does look for a common "theme" which is conceptualized as a law and hence possesses at least rudimentary explanatory/predictive power. (More will be said about explanation and prediction very shortly.) According to the nominalist view the law is a convenient summary of facts (classes of instances), but this does not accord with research procedure. Classification is one thing; deriving the common theme, formulating and explicating it, is one step (or more) beyond. To a scientist,  $s = \frac{1}{2}gt^2$  is more than just the collection of all instances of acceleration of falling bodies;
- (d) any talk of universals initiates problems of justification which often tend to become metaphysical. That is, there is no way of empirically verifying its truth value. (Empirical verifications, of course, are not required of propositions which are necessarily true; but laws of science are not these types of propositions, excepting mathematics and logic.) Not only are universals difficult to justify, they fail to accord with the scientists' conception of laws;
- (e) the conceptualist view most closely fits the ideas and arguments of the thesis as brought out in Chapters Three through Five.

The full import of this justification must necessarily be deferred for the present.

One further distinction needs to be drawn, lest confusion result. The term "discovery" as used in this thesis refers to the formulation of an hypothesis and thence of a law of science. This is not to be confused with the second sense of the term, as discussed in the immediately preceding section; that is, the sense in which laws of nature are discovered. And for those who wish to associate "discovery" only with "laws" it should be noted that only a thin line separates the concept of "a (fruitful) hypothesis" (which is what the scientist is interested in) and a corroborated "law." The form and logical structure of the two are the same, but a law of science has to be successfully tested. This is in no way inconsistent with the concept of "discovery" as applied to a law of nature. However, it is with the usage as associated with hypothesis that the term will be treated in this thesis.

While it may be said that laws are discovered, it may be said that theories are constructed. A scientific theory always contains some term (representing a concept) which denotes something that cannot be observed directly. This is called a theoretical entity, and when it is part of a statement, one obtains a theoretical statement. Such a description applies to the proposition: "Protons and neutrons are component parts of atoms." Both entities cannot be observed, but they are assumed to exist in order that many different observations may be explained. In short, the unobserved is invoked to explain the observed. A theory, then, is



not just a summary of observed facts, for such a "system" would have no explanatory power. It must contain concepts from which new facts may be deduced. The scientific potency of a theory is directly proportional to the quantity and range of facts it explains, particularly those that were unknown before the theory was constructed. It is now time to investigate more closely the concept of "theory" and its construction.

Rudner defines "theory" as "a systematically related set of statements, including some lawlike generalizations, that is empirically testable."<sup>13</sup> He goes on to add that a scientific theory is a deductive system, which feature is the basis of its systematic relatedness. Thus, if it can be shown that one statement is logically connected with a number of others, additional support is obtained in the sense that the evidence for all the constituent statements is "pooled" and can be drawn upon in support of any given statement.

The "inner structure" of theories will now be considered. (The "inner structure" is concerned with the construction and over-all, formal structure of theories, while the "outer structure" is concerned with testing of the system.) According to Nagel, in the initial construction of a theory, there are (or should be) three major components. First, there is an abstract calculus, or "logical skeleton," or explanatory system, that implicitly defines the basic notions of the system ("implicitly defined" means "those things that satisfy the conditions stated in the postulates"). Next, there is a set of rules that assigns an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment. Finally, there is an

interpretation or model which supplies some "flesh" to the skeletal structure in terms of association with somewhat more familiar conceptual or visualizable materials. If one accepts the assumption that every expression employed in constructing a model is in some sense "meaningful," then a theory, provided with an appropriate model, can be considered completely interpreted. This explanation-understanding is one of the "natural homes" of models. Another natural home for models is in the context of discovery, where it will be later shown that they can play a powerful heuristic role. However, the immediate task here is to investigate more closely the notion of "logical skeleton," (abstract calculus).

Theories are composed of both logical and non-logical terms. The non-logical terms can always be disassociated from the concepts to which they refer. Now, in philosophical language, "sense," "connotation" and "intention" are treated as roughly equivalent; so too are "reference," "denotation," and "extension." With respect to non-logical terms, it may be held that they are, or are treated as, both intension-free and extension-free, such that they should refer neither to the concept of the non-logical term (e.g., the concept of "table") nor to the object designated (e.g., the object called "table"). Thus, these non-logical terms are cognitively "neutral" simply by ignoring the concepts and images associated with the particular term, and attention can now be focused exclusively on the logical relations of the terms. In fact, one may go one step further and replace all the non-logical terms with neutral symbols, as shown by the following syllogism:

All men are mortal  
 Socrates is a man  
 Therefore, Socrates is mortal.

This syllogism may be symbolized by replacing the non-logical terms with symbols:

All x are y  
 S is an x  
 .  
 . . S is y.

The focus now is on logical relations independent of any connotative and denotative elements, and yet the argument in symbolic form is just as valid as the non-symbolic. Indeed, it may be argued that the symbolic form is superior in that it is more general.

However, not all calculi are as easy as the example just cited. For more difficult ones, it is necessary to specify exactly and in detail, what sorts of inferences can be legitimately made. These rules of inference would then allow the transformation of a set of premises into a conclusion with the guarantee that, if the premises are true, then the conclusion cannot be false. Thus, when a theory is so carefully codified by these symbols and inference rules, then it acquires the form of a deductive system, and the fundamental assumptions of the theory formulate nothing but an abstract relational structure (calculus), or "logical skeleton."

It is opportune now to touch briefly on the heuristic role of models, when models are considered as logical extensions of theories, for

if the non-logical words (i.e., words other than "All," "are," "is") are now substituted by meaningful words, a model is obtained. If the symbolic expression: "All x are y" were to have the unknowns substituted, some examples of the resultant statements would be: "All men are mortal," "All Athenians are wise," "All swans are white," and these could be treated as alternative models of the same underlying calculus. A model then, seems intermediate between theory and experimentation. The theory is a set of abstract relationships, whereas the model is a set of statements based on, and derived from, this abstract relationship, and operates in conjunction with a lexicon of precise terms, transformational rules for the employment of those terms when changing from abstract to substantive statements, and syntax rules. In addition, models are heuristic, as when, for example, "All x are y," specifying an abstract relationship is transformed into "All swans are white" one is led to discover, or attempt to discover, whether this statement is factually true.

The "outer structure" of a theory is concerned with the testing of the system by experimentation, and checking the postulates against the *a posteriori* results. Viewed in this light, theory is, then very much like an hypothesis. When the observations of facts do not agree with theory--that is, when they do not make sense within the frame of theory which is being utilized in carrying out the research--then the theory must be discarded, to be replaced by one which, hopefully, more closely correlates with these facts.

In recapitulation, then, the general features of theory construction include the following:

- 1) the formulation of hypotheses;
- 2) the deduction of testable consequences;
- 3) the elimination of hypotheses whose consequences do not withstand testing.

However, there appears to be little or no agreement as to the initial formulation of hypotheses; that is to say, how hypotheses are arrived at within the context of discovery. This is a central problem in the philosophy of science, and more will be said of it as the thesis develops. At this point it will only be necessary to touch upon it as preparation for subsequent development.

Nagel holds that a scientific theory (generated from an hypothesis) is often suggested by materials of familiar experience or by certain features noted in other theories.<sup>14</sup> Furtado claims that a scientific social theory presupposes the existence of problems whose solution is a matter of concern to some social group.<sup>15</sup> Thus, it follows that it is essential to recognize the existence of a problem in order that a solution may become the object of inquiry, even though the recognition of the existence of a problem is not always easy. Popper, however, claims that hypothesis formulation is not associated with inductive generalizations in the sense of deriving theories from observations.

As was discussed before, according to Popper, the most important aspect of theories is contained in the question: "How did one test the theory?" and not in: "How did one discover the theory?" Testing, then, becomes central to the notion of theory, for it can lead to new

observations. It is an attempt to weed out false theories, to find the weak points of a theory in order to reject it if it is invalidated by the test. In fact, for the very reason one aims to establish theories as well as possible, one must test them as severely as possible. That is, one must try to find fault with them, one must try to falsify one's own theories. If, in spite of one's best efforts, the theory appears unfalsifiable, then one can say only that it is highly corroborated, that it has stood up to severe tests. For this reason, the discovery of instances which confirm a theory means little if one has not tried, and failed, to discover refutations. The main point, then, is that if one is uncritical one shall always find what one wants. Testing that is designed to confirm hypotheses neither advances our knowledge nor generates new problems.

At the beginning of this discussion on theories it was stated that laws are discovered, but theories are constructed. This is perhaps somewhat too neat, for the term "discovery" can also be applied to theories, albeit indirectly through the constructs as first formulated in hypotheses, thence to law, and, finally, to become incorporated in the structure of theory. It can be seen that the "path" from hypothesis to theory is like a continuum. After successful testing, a concept introduced in an hypothesis ("hypothetical construct") becomes, within a theory, a "theoretical entity" (theoretical construct). This brings up one of the central problems in the philosophy of science (and one that is closely bound with the problem of discovery): viz., the ontological status of these concepts, for, as mentioned before, these terms denote the

unobservable. (This is preparatory to Chapters Three and Five where this problem will be touched upon again. See Appendices C and D.)

Realism holds that these theoretical entities are warranted approximations to reality as tested not only by their empirical adequacy, but also by their systematicity within the theory and with other theories. On the other hand, constructivism holds that these theoretical entities are imaginary or ideal constructs which are known not to be true, but which function to aid the imagination, or function as economical representations for the purpose of theory formation. Both positions present many problems. The realist view raises serious questions of empiricist and rationalist criteria of truth, and the latter constructivist position presents the awkward consequence of having false explanations for laws which are presumed to be true (since, to repeat, in constructivism theoretical entities are known not to be true). This thesis will present a slightly different view, perhaps best labelled as "pragmatic." Theoretical constructs may or may not approximate reality, but hopefully they do. Their employment can be justified on criteria of adequacy (rather than truth-value); as explanatory and predictive devices; on their "fit" within the theory and amongst other theories; and their simplicity. In this way they may be freely created since they will be subjected to rigorous empirical testing (i.e., Popper's criterion) and, hence, a testing of their value according to these criteria. Moreover, such an approach seems to accord with the way scientists treat these concepts, and also accords with the main argument of this thesis.

The value of a good theory lies not only in the answers it gives,

but also in the new questions it raises, leading, hopefully, to new theoretical formulations; for, if upon testing an hypothesis one finds that it does not accord with the facts, then it must be modified, and hence a "new" hypothesis, is generated. (The "new" hypothesis, of course, may require only slight modifications, or, perhaps a completely new, or almost completely new hypothesis is required.) Also, in testing an hypothesis, one may perhaps come across material or data that suggest other lines of inquiry such that, in order to investigate this material, "new" hypotheses again are generated. Furthermore, as noted before, a theory is a systematically related set of statements, with each statement gaining support from the others. In the same way, the value of a theory is enhanced by its inclusion in a set of theories, just as a theory is of more value than one theoretical statement. Furthermore, as any theory is used, its meaning becomes progressively more fixed, although, as Kaplan<sup>16</sup> argues, some degree of "open-ness" remains. Also, it is possible that some theories, such as, perhaps, the social theory of Marx, may be turned into dogmas, such that it may not be susceptible to change by any but the most drastic means. Yet it would indeed seem odd, not to mention unscientific, to try to retain some theory at the expense of an unwarrantly large number of *ad hoc* hypotheses, let alone for purely dogmatic or ideological reasons.

And this brings the discussion to a return to Popper, and his important criterion of falsifiability (or testability), although he may perhaps be accused or unwarrantly de-emphasizing the role played by experience (especially in regard to discovery) in the total scientific



enterprise. This is Blackwell's point when he argues that the first stage of discovery, "discovery that," is an elaboration of experience, of an interaction between matter and mind. This will be discussed in the next chapter. Yet Popper, in terms of specifying a criterion (that is, falsifiability), seems correct in arguing that testing, with a critical and constructive attitude toward errors, is of extreme importance. He says:

Our propensity to look out for regularities, and to impose laws on nature, leads to the psychological phenomenon of dogmatic thinking or, more generally dogmatic behaviour: we expect regularities everywhere and attempt to find them even when there are none; . . .<sup>17</sup>

He goes on to say

. . . For the dogmatic attitude is clearly related to the tendency to verify our laws and schemata by seeking to apply them and to confirm them, even to the point of neglecting refutations, whereas the critical attitude is one of readiness to change them--to test them; to refute them; to falsify them, if possible. This suggests that we may identify the critical attitude with the scientific attitude, and the dogmatic attitude with one which we have described as pseudo-scientific . . .

The critical attitude, the tradition of free discussion of theories with the aim of discovering their weak spots so that they may be improved upon, is the attitude of reasonableness, of rationality . . . The Greeks' discovery of the critical method gave rise at first to the mistaken hope that it would lead to the solution of all the great, old problems . . . that it would help to prove our theories, to justify them. But this hope was a residue of the dogmatic way of thinking; in fact nothing can be justified or proved (outside of mathematics and logic). The demand for rational proofs in science indicates a failure to keep distinct the broad realm of rationality and the narrow realm of rational certainty: it is untenable, an unreasonable demand.<sup>18</sup>

Popper then goes on to point out (as this thesis will support) a most crucial point: that theories in science are provisional, and subject to modification and revision (if not replacement).

As mentioned earlier (see "SR models") the great advantage of theories is that they possess great explanatory power. But one must be careful to delineate what "explanation" is. This will be the purpose of the following discussion.

A request for an answer to the word "Why" . . .? may or may not result in an explanation, for one may simply receive nothing but a number of reasons. In general, the reasons for  $p$  are simply those propositions which, if true, make belief in  $p$  more plausible. The giving of reasons, then, is most closely associated with beliefs. Explanation, on the other hand is more closely associated with events, processes, phenomena, or happenings (e.g., "Why does iron rust?"). Furthermore, in scientific explanations, it is often not particular events that are in want of explanation, but, rather, the laws themselves, and this is accomplished by the giving of other laws and theories. As Hospers says:

Both theories and laws are normally involved in explaining laws; certainly we cannot go far in explaining laws without reference to a theory.<sup>19</sup>

Consider the question: "Why does this wire conduct electricity?" Also, consider the answer: "All copper conducts electricity," which reply may be reduced to two further sentences: "This wire is made of copper" (a particular fact) and "Copper is a conductor of electricity" (a law), and this latter law may be transformed into standard form "If this is a sample of copper, then it conducts electricity." This provides an explanation of the immediate phenomenon, but it may still be asked: "Why does copper conduct electricity?" This, of course is a request for

an explanation of a law. In this case, physical theory (theories) must be resorted to, specifically the theory of electricity and the theory of the crystal structure of metal. Moreover, this theory must be one that is already accepted.

But how does one determine the "acceptability status" of a theory? First of all, it should be noted that an explanation that explains everything in fact explains nothing. According to Popper, the more that one excludes by falsification, the more real knowledge one has at his disposal. Thus, if one "explains" the expansion of water upon freezing by saying "God wills it," one has not explained anything at all. The status of an explanation may be determined by how much it goes beyond the immediate event (or law) to be explained. For example, does theory *X* also explain other events or laws as well, and does it also explain some events or laws whose existence was not known or even suspected prior to the formulation of theory *X*? Another test of the value of a theory is its ability to predict. In the present example, knowing that water expands on freezing allows one to predict the bursting of water pipes, and hence control measures can then be taken.

But one may wish to argue that although geologists know the "laws of earthquakes" they are singularly inept at predicting (let alone controlling) new ones. This calls into question the criterion of prediction for determining the status of theories. However, this objection does not in fact upset the criterion, for it is not just the laws that are involved in explanation, but these laws in combination with initial conditions. It is not the laws that are at fault (i.e., have no predictive

power) but that so little is known of these initial conditions. In contrast, consider Newton's Law of Universal Gravitation which has enormous explanatory and predictive powers. It can explain the falling of an apple as well as the motion of the planets; and it can also predict such diverse phenomena as the eclipse of the sun and the evolution of galaxies into a spatial structure. This brings up again the question of ontological status for, in considering why a piece of copper expands when heated, recourse was made to a theory that included such terms as electrons, protons, and so on. Why could not a gremlin have been employed in such an explanation? It is, like the electron, also unobservable.

But scientists hold onto this theory because these theoretical entities do have great explanatory and predictive power. From the belief in electrons many consequences can be deduced such that their occurrences, which are observable, tend to support (i.e., corroborate) the theory. But from the gremlin theory, no further consequences can be deduced.

So ends the overview of scientific explanation. The thesis will now concern itself with some important forms of explanation.

As mentioned in Chapter One, the modern scientific outlook did not begin until the 17th century. It was also noted that from the latter half of that century until the 19th century, much of the thought of western culture (scientific and otherwise) was influenced by the deterministic framework of Newton in whose conceptual scheme all material events were held to be grounded in mathematical and logical rules. This particular form of determinism may be labelled "mechanism," and in the context of explanation, "mechanical explanation." Today, however, philosophers

use this term only to refer to explanations within the context of one type of physical theory described as mechanical, namely, Newtonian mechanics. Since this explanation "type" has exerted such profound influence, the thesis will consider it in some detail before going on to discuss the main "type," namely the Deductive or Covering Law Model of Explanation.

Newton's three axioms or laws of motion, as presented in his *Philosophae Naturalis Principia Mathematica* (1687) are:

- I. Every body continues in its state of rest, or of uniform motion in a right (that is, straight) line, unless it is compelled to change that state by forces impressed upon it.
- II. The change of motion is proportional to the motive force impressed; and it is made in the direction of the right line in which that force is impressed.
- III. To every action there is always an equal reaction; or, the mutual actions of the bodies upon each other are always equal, and directed to contrary parts.

Now, in accordance with Newton's Definition II, the word "motion" may be replaced by "momentum," which is mass multiplied by velocity. The second law is usually expressed mathematically as:

$$F = m \cdot a \text{ (where } F = \text{force; } m = \text{mass;}$$

$$a = \text{acceleration)}$$

or, in differential calculus as:

$$F = m \frac{dv}{dt}$$

(that is, "force" is equal to mass multiplied by the rate of change of velocity with respect to time).

Physical systems in Newtonian mechanics are treated as "masses" in motion (changing with respect to time), affecting each other by "forces," which in turn are specified in terms of the spatio-temporal relations of masses and certain constants associated with those masses. Very simply, then, these three laws and their consequences specify the behaviour of such masses, motions, and forces.

To the eighteenth and much of the nineteenth centuries, Newton became idealized as the perfect scientist: cool, objective, never venturing beyond what the facts warrant to speculative hypotheses. *Principia* (1687) became the model of scientific knowledge, and the expression of the Enlightenment conception of the universe as a rationally ordered machine governed by simple mathematical laws. Mechanics came to be regarded as the ultimate explanatory science, and it was believed that phenomena of any kind could and should be explained in terms of mechanical conceptions. In fact, mechanism became equated with determinism, and the Newton world picture came to be thought of as a picture of a Newtonian world machine.

There have been many criticism against Newtonian mechanics, and to consider all of them to date would be to go far beyond the scope of this thesis. One of the main difficulties stemmed from the Cartesian notion of "mechanics" which insisted that the forces involved in any ultimately valid mechanical explanation must be contact forces, as action at a distance was considered impossible. Newton himself tended to agree

with his critics that his theory was incomplete in this respect, such that gravitation, being a force acting at a distance, required a "deeper" mechanical explanation. Furthermore, there is still some question as to how much influence the work of Galileo actually had on Newton, and it has been claimed that the laws of motion, in fact, were derived rather from Cartesian physics via the principle of the conservation of momentum. To this day, controversy still remains regarding the kind and degree of justification, if any, for accepting the Newtonian laws of motion; the relations between them; the role they play in mechanics; the interpretation of the fundamental terms occurring in them; the presuppositions, if any, of these laws; and whether these laws and the terms occurring in them are fundamental in mechanics, or, indeed, in all science.

The nineteenth century also witnessed growing dissatisfaction with the foundations of mechanics. Critiques such as those put forth by Mach, Pearson, Stallo, and Poincaré developed formulations of mechanics that were claimed to be epistemologically more satisfying than Newton's. However, these alternative versions, though equivalent mathematically to Newton's, may not also be equivalent in all other important respects. In short, controversies still remain regarding the fundamental concepts and propositions of Newtonian mechanics, and also between that and other versions of classical mechanics, and between classical mechanics and other types of mechanical theories. Such explanation, then, becomes hard to justify as a model for all disciplines, social science included.

It is now appropriate to examine a model of explanation that seems

adequate for explanations in the physical sciences, namely, the Covering-Law Model. The fundamental idea of this analysis of explanation is the view that the occurrence of an event is explained when it is subsumed under, or covered by, a law of nature; that is, when it (the event) is shown to have occurred in accordance with some general regularity of nature. Its logical form is:

$$\begin{array}{rcl} L_1, \dots, L_m & & \text{The explanans} \\ \hline C_1, \dots, C_n & & \\ \hline E & & \text{The explanandum} \end{array}$$

The explanans consists of two sets of premises: (1) a set of singular statements,  $C_1, \dots, C_n$ , describing relevant initial conditions, and (2) a set of general laws,  $L_1, \dots, L_m$ . The explanandum statement,  $E$ , which describes the phenomenon to be explained, is logically deduced from the explanans. The phenomenon to be explained is called the explanandum event. Moreover, the derivation of  $E$  from the  $C$ 's and  $L$ 's may involve principles of higher mathematics as well as the usual rules of logic. This, essentially, is the deductive model. A deductive explanation is sometimes called a causal explanation, such that the conditions referred to by the singular premises,  $C$ 's of the explanans, may jointly be called a cause of the explanandum event.

The Probabilistic Model is akin to the Covering-Law-Model. A simple statistical law takes the following form: "The statistical probability (roughly, the long-run relative frequency) of the occurrence of an event of kind  $G$  under the conditions of kind  $F$  is  $r$ . This may be



more briefly stated as  $p(G,F) = r$  where  $0 \leq r \leq 1$ . When the value,  $r$ , of statistical probability is close to 1, such a law can be used to explain why an event of kind  $G$  occurred on the basis of the additional information that an event of kind  $F$  occurred. The logical form can be expressed as:

$$\begin{array}{ccc}
 p(G,F) = r & & p(G,F) = r \\
 \hline
 b \text{ is an } F & \text{or more briefly} & \hline
 b \text{ is a } G & & Gb
 \end{array}$$

where  $b$  is some individual case. An example from physics would be, say, the emergence of a muon-neutrino pair, explained by pointing out that a pion decayed and that an overwhelming percentage (say, 99.986 per cent) of pion decays result in muon-neutrino pairs.

The explanatory power of such a probabilistic model is weaker than that effected by a deductive explanation because the explanans of a probabilistic explanation does not logically imply the explanandum; the former only gives a more or less high degree of inductive support or confirmation to the latter. Furthermore, to meet the requirement of "total evidence," the explanans must include all the available information that is inductively relevant to the explanandum.

An important corollary of the Covering Law Model is the thesis that explanation and prediction share identical logical structures, and this is due to the fact that the explanation is an argument in logical form. If the premises are known, there is sufficient warrant, either deductive or inductive, for the assertion of the explanandum as a prediction.

In fact, the symmetry is much greater than at first glance, for it may occur between explanations and projective arguments in general, whether predictive or retrodictive, for if  $E$  is an explanatory argument explaining an event  $e$ , then  $E$  can be used to predict  $e$  before the occurrence of  $e$  (prediction), to retrodict  $e$  after it, and to infer the occurrence of  $e$  simultaneously with it. For the actual use of an explanation as a prediction, its premises may have to be ascertained by independent predictive or retrodictive arguments, which may be practically impossible, but this does not detract from the generality of the thesis.

There have been many criticisms raised against this model which criticisms again go far beyond the scope of this thesis. The justification for its priority is that, if the distinction between law of nature and law of science is kept in mind, then the explanandum is logically valid. (Problems arise from the *Modus Ponens* structure of the model, for if a law is false, it may yield a logically true conclusion by valid inference. But this problem will not be dealt with here.) However, perhaps the strongest justification for its use is that it is a standard scientific method of inference. If one insists on the truth-value status of laws, and pushes the inductive problem to its limit, then one may have to resort to the less accurate probabilistic model. But this will still be in accord with scientific research methodology. It is now time to examine one further explanation type, found especially in the biological and behavioural sciences, namely teleological explanation.

The social sciences, as the term implies, are concerned with people, and as people often act or behave purposefully, all of the social

sciences have at least some concern with the explanation of phenomena of such purposive, goal-oriented systems. Collectively, these are termed teleological systems, and, in the context of explanation, teleological explanations. Men have often interpreted a wide variety of systematic phenomena as being teleological in nature. To Hegel and Marx, human, or at least societal, history is only understandable as a goal-directed system, and for Marx, this meant the downfall of capitalism and the arrival of the new, classless, communistic state.

However, the status of teleological explanations in the social (and natural) sciences has always been an unsettled one, and this has led some authors to argue that a distinctly separate logic of justification, is appropriate to the social sciences. Rudner terms these writers the "separatists," of whom their most important tenet is the belief that social phenomena are pervasively teleological in character.

In all of the social sciences, but particularly in sociology, anthropology, and political science, the teleological problems are most closely associated in terms of functions of say, certain social practices seen as functional for the biological needs of its members, or perhaps of the total social life seen as a unified social system.

One of the central problems of functionalism (amongst others, including just what it is, and what its merits are, if any, with respect to a methodological procedure in the social sciences) can be raised by the question: "Is teleological explanation radically different from, and not reducible to, the type of scientific explanation that is associated with non-teleological phenomena?" This question, then, is closely

associated with the separatist stand on methodology for, on a relatively simplistic level, it is quite easy to argue that certain statements--ordinary, commonsensical discourse about human behaviour, for example--are about behaviour whose occurrence is understood as caused or explained by certain goals or ends. For example, one may be led to assert that John X's behaviour (studying hard) is explained or caused by his goal (graduating with an honours degree). However, it seems that such an explanation of behaviour leads to the paradoxical conclusion that future (and hence, nonexistent) events have causal efficacy, for, at the time of the assertion of the cause, John X's graduation has not as yet occurred. However, this problem may be resolved by adopting a more explicit and rigorous language, for what was intended in the original assertion may be expressed by the following sentence: "John X's present hard work is explained or caused by his (present) desire to achieve the goal of graduating with honours." Now, since the cause of John X's behaviour is identified with a state of affairs that does not occur subsequent to its effect, and since the same analysis will apply *mutatis mutandi* to any assertion of unrealized goals as causes, the problem is solved. One is not forced to conclude, despite the misleading possibilities of ordinary usage, that the realm of teleological events is inhabited by ephemeral states of affairs that are nonexistent, yet in some manner causally efficacious.

However, the above example is far from complicated. Examples of more technical teleological explanations are the following:

- 1) The whiteness of the fur of polar bears is due to the fact that, in their natural habitat, this makes it difficult for them to be seen by their prey.
- 2) The purpose served by an increase of leucocytes in the blood stream during times of infection is that of guarding the body against attack by deleterious invading organisms.
- 3) Among the constituents of any human personality system will be a mechanism such as the propensity to forget material, the conscious entertainment of which would cause great pain. Without a mechanism such as repression, for example, the personality system might well give way under the strain of conflicts too painful to be long tolerated.
- 4) The persistence of type-X burial customs in society Y is explained not by the manifest functions or purposes attributed to them by the members of that society, but rather by their latent function: shoring up the members' feelings of group solidarity and hence improving morale in the face of the terrors death inevitably inspires in most humans.

All of these examples are typical of many that can be found in textbooks and scientific treatises. All of them refer, either explicitly or implicitly to functions or purposes and, as such, are teleological. However, they do differ from the "John X" example in that they make no overt reference to nonexistent things as causes of existent ones.

In the first example, the suggestion of teleological agencies is not obvious; it is oblique, yet strong enough to be charged with the

claim that it does illustrate a type of explanation radically different from ones that are appropriate to nonteleological phenomena. To the separatist, the whiteness of the fur is explained by a "function," or "end," or "purpose," namely, its conferring of a comparative invisibility from prey. However, the scientist, taking the statement as explanatory, can argue that it is simply (but severely) elliptical, and he may offer an alternative that is meaningfully equivalent to the original ellipsis and, furthermore, worded in a methodology of explanation not unlike that used in explanations of non-teleological events. This alternative presentation draws upon the conceptual resources provided by Darwin's evolutionary theory, for, by employing such concepts as "mutation" and "natural selection," statements linking the probability of survival of the polar bear with enhanced ability to carry on predatory activities that white fur contributes, are capable of providing a standard explanation for the phenomenon in question. A more fully explicit alternative might state that in snowy regions, the occurrence of white fur as a mutation will increase the probability of survival of those animals in which it does occur because it increases their probability of successful food-gathering which in turn increases the probability that they will live long enough to reproduce and transmit the mutant gene(s). In successive generations, the proportion in the total species of members exhibiting the successful mutation is likely to rise (assuming random matings in any given location) until white fur occurs virtually without exception in the species. Such statements by the nonseparatists, supported by the contributions of Darwin, have become almost routine. In

short, an alternative and explicit formulation of the explanation of white fur in polar bears can be given which does involve the relative invisibility to prey conferred by that characteristic, but which refers only to the nonteleological mechanisms of evolutionary theory. (Statements 2, 3, and 4, however, are slightly more complicated, making an essential reference to teleological systems which will not be dealt with in this thesis. See also Appendix E.)

However, very few (if any) scientists or philosophers of science would now deny that some human beings sometimes behave in a purposive or goal-directed manner, and many would, furthermore, argue that social groups, some nonhuman animals, and perhaps even some machines, behave in such a teleological manner. As Rudner states

In any case, it is especially important to notice that the non-separatist position by no means involves either a denial that there are such things as purposes, or a denial that the term "purpose" may come to figure essentially in some scientific theories and explanations. Indeed, some phenomena for which analyses could be forthcoming might very well be teleological systems of human purposes. In such cases there may be no question of the indispensability of locutions such as "purpose," "goal," or "end." In sum, the social scientist may have to cope not only with examples in which there are, ostensibly, references to the purposes of obviously nonpurposive entities, but also with examples such as 3 and 4, which refer to systems containing constituents that undeniably are purposive entities: human beings and their purposive behaviour.<sup>20</sup>

It is time now to return to the social sciences in order to analyze and evaluate the functionalist approach, especially as it has had such great influence on such disciplines as cultural or social anthropology. Such influences are not often overt, but may be hidden, implicit, or elliptically rendered by certain functionalist locutions. The following quotations are examples:

Any cultural practice must be functional or it will disappear before long. That is, it must somehow contribute to the survival of the society or to the adjustment of the individual.<sup>21</sup>

One chapter later in the same work, his involvement with a system, while still there, is not nearly so overt.

Let us take the example of the Ghost Dance cult among the Sioux Indians sixty years ago when they were beset on all sides by the White Man. The more general features of this predominantly native religion can probably be explained in functional terms.<sup>22</sup>

The function of a particular social usage is the contribution it makes to the total social life as the functioning of the total social system. It aims at the explanation of anthropological facts at all levels of their development by their function, by the part which they play within the integral system of culture, by the manner in which they are related to each other within the system. Embedded in every functional analysis, is some conception, tacit or expressed, of the functional requirements of the system under observation.<sup>23</sup>

Now, in evaluation of functionalism, it may be argued that there need not be any radical methodological break (that is "separatist" versus "nonseparatist"), for functional explanations as they often occur in, say, the anthropological literature do not fulfil all the necessary and sufficient criteria for such systems as laid out by Rudner. As he goes on to state:

. . . the actual achievement of, say, anthropologists who claim to have arrived at functional explanations of cultural items, is miniscule. The results produced to date must be seen to amount only (so far as explanation is concerned) to the articulation of some pre-scientific hunches or pious hopes that a functional explanation for the item in question can ultimately be given.<sup>24</sup>

Moreover, Rudner claims that the value of such explanations can best be employed in the context of discovery.



## SUMMARY

Having surveyed briefly the history of the scientific research enterprise in Chapter One, the thesis has been concerned in Chapter Two with a fairly intensive examination of the enterprise itself. First, a brief overview was presented and summarized in three models, and the Popperian modification of the traditional hypothetico-deductive account was emphasized. This led into the next section which dealt exclusively with Popper's approach. The first two sections are sufficient in themselves for the reader to maintain the continuity of the main argument as it will culminate in Chapter Five.

Section III initiated a more intensive examination of the two preceding sections, with special attention drawn to discovery and to Popper's approach, both of which, having been introduced in the two previous sections, are to be considered as preparatory for the analysis of discovery to which all of Chapter Three will be devoted.

In Section III discussions are carried out on the main items of scientific research. As compared to Section I, the focus may be described as "sharper" and at "higher power." Hypotheses and laws were taken up, including the logical sentence structure, forms of laws, and the notion of prediction. The distinction between a "law of nature" and a "law of science" was made which facilitated, in part, the discussion of the realist, nominalist, and conceptualist position on the status of laws. The thesis supports the latter position, and justifications are given. Next, theories were taken up involving a discussion of the abstract

calculus, models, and the ontological status of theoretical entities. Two positions on theories were taken up, and the thesis developed a modified third approach, called the pragmatic. Popper's notion of testability was again emphasized. Finally, an investigation was carried out on the concept of "scientific explanation," with some special attention given to Newtonian mechanism because of its historical importance. The main thrust, however, was centered upon the Covering-Law Model, teleological explanation, and the reduction of teleological statements into nonteleological ones. The thesis of the symmetry between explanation and prediction was also mentioned.

Appendix F deals with an examination of the Kinetic-Molecular Theory in order to point up some of the key terms as previously outlined. Some of the concepts discussed included: empirical observation and formulation of assumptions; empirical justification of assumptions; Boyle's Law, Charles' Law, Dalton's Law, Brownian Motion, and Graham's Law. It is hoped that the reader may reflect upon this and the previous discussions and so better understand the manner in which a standard theory in science is generated and presented.

In the historical overview presented in Chapter One, it was indicated that the scientific tradition has had a long and singularly successful history. However, it was also hinted that the enterprise was not without conceptual and/or methodological, foundational problems. Some of these have been touched upon in Chapter Two, with special attention given to one of these problems in particular, namely, scientific discovery. This will be the subject of intensive examination in the following chapter.

## FOOTNOTES

<sup>1</sup>John Hospers, *An Introduction to Philosophical Analysis*, Englewood Cliffs, New Jersey, 2nd ed., 1967, Prentice-Hall Incorp., p. 230.

<sup>2</sup>Wesley C. Salmon, *The Foundations of Scientific Inference*, U.S.A., University of Pittsburgh Press, 1967, p. 24.

<sup>3</sup>*Ibid.*, p. 25.

<sup>4</sup>Karl R. Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, New York and Evanston, Harper and Row, 1968, p. 34.

<sup>5</sup>*Ibid.*

<sup>6</sup>D. Hume, *Enquiry Concerning Human Understanding*, London, 1748. Published as *Philosophical Essays Concerning Human Understanding*. Modern edition by C. W. Hendel, New York, 1955.

<sup>7</sup>*Ibid.*

<sup>8</sup>Popper, *Conjectures and Refutations*, p. 36.

<sup>9</sup>*Ibid.*, p. 37.

<sup>10</sup>*Ibid.*, p. 38.

<sup>11</sup>*Ibid.*, pp. 36-37.

<sup>12</sup>Marx W. Wartoksky, *Conceptual Foundations of Scientific Thought*, New York: The Macmillan Company, 1968, p. 258.

<sup>13</sup>Richard S. Rudner, *Philosophy of Social Science*, Englewood Cliffs, N.J., Prentice-Hall, Inc., 1966, p. 10.

<sup>14</sup>Ernest Nagel, *The Structure of Science*, New York: Harcourt, Brace and World, 1961, p. 91.

<sup>15</sup>C. Furtado, *Development and Underdevelopment*, Berkeley, University of California Press, 1967, p. 62.

<sup>16</sup>Abraham Kaplan, *The Conduct of Inquiry*, San Francisco: Chandler Publishing Co., 1964, Chapter VII.

<sup>17</sup>Popper, *Conjectures and Refutations*, p. 49.

<sup>18</sup>*Ibid.*, pp. 50-51.

<sup>19</sup>Hospers, *An Introduction to Philosophical Analysis*, p. 241.

<sup>20</sup>Rudner, *Philosophy of Social Science*, p. 88.

<sup>21</sup>C. Kluckhohn, *Mirror for Man*, New York, Fawcett World, February, 1957, Premier edition, p. 28.

<sup>22</sup>*Ibid.*, pp. 53-56.

<sup>23</sup>R. Merton, *Social Theory and Social Structure*, revised and enlarged edition, New York, Free Press of Glencoe, Inc., 1957, pp. 25-27.

<sup>24</sup>Rudner, *Philosophy of Social Science*, p. 109.

## CHAPTER THREE

### SCIENTIFIC DISCOVERY

#### SECTION I

*Introduction: Whilst the main argument of the thesis is conceptual, some empirical approaches are cited for additional support.*

The discussion of scientific discovery will be based upon four approaches as follows:

- (a) psychological, focusing on the manner in which some psychologists describe the process;
- (b) direct report, being a selection of quotations from prominent men of science (closely related to (a) above);
- (c) philosophical (I), being a discussion and stand on the traditional problem of the existence, or not, of a logic of scientific discovery. Borrowing concepts from psychology, the thesis assumes, for simplicity of argument, the exhaustiveness of a "logic" and a "non-logic" of discovery. Convergent thinking is taken to be equated with "logic" and divergent with "non-logic," and, as "divergent" is equated with "creative," then "non-logic" is associated with "creativity."

Thus, if discovery can be argued as non-logical then it may be considered divergent and, hence, creative;

- (d) alternative approaches for a logic of discovery founded on the notions of the logic of heuristic plausibility inferences and on models;
- (e) philosophical (II), exegesis and analysis of Blackwell's theory of discovery.

## SECTION II

*D. O. Hebb distinguishes two categories: (a) discovery and (b) verification. F. C. McLean emphasizes science as an art. H. A. Simon assumes the creativity of scientific discovery and attempts to develop a psychological theory (as yet incomplete) of scientific discovery. These empirical studies to lend support to subsequent conceptual claims.*

In *A Textbook of Psychology*, D. O. Hebb includes a chapter entitled "Problems Relating to Thought" of which one section is concerned with discovery, invention and logic *vis-à-vis* the nature of scientific thought. He distinguishes two categories: (1) discovery or the attainment of new ideas, and (2) verification, or the process of testing, clarifying, and systematizing these ideas. The researcher often does not know what combination he is looking for and, after exhausting the logical procedures available, turns to what may be described as a "blind" manipulation of the problem elements. Hebb also holds that here chance plays a significant role. It is interesting to note a psychologist's

minimal ascription of logic to the discovery process. Often there seems to be no logical justification to the entertainment of the idea except that, in the end, as Hebb says, it "works."

Hebb does admit of a role for formal logical analysis, such as syllogism and systematic induction, but these are within the context of validation. In the context of discovery he speaks of "insight" and "purpose," noting that "new insight consists of a re-combination of pre-existing mediating processes" which latter are products of sensory data. To him, these re-combinations are original and creative.

F. C. McLean in his essay "The Happy Accident" (published in *The Scientific Monthly*)<sup>1</sup> emphasizes the fact that science is an art. He maintains that there is some misconception as to the role played by rational analysis in discovery, and that rational analysis can, at best, only "point the way" to discovery.

McLean holds that the perception of unexpected relationships first occurs in the unconscious to which the scientist adds material gained from conscious activity. This "addition" leads to discovery, but the process as a whole cannot be trained.

Herbert A. Simon's article "Scientific Discovery and the Psychology of Problem Solving" (published in *Mind and Cosmos* in the University of Pittsburgh's lecture series)<sup>2</sup> argues that discovery is a form of problem-solving, and can be explained in the terms that have been used to explain the latter. In developing his psychological theory of scientific discovery he hypothesizes that there is no qualitative difference between work of high creativity, associated with what T. S. Kuhn would term "revolutionary

science," and "journeyman" creativity, associated with "normal science." It can be seen that Simon assumes the creativity of scientific discovery. Moreover, his theory emphasizes the "hard work" of discovery, especially the painstaking process of selective trial and error. The theory also provides for a mechanism that would account for the process of "incubation" and the phenomenon of "illumination." The theory, as Simon points out, makes no pretense at a complete understanding of the total structure of the psychological processes involved.

The data used were obtained from interviews with, and questionnaires sent to scientists, and the passages discussed here seem to suggest, from a psychological point of view, support for a claim of non-convergence or non-logical formation of new ideas (hypotheses). Much work is still being done on the nature of the psychology of creativity, and these present studies do include sample populations of scientists as well as the perhaps more commonly perceived types of creative individuals such as writers, painters and musicians. The thesis makes no pretense that the psychological work that has been done, and is being done, supports a strong (necessary) claim for the creativity of scientific discovery. Still less does it pretend that the three discussions by psychologists presented in this section are sufficient. Their inclusion is simply to show another means of investigation, albeit empirical, which can perhaps be fruitfully pursued and utilized in support of the conceptual claims to be made later.



### SECTION III

*Introduction: another approach for investigating the nature of discovery is to examine what scientists themselves say, which approach is very closely related to that of the immediately preceding section. The four categories into which these quotations are systematized:*

- (1) *subjective speculation and the humanistic dimension:*  
(A. Szent-Gyorgyi. R. R. Wilson. A. Einstein. H. Poincaré. W. I. B. Beveridge).
- (2) *(creative) imagination:*  
(M. Planck. F. Kekulé. R. B. Lindsay. J. Tyndall).
- (3) *Inventiveness, intuition and ingenuity:*  
(A. Einstein. H. Poincaré. K. Gauss).
- (4) *Selectivity, critical evaluation, and role of prior knowledge:*  
(H. Poincaré. W. I. B. Beveridge. J. Watson. R. B. Lindsay).

The thesis takes the stand that a fruitful approach to the understanding of scientific discovery is to examine what eminent scientists themselves have said about it. For the most part these thoughts, although recorded, have not been systematized. After an examination of several passages, a number were selected and categorized into four "themes" in order to facilitate the total argument. The categories are arbitrary and are not intended to be clear-cut, indisputable classifications; indeed, it is not always easy to articulate the exact difference between any two terms. Furthermore, they are probably inter-related and inter-dependent. These four categories are:

- (1) subjective speculation and the humanistic dimension;
- (2) (creative) imagination;

(3) inventiveness, intuition, ingenuity;

(4) selectivity, critical evaluation and role of prior knowledge.

(1) *Subjective speculation and the humanistic dimension:*

Israel Scheffler has coined the term "the standard view of science" which term describes the attitude that many people have toward the enterprise. It is a view which not only ascribes a cold objectivity to scientific methodology, but also asserts that science is wholly a systematic, public enterprise, controlled by logic and empirical fact, and having for its purpose the formulation of truths about the natural world. Scientists themselves would not deny the objective nature of their discipline. However, if it is over-emphasized, then one tends to underestimate and even conceal the humanistic aspect of scientific discovery. Based upon the writings of scientists themselves, this aspect includes a sense of inwardness, of creative impulse and secret ambitions, of irrationalities and intellectual growth--in short, of science, scientists, and scientific activities as expressions of *human* endeavour. Any picture of the scientist as a cold, detached robot, discovering great truths by systematic methods and rigorous controls would be to distort grossly what may be termed the subjective, humanistic nature of scientific research, for (as has been indicated in the immediately preceding section) it appears that the function of systems and controls in science is not to generate discoveries by mechanical routines, but rather to channel criticism and facilitate evaluations. Discovery is probably a more humanistic, personal, and subjective process such that, as Scheffler says: "The cold and aloof scientist is, then, a myth."<sup>3</sup>

As support for this claim of subjective speculation and humanistic endeavour, consider what Nobel-laureate Albert Szent-Gyorgyi writes:

Naturally like all other scientists, when I publish or speak about my work, I like to make it appear as though it has been one straight line, one preconceived logical unit.

But while I work I usually do not know where I am going. I just follow hunches. I dream up all sorts of theories at night and then disprove them in the laboratory the next day. Checking a hunch, sometimes I see some discrepancy, something unexpected--then I follow it up. Success depends on whether the hunch was good or bad . . . .

Yes, I was more lucky than many of my colleagues and this surprises me sometimes. So many scientists that I meet here during the summer know much more than I. They can put down their ideas much more clearly, more mathematically, and can describe exactly what they are after . . . I just wander about, without especially clear ideas or preconceived notions so far as I know, and now and then something pops up--boom!--something that is entirely new, that leads to new lines of research . . . .

Claude Bernard, the greatest of physiologists and one of my idols, worked in this way, too. I like the distinction he made between the two kinds of knowledge--that which has been achieved already and that which has yet to be wrung from the unknown. The great scientists were those who were at home in the unknown and left their footsteps there.<sup>4</sup>

Also, consider the following quotations from American nuclear physicist Robert R. Wilson speaking of his subject:

I do think of it as a form of human activity, and I've thought of physics as a way in which I've expressed myself not only to others but also to myself. There's much more to it--but this is an important part.<sup>5</sup>

In speaking of one of his failures he mentions the creativity of science,

Yes, a failure, but that didn't take away from the thrill of creativity, and the strong identification of oneself. Through it

I had become involved suddenly with all humanity. It's a peculiar way of thinking about yourself and the rest of the world.<sup>6</sup>

and he goes on to liken this pleasurable creativity to artistic creativity. His reply to a question as to whether or not he considers that there is a qualitative difference between scientific creativity and the more familiar artistic creativity, he replied:

No, I don't, I would guess that they're identical. When I am doing physics, as I said earlier, I don't think of it as science with a capital S. I am only conscious of myself as a man doing what I am doing in a manner nowise different from that when I do other things than physics. I don't like that connotation. I wish we weren't known as scientists. I think of myself only as a person working in a personal manner. I'm sure if I could make a good poem, I would experience the same feelings.<sup>7</sup>

Albert Einstein also speaks of creativity in science, both with concepts:

. . . all our thinking is of this nature of a free play with concepts . . .<sup>8</sup>

and also with functional relationships:

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.<sup>9</sup>

Valuable insights can also be obtained from Henri Poincaré's celebrated essay entitled "Mathematical Creation." He notes two phases of the discovery process, the first being a type of "inspiration":

Most striking at first is this appearance of sudden illumination, a manifest sign of long, unconscious prior work. The role of this

unconscious work in mathematical invention appears to me incontestable . . . . These sudden inspirations . . . never happen except after some days of voluntary effort which has appeared absolutely fruitless . . . . It is necessary to put in shape the results of this inspiration, to deduce from them the immediate consequences.<sup>10</sup>

This stage is followed by a more rationally controlled one, one which has already been labelled the context of validation:

As for the calculations themselves, they must be made in the second period of conscious work, that which follows the inspiration, that in which one verifies the results of this inspiration and deduces their consequences. The rules of these calculations are strict and complicated. They require discipline, attention, will, and therefore consciousness.<sup>11</sup>

Also, Poincaré compares mathematical creation to artistic creation:

It may be surprising to see emotional sensibility invoked *à propos* mathematical demonstrations which, it would seem, can interest only the intellect. This would be to forget the feeling of mathematical beauty, of the harmony of numbers and forms, of geometric elegance. This is a true aesthetic feeling that all real mathematicians know, and surely it belongs to emotional sensibility.<sup>12</sup>

Beveridge supports this view:


Emotional sensitivity is perhaps a valuable attribute for a scientist to possess. In any event the great scientist must be regarded as a creative artist and it is quite false to think of the scientist as a man who merely follows rules of logic and experiment.<sup>13</sup>

In addition, he notes that both Einstein and Planck were keen musicians, and that Pasteur had a distinct aptitude for painting.

## (2) *Imagination*

Although the controlled thinking as advocated by Dewey has a place in science, it seems that researchers, in seeking original ideas, often abandon this mode of thought and allow their imaginations to wander freely. Consider what the famous physicist Max Planck once said:

Again and again the imaginary plan on which one attempts to build up order breaks down and then we must try another. This imaginative vision and faith in the ultimate success are indispensable. The pure rationalist has no place here.<sup>14</sup>

This imaginative capacity, subjective though it may be, seems definitely to play a more prominent role than is ordinarily realized. It has been reported that Clark Maxwell was in the habit of making mental pictures of problems, as also was Paul Ehrlich. Of particular historical interest is the manner in which the German chemist Kekulé hit upon the structural formula for benzene (the benzene ring ) , C<sub>6</sub>H<sub>6</sub>.

'But it did not go well; my spirit was with other things. I turned the chair to the fireplace and sank into a half sleep. The atoms flitted before my eyes. Long rows, variously, more closely, united; all in movement wriggling and turning like snakes. And see, what was that? One of the snakes seized its own tail and the image whirled scornfully before my eyes. As though from a flash of lightening I awoke; I occupied the rest of the night in working out the consequences of the hypothesis . . . . Let us learn to dream, gentlemen.'<sup>15</sup>

Also, consider what Lindsay writes concerning imagination:

Actually the process of theory building and modification has many elements that relate it closely to artistic creativity.

The next step requires a closer look at the nature of the postulates at the basis of physical theories. Where do they come from? A

theory begins as an act of imagination; it is in essence the creation of a picture in the mind of the scientist. This introduces the problem, but hardly solves it, for how does the physicist get the ideas that form the basic elements of the picture? As we have already suggested, this is a problem in psychology and one to which too little attention has so far been devoted. Too few scientists have ever divulged in any detail the processes they have gone through in setting up their theories, and even when they have discussed the matter, what they have written has not usually been helpful. This may be due partly to a feeling that the line of thought being followed was so straightforward and compelling that it should appear obvious without further comment to all thinking persons. It may also be due to a feeling that inspiration is not discussable. It is true that some very famous scientists have written about flashes of illumination that came to them when, after long periods of unsuccessful conscious thought, they had attempted to forget the matter at issue in the hope that the subconscious would take over. Such stories are suggestive but do not really instruct. Perhaps there is no instruction to be had in such matters, except by close association with the creative scientist. This is, however, possible for few. Psychologists acquainted with physics should give the problem greater attention.<sup>16</sup>

The importance of imagination is further emphasized by these two quotations by Tyndall:

- (a) Newton's passage from a falling apple to a falling moon was an act of the prepared imagination. Out of the facts of chemistry the constructive imagination of Dalton formed the atomic theory. Davy was richly endowed with the imaginative faculty, while with Faraday its exercise was incessant, preceding, accompanying and guiding all his experiments. His strength and fertility as a discoverer are to be referred in great part to the stimulus of the imagination.<sup>17</sup>
- (b) With accurate experiment and observation to work upon, imagination becomes the architect of physical theory.<sup>18</sup>

But, dreams and images are useless in science unless reason turns them to useful purpose. Vague ideas must be formulated into specific propositions or hypotheses. Although imagination can be a fruitful source of new knowledge, it must be balanced by criticism and judgment.

### (3) *Intuition and ingenuity*

The psychology of intuition is not thoroughly understood, but the so-called intuitive leap is generally recognized and an important element in most problem solving. The most characteristic circumstances of an intuition are a period of intense work on the problem accompanied by a desire for its solution, temporary abandonment of the work, and then the appearance of the idea with dramatic suddenness and often a feeling of certainty. There is a fairly general, though not universal, agreement that these intuitions arise from the subconscious activities of the mind which has continued to turn over the problem, though on the conscious level it appears that the mind is no longer giving it attention.

Consider what Einstein has said:

There is no logical way to the discovery of these elemental laws. There is only the way of intuition, which is helped by a feeling of the order lying behind the appearances.<sup>19</sup>

Poincaré relates how, after a long period of intense mathematical work he went for a journey into the country and dismissed his work from his mind:

Just as I put my foot on the step of the brake, the idea came to me . . . that the transformations I had used to define Fuchsian functions were identical with those of non-Euclidian geometry.<sup>20</sup>

On another occasion, when baffled by another problem he went for a walk along the seaside, and:

thought of entirely different things. One day, as I was walking on the cliff the idea came to me, again with the same characteristics



of conciseness, suddenness and immediate certainty, that arithmetical transformations of indefinite ternary quadratic forms are identical with those of non-Euclidian geometry.<sup>21</sup>

The "flash of intuition" is also related by Gauss:

. . . finally two days ago I succeeded . . . like a sudden flash of lightening the riddle happened to be solved. I cannot myself say what was the conducting thread which connected what I previously knew with what made my success possible.<sup>22</sup>

(4) *Selectivity, critical evaluation and role of prior knowledge*

With respect to mathematical creativity, Poincaré writes:

To create consists precisely in not making useless combinations and in making those which are useful and which are only a small minority . . . <sup>23</sup>

He goes on to say:

The true work of the inventor consists in choosing among these combinations so as to eliminate the useless ones or rather to avoid the trouble of making them . . . they are felt rather than formulated.<sup>24</sup>

and in further describing these useful combinations he says they are "precisely the most beautiful."<sup>25</sup>

According to Beveridge, "The fertile mind tries a large number and variety of combinations."<sup>26</sup> To see how selectivity and combination together with the role of prior knowledge play a significant part in scientific discovery, consider the discovery of the structure of the DNA (Desoxyribonucleic Acid) molecule by Nobel laureate James Watson and Francis Crick.

Watson's method of model building was highly disregarded at first; the standard method was X-ray crystallography. It is interesting to note that Maurice Wilkins, who played a significant role in the discovery by providing the X-ray evidence, finally arrived at model building as a valid method. According to Watson:

. . . our past hooting about model building represented a serious approach to science . . . 27

Watson had earlier developed a model which had several flaws. This first model involved like-with-like pairing of bases, but the model failed, amongst other things, to explain the Chargaff rules (that the concentration of adenine = the concentration of thymine and that the concentration of guanine = the concentration of cytosine), and the lack of any X-ray evidence for a regular Hydrogen-bonding. Watson writes:

Though I initially went back to my like-with-like prejudices, I saw all too well that they led nowhere. When Jerry came in I looked up, saw that it was not Francis, and began shifting the bases in and out of various other pairing possibilities.<sup>28</sup>

Today, genetics students are expected to have a reasonable understanding of both organic chemistry and biochemistry. Watson, however, without this ready information had unknowingly copied the incorrect structural formulae of the nitrogen bases from text books. It was only after he was informed by a chemist that the configurations were incorrect, or at best, very tenuously accepted, that Watson was able to make further progress.

. . . . Suddenly I became aware that an adenine-thymine pair held together by two hydrogen bonds were identical in shape to a guanine-cytosine pair held together by at least two hydrogen bonds. All the hydrogen bonds seemed to form naturally; no fudging was required to make the two types of base pairs identical in shape. Quickly I called Jerry over to ask him whether this time he had any objection to my new base pairs.<sup>29</sup>

This quotation is a good example of the role of selectivity, ordering, and critical evaluation. According to Beveridge, productive scientific thinking begins with an awareness of differences, after which a suggested solution probably springs into mind and is considered (plausibility considerations), or rejected. Then, new combinations often arise from rational association, or from fancy (imagination), or perhaps by chance circumstances.

Lindsay echoes the same thoughts regarding the role of prior knowledge.

One thing is sure. The thoughts that develop into valuable physical constructs come to those who are equipped to receive them by thorough preliminary preparation in the subject matter in question. It is obviously impossible to start to theorize on a branch of physics about which one knows nothing, and it is equally clear that every theorist inevitably is influenced in the creation of new ideas by the ideas he has obtained from others in his previous study.<sup>30</sup>

#### SECTION IV

*The philosophical (I) approach to the problem of discovery. The traditional arguments in favour of a logic of discovery. Francis Bacon and J. S. Mill: exegesis and criticism. Can any sense be made of a logic of discovery? N. R. Hanson's position: plausibility considerations. W. C. Salmon's criticism of Hanson's position: formulation of hypotheses are prior to*

considerations of plausibility. G. Holton and the X -, Y -, and Z - planes of scientific research. C. Hempel's position. R. Rudner's position: one must not confuse the context of discovery with that of validation. M. Polyani: the concept of "scientific neutrality" is akin to Scheffler's "standard view of science." I. Scheffler: there is both an objective and a subjective phase to scientific research. The nature of scientific subjectivity and its relationship to discovery.

*Philosophical discourse on the nature of scientific discovery is systematized into the same categories as in Section III, i.e.,*

- (1) *subjective speculation and the humanistic dimension;*  
(I. Scheffler. K. Popper).
- (2) *(creative) imagination;*  
(C. Hempel. A. N. Whitehead).
- (3) *intuition and ingenuity;*  
(C. Hempel. I. Scheffler).
- (4) *selectivity, critical evaluation, and the role of prior knowledge.*  
(C. Hempel).

The two strongest advocates of an inductive approach to discovery were Francis Bacon (*Novum Organum*) and John Stuart Mill (*A System of Logic*). Bacon was the first of what was to develop as a long line of scientifically minded philosophers who not only emphasized the importance of induction, as opposed to deduction, but also attempted to find some superior type of induction to replace what is termed "induction by simple enumeration." For example, Bacon's method as applied to discovering the nature of heat (which he supposed, rightly, to consist of rapid, irregular motions of the small parts of bodies) would have been to make lists of hot bodies, cold bodies, and of bodies of varying degrees of heat. He

then expected that these lists would then show some characteristic always, for example, present in hot bodies, and absent in cold bodies, and present in varying degrees in bodies of different degrees of heat. By this method he expected to arrive at general laws, which would fit into a hierarchy according to degree of generality. However, notwithstanding both his ignorance of the developments in science of his day, as well as his unappreciative attitude towards mathematics and syllogistic, Bacon's inductive method is faulty because of his insufficient emphasis on hypothesis. He hoped that mere orderly arrangement of data would make the correct hypothesis obvious, but, in fact, this is seldom the case. Indeed, not only is the framing of hypotheses often the most difficult part of scientific inquiry, but what is generally overlooked is that usually some hypothesis is a logically prior condition to the collection of facts. The act of hypothesis generation, then, seems devoid of any rule-centered method.

Mill, in applying principles of Empiricism to scientific method, advocated (by rounding out and perfecting Bacon's inductive technique), induction as a new approach to problem-solving that would supersede Aristotelian deductive logic. Moreover, he contended that all reasoning is essentially inductive, that we reason from particular instances and then generalize upon them. As for Mill, notwithstanding his five canons of inductive method, the same charges laid against Bacon hold. Furthermore, his canons can be usefully employed so long as the law of causality is assumed, but this law itself, as he himself had to confess, is to be accepted solely on the basis of induction by simple enumeration. Moreover

it is not altogether clear whether these canons fit into the context of discovery or the context of validation.

Consider the following passage from Mill:

An hypothesis is any supposition we make (either without actual evidence, or on evidence avowedly insufficient) in order to endeavor to deduce from it conclusions in accordance with facts which are known to be real, under the idea that, if the conclusions to which the hypothesis leads are known truths, the hypothesis itself either must be, or at least is likely to be true. . . . We want to be assured that the law we have hypothetically assumed is a true one, and its leading deductively to true results will afford this assurance, provided the case be such that a false law cannot lead to a true result, provided no law except the very one which we have assumed can lead deductively to the same conclusions which that leads to. . . . It appears, then to be a condition of the most genuinely scientific hypothesis that it be not destined always to remain an hypothesis but be of such a nature as to be either proved or disproved by comparison with observed facts.<sup>31</sup>

He makes it clear that hypothesis are to be associated with facts of experience (consider especially the last sentence), but contemporary scientific methodology (contemporary empiricism), especially in physics, use hypotheses highly theoretical in nature and which have no immediate observational consequences. But this is not to deny that at some point, usually at the end of a long deductive chain, propositions do come into contact with facts of experience. Nevertheless, the immediate hypothesis itself may be formulated quite apart from the observational predicate of the universe of experience. Moreover, Mill wants to treat hypotheses as an inference technique to arrive at, presumably, discoveries, or at least "to deduce from it facts which are known to be real" which facts are "to be either proved or disproved by comparison with observed facts." But if this be Mill's position (that is, that the truth and the verification

of an hypothesis is the same as the truth of the consequent in a material implication affirming the antecedent), then he has committed a logical error (fallacy of affirming the consequent), for

$$([p \supset q] \cdot q) \supset p$$

is not logically necessary. In short, Mill's position can be attacked on at least two logical grounds for, as argued in Chapter Two, hypotheses are never proved, but only corroborated. Mill is following the traditional hypothetico-deductive account and this turns out to be logically unsupportable, for, as has been discussed previously, *Modus Ponens*

$$([p \supset q] \cdot p) \supset q$$

although logically necessary, is inadequate to describe the empirical scientific research enterprise, and that, in fact, *Modus Tollens*

$$([p \supset q] \cdot \sim q) \supset \sim p$$

is more accurate. This point demonstrates Mill's fallacy in talking about hypotheses as being "either proved or disproved by comparison with observed facts." But Mill seems to stray further when, as discussed earlier, he goes on to commit the fallacy of affirming the consequent.

Can any sense, then, be made of a logic in the discovery process? It seems undeniable that science uses a type of deductive inference at least loosely akin to the hypothetico-deductive method, and this has led many people to conclude that the logic of science, and of discovery, is an indisputable fact, thoroughly deductive in character. The prevailing

stand of modern philosophers of science is that there is no such logic, and that the process by which scientific hypotheses are thought up, at least in part, is a psychological one requiring insight, originality and ingenuity, rather than any mechanical procedure. As W. S. Salmon says,

Science . . . is not a sausage machine into which you feed the data and, by turning a crank, produce finished hypotheses.<sup>32</sup>

However, this view is disappointing, even intolerable, to some because it is difficult to accept that, for a scientist grappling with a difficult problem, logic is of no help whatsoever in effecting a solution. Indeed, it is very probable that logic does figure in very importantly in scientific discovery (general sense), so that one may then agree with philosophers, such as N. R. Hanson, who have tried to demonstrate that logic has indeed a very real bearing on scientific method. He maintains that, in addition to the admittedly psychological aspects of scientific discovery, certain logical considerations play an important role in formulating the hypotheses.

What would be our reasons for accepting *H*? These will be those we might have for thinking *H* true. But the reasons for suggesting *H* originally, or for formulating *H* in one way rather than another, may not be those one requires before thinking *H* true. They are, rather, those reasons which make *H* a *plausible type of conjecture*.<sup>33</sup>

However, as shall be investigated in more detail, it could also be argued that Hanson may not have adequately analyzed the concept of scientific discovery, especially in respect to the discerning of a difference between a general and a special sense of the term "discovery."



W. C. Salmon, in his discussion of Hanson's logic of discovery, begins by cautioning against overly stringent or excessive demands on such a logic, saying that such an algorithm of mechanics generating true explanatory hypotheses is probably impossible as there seems to be no way of determining for certain whether or not one has in fact a true hypothesis. As Salmon says:

Not since Francis Bacon has any empiricist regarded the logic of science as an algorithm that would yield all scientific truth.<sup>34</sup>

It may then be asked: "What is the nature of Hanson's logic?" He says that it must not be understood as a method of generating true hypotheses, but rather as a method of generating plausible conjectures. Hanson differentiates two stages:

A reasons for accepting  $H$

(supporting reasons) reasons for thinking  $H$  is true "validation or justification").

B reasons for suggesting  $H$  in the first place (or for formulating  $H$  in one way rather than another).

(supporting reasons) reasons which make  $H$  a plausible conjecture.

Hanson readily concurs, at least in part, with those who have denied the existence of a logic of discovery, agreeing with them that the process of discovery is governed by psychological factors and by the existence of

nonlogical aspects. However, he does argue for the existence of good, logical reasons for regarding certain hypotheses as those most likely to succeed. Moreover, these reasons are logically distinct from the supporting reasons given under A in the diagram above. These kinds of justifications, allow one to elevate the hypothesis from the level of plausible conjecture to that of acceptable, true or highly confirmed hypothesis.

In his criticism of Hanson, Salmon begins by differentiating three aspects of the treatment of scientific hypotheses, each aspect of which may be taken separately, together, or in any order. These three stages are: (1) thinking up the hypothesis; (2) plausibility considerations; and (3) testing or confirmation.

Salmon agrees with Hanson's argument that there is an important logical distinction between plausibility arguments (B supporting reasons in diagram above) and the reasons that, after testing, confirm a hypothesis (A supporting reasons, above). On the basis of Hanson's example, which uses Kepler's elliptical-orbit hypothesis, Salmon argues that Hanson has mistakenly conflated plausibility arguments with discovery, or more specifically, with a logic of discovery. There is, presumably, a time lapse between the moment of thinking up an hypothesis and of subsequently considering its plausibility. It is this first step of Salmon's system that, in Chapter Two of this thesis has been termed the stage of scientific discovery (special sense).

Salmon's system, with the three stages of a scientific hypothesis, can be viewed from another perspective. Gerald Holton speaks of the three

dimensions of science as (1) the X - plane of empirical observation and experiment; (2) the Y - plane of heuristic-analytic theorizing; and (3) the Z - plane of fundamental presuppositions, methodological judgments, decisions and notions, all of which he collectively terms "themata." These themata, such as those of "constancy," "conservation," and "quantification," are not directly resolvable into objective observations, on the one hand, nor logical, mathematical, or other formal, analytic, or exact reasoning on the other hand. He then goes on to say:

. . . perhaps all of these themata are not restricted merely to uses in scientific context, but seem to come from the less specialized ground of our general imaginative capacity.<sup>35</sup>

Hempel also tends to rule out a logic of scientific discovery. He describes the process which, according to Bacon, involves induction and a set of mechanical rules applied to observed facts that lead to corresponding general principles, which mechanical rules could be compared to those of multiplication. He argues that if we were to have such an effective means of scientific discovery, many chronically perplexing problems, such as a cure for cancer, or a probabilistic basis, would have already been discovered. Furthermore, it is doubtful whether such a logic could even be formulated, one reason being that scientists use theoretical terms that do not occur at all in the descriptions of the empirical findings on which the theory rests. However, it may be possible to infer inductively a hypothesis as long as it contains no novel terms. Consider as an example of the first case the theories about the atomic and subatomic structure of matter, in which "novel" terms such as *atom*,

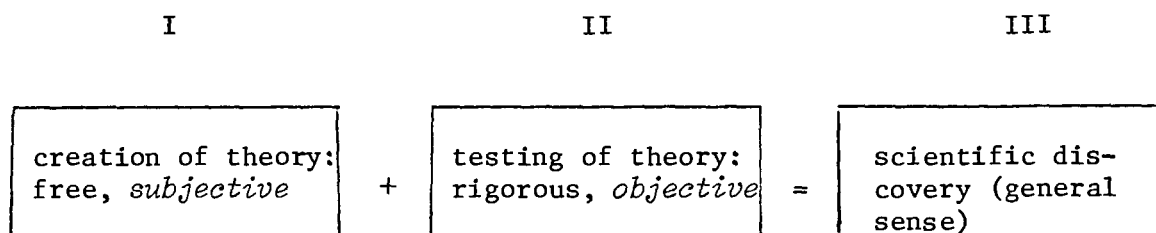
*proton*, *neutron* and *psi-function* are used, but whose existence are not descriptive of the empirical observations, such as the spectra of various gases, tracks in cloud and bubble chambers, stereochemistry, and so forth. On the basis of the fact that these terms do not describe the laboratory observations, one may argue that a logic of discovery can never be found. In the other case, where it would be possible to inductively infer a hypothesis as long as there are no novel terms, Hempel uses the example of the special, and rather simple relationship of the length of a copper wire as a one-to-one function of the temperature. After plotting the data and drawing curves, the scientist finds that the length varies directly with the temperature and hence one may assume that he has discovered the hypothesis (length varies directly with temperature) by a mechanical routine or logic of discovery (inductive inference). However, Hempel draws attention to the fact that this mechanical procedure actually presupposes an antecedent, less specific hypothesis, viz., that a single variable, length, is a function of one single other variable, temperature. Also the hypothesis contains no new terms, and is expressible in terms of the concepts of temperature and length which are also used in describing the data.

Rudner also cautions against confusing the "context of validation" (the acceptance or rejection of an hypothesis) with the "context of discovery," or how one arrives at good hypotheses and this means, in the case of the social sciences, how one draws upon sociological, psychological, political, or economic conditions conducive to thinking up fruitful hypotheses. He says:

No one, in fact, has demonstrated that there is or could be such a thing as a logic of discovery. On the other hand, a logic, or methodology of validation, of explanation, or of prediction, is precisely what is referred to when it is asserted or denied that (regardless of differences in technique of observation or experiment) the scientific method is pervasive through all the sciences or is applicable in the investigation of social as well as non-social phenomena.<sup>36</sup>

Polanyi has been largely concerned with the process by which order and coherence are extracted from the scientific and technological manipulation of matter, and he holds that the discovery of significant patterns in nature often involves acts of personal judgments and participation that refute, to at least some degree, the "ideal" of scientific neutrality in dealing with natural phenomena. This scientific neutrality would probably be what Scheffler would label "the standard view of science."

One comes, then, to an examination of this notion of scientific subjectivity which was mentioned earlier as being bound up in some way with the process of scientific discovery. Israel Scheffler has written substantially on this topic, and his argument is basically that scientific method employs both an objective and a subjective phase, the latter of which is often little understood, recognized, or even admitted by many scholars and laymen alike. Objectivity in science is largely confined to that part of the total methodology leading to the scientific discovery (general sense) that deals with tests, controls and critique. It is involved in the processing of ideas that have been independently and freely generated. The following diagram shows this objective-subjective dichotomy:



Step II is objective, and is the stage where rigorous discipline enters into the evaluation of the theory's empirical adequacy, logical coherence, and relative simplicity.

Scientific objectivity, then, in imposing the constraints of descriptive accuracy, theoretical coherence and logical discussion, is not a routine for theoretical discovery, for this latter concept presupposes, as Scheffler would argue, no general limitations of passion, imagination or flair. There is a subjective nature to the scientific enterprise, for as there appears to be no objective routine for scientific discovery, one must use imagination to speculate, and this process is subjective. Scheffler says:

Nor does scientific objectivity consist in a routine for theoretical discovery; if the scientist is not a disembodied and passionless intellect, neither is he a robot.<sup>37</sup>

What is the nature of this subjective aspect of scientific discovery? According to Scheffler, in this act the scientist chooses and revises descriptive categories, guesses at hidden factors, extrapolates selectively on limited data and invents new mechanisms, calculi, and theories which only later are submitted to objective, critical assessment. If this be the true nature of scientific discovery (special sense),

then the Baconian ideal of devising mechanical rules of scientific discovery is not supportable, and Mill's method of experimental inquiry will be reduced simply to a method of testing hypotheses rather than a method of discovery. This kind of subjective, speculative thinking seems to be the basis of discovery in the special sense.

The basis for the establishment of a subjective foundation for scientific discovery has now been completed. The thesis will now attempt to relate the four "themes" of Section III in terms of what philosophers of science have said about discovery. These four themes, by which the quotation of several scientists were categorized, are as follows:

- (1) subjective speculation and the humanistic dimension;
- (2) (creative) imagination;
- (3) inventiveness, intuition and ingenuity;
- (4) selectivity, critical evaluation, and the role of prior knowledge.

(1) *Subjective speculation and the humanistic dimension:*

Scheffler, in discussing the role of speculation in scientific thought, says:

For impartiality and detachment are not to be thought of as substantive qualities of the scientist's personality or the style of his thought; scientists are as variegated in these respects as any other group of people. Scientific habits of mind are compatible with passionate advocacy, strong faith, intuitive conjecture, and imaginative speculation.<sup>38</sup>

Also, Karl Popper echoes somewhat the same idea when, in his *The Logic of Scientific Discovery* he writes:


Science is not a system of certain, or well-established, statements; nor is it a system which steadily advances towards a state of finality. . . . We do not know: we can only guess. And our guesses are guided by the unscientific . . . faith in laws, in regularities which we can uncover-discover.<sup>39</sup>

In Salmon's system, this would probably be an act in the stage of thinking up the hypothesis, antecedent to any consideration of plausibility, but not, as Hanson might suggest a concurrent event with these same plausibility considerations.

## (2) *Creative imagination*

Hempel has written on this role of creative imagination to science, saying that:

There are, then, no generally applicable "rules of induction," by which hypotheses or theories can be mechanically derived or inferred from empirical data. The transition from data to theory requires creative imagination.<sup>40</sup>

He goes on to discuss two classic examples from the history of science in which imagination played a most interesting role. The first is Kepler's study of planetary motion, a study which was inspired by the scientist's passion, almost obsession, to demonstrate a mystical doctrine about numbers, and to demonstrate the music of the spheres. The second example involves the chemist, Kekulé, and his dream of snakes that led him to arrive at the structural formula (  ) for the benzene molecule (C<sub>6</sub>H<sub>6</sub>). Thus, in the endeavour to arrive at a solution of a problem, a scientist may often give free rein to his imagination and, in the course of his creative thinking, may be influenced even by scientifically questionable methods. This same type of creative imagination plays a similarly



important role in non-empirical sciences, such as mathematics, for here, too, it is the results which are validated exclusively by deductive reasoning. In other words, the rules of deductive inference are not, in mathematics, a logic of discovery, for if we are given a set of statements or premises, the rules of deduction give no direction whatever to the inferential procedures to be employed. Moreover, these rules do not single out any one statement as the only possible derivable conclusion to be obtained from the original premise(s), nor do they lead necessarily to interesting or systematically important conclusions; they are not a set of mechanical routines or postulates for deriving significant mathematical theorems. Hempel says:

. . . . The discovery of important, fruitful mathematical theorems, like the discovery of important, fruitful theories in empirical science, requires inventive ingenuity; it calls for imaginative, insightful guessing. But again, the interests of scientific objectivity are safeguarded by the demand for an *objective validation* of such conjectures.<sup>41</sup>

Furthermore, Hempel states that once a mathematical proposition has been proposed as a conjecture, its proof or disproof still requires imagination and further conjecture, often of a higher order. Salmon appears to be in agreement here with Hempel, when he writes:

I should think it is psychologically more realistic to regard discovery as a process involving frequent interplay between unfettered imaginative creativity and critical evaluation.<sup>42</sup>

A. N. Whitehead also speaks of the creativity of mathematics (and also makes a passing reference to the main argument of this thesis):

The Science of Pure Mathematics, in its modern developments, may claim to be the most original creation of the human spirit. Another claimant for this position is music. But we will put aside all rivals, and consider the ground on which such a claim can be made for mathematics. The originality of mathematics consists in the fact that in mathematical science connections between things are exhibited which, apart from the agency of human reason, are extremely unobvious. Thus the ideas, now in the minds of contemporary mathematicians, lie very remote from any notions which can be immediately derived by perception through the senses; unless indeed it be perception stimulated and guided by antecedent mathematical knowledge.<sup>43</sup>

(3) *Intuition and ingenuity:*

Hempel states that scientific hypotheses and theories, rather than being derived from observed facts, are invented in order to account for the data. This inventiveness involves a certain amount of guessing at relationships that might exist between the phenomena under study, and of the uniformities, regularities and patterns that may underlie their occurrence. Furthermore, this inventiveness, often resembling "happy guesses," requires great ingenuity, often involving, as in the case of the formulation of relativity theory and quantum theory, a radical departure from current modes, or as T. S. Kuhn would term "paradigms," of scientific thinking. Phrased in another manner, one might say that, in science, for fruitful endeavour to occur, a certain intuition and appropriate contempt for authority is likely to be required.

In terms of this notion of intuition, it is appropriate to quote again, in part, the following by Scheffler:

Scientific habits of mind are compatible with passionate advocacy, strong faith, intuitive conjecture, and imaginative speculation.<sup>44</sup>

(4) *Selectivity, critical evaluation, and role of prior knowledge*

Inventiveness ingenuity and intuition, even creative imagination and speculation already discussed, seem to be related, in some way, and to varying degrees, with prior knowledge. Moreover this knowledge need not be understood in a narrow sense, i.e., knowledge of only one field in depth, but more in terms of a broad but secure grasp of several fields.

Hempel takes up this theme when he intimates that the inventiveness of scientific research will benefit from a solid familiarity with the current knowledge in the field. As Pasteur has intimated, in the realm of observation, chance favours only the prepared mind.

A complete novice, such as an historian (to use a rather extreme example) would hardly be expected to make important scientific discoveries (such as those made by Pauling regarding sickle-cell anaemia). However, more realistically, and in an even more specific sense, an animal geneticist, for example, might be grappling with a problem slightly outside his field, such as the biochemical genetics of viruses, *strain X*, (assuming for argument that these viruses have attacked certain animals in which he has been studying genetic diseases). He might, for example simply enclose himself by his own favorite theories and methods (derived, for example, from knowledge of animal genetics), duplicate what has been tried before, or simply run against well established facts or theories of which he is unaware. This brings us to another slightly different case, that of the scientist (not a complete novice) who, being new to a special field (such as a biochemist doing genetic research), often is at an advantage in that he is able to stand slightly apart from the problem and

take a fresh, new look that may well lead to a significant hypothesis or theory.

It may be seen, then, that in the general view of most philosophers of science, there is no logic or algorithm mechanically leading to scientific discoveries, and this is in no way inconsistent with the fact that the scientific enterprise (formal sciences excluded) is empirically based. The inductive perception of phenomena in the universe of experience does not guarantee the formulation of an hypothesis, for these latter, as has been argued, are freely created. One must not blur or conflate the inductive process on which empirical science is based with the discovery process (special sense), i.e., the formulation of hypotheses. The former may lead to the latter, but is not equivalent to the latter. This distinction will be discussed further in Section VI.

## SECTION V

*Alternative suggestions and "working notions" of a logic of discovery which "logics" are not strictly in accord with the term as it is traditionally employed but which have definite heuristic value. The logic of heuristic plausibility inferences. The approach of C. S. Peirce. Kaplan's distinction of "reconstructed logic" and "logic-in-use." Consideration of models as a logic of discovery.*

*The heuristic role of models. Inferring techniques are linked with models in the context of discovery. Evidence of the explanatory power revealed in the model's ability to explain higher-level phenomena. Refraction. Mathematical models and theories. Mathematical models traditionally have had a physical model "understood," which physical model may be a redundancy. Mathematical models in the social sciences. Advantage of mathematical models. Recapitulation: role of models in the context of discovery.*

*"Model" as "logic of discovery": an analogy is drawn with Hume's problem of induction showing the weakness of this notion. Limitations of models:*

- (a) *overemphasis on symbols*
- (b) *overemphasis on form*
- (c) *oversimplification*
- (d) *overemphasis on rigour*
- (e) *map reading*
- (f) *pictorial realism*
- (g) *accord with data.*

There does appear, however, to be a more promising approach for what may be termed a rule-centered logic that works for some, if not all, cases of scientific discovery (Polya 1945; 1965 and Durbin 1968). This "logic of heuristic plausibility inferences" is centered around the evaluation of credibility, and its fundamental logical pattern is:

$$\begin{array}{l}
 p \supset q \\
 q \text{ is true} \\
 \therefore p \text{ is more credible}
 \end{array}$$

This type of logic is not reducible to either standard deductive logic or to traditional inductive logic for, on the one hand, its conclusions do not necessarily follow and, on the other, it relates to the formulation of hypotheses rather than to their subsequent acceptance or rejection through procedures of verification. In relation to the problem

of discovery, it is more closely associated with the assignment of a degree of plausibility to an hypothesis after or while it is being formulated, but prior to its submission to the context of validation. Of course, the proponents of this approach do not try to claim that this logic provides a mechanical, logical means for the generation of new hypotheses in science. It is, simply, a heuristic device, and in the final evaluation, a distinctive heuristic application of logical procedures is not enough to establish a distinctive logic as such.

Charles Sanders Peirce's approach has also not been very successful, for although he introduced the term "abduction" to refer to the logical process used in arriving at a plausible hypothesis, he has not expanded on this method; indeed, it appears that all he has accomplished is the labelling of the mental process that leads to what Hanson has already called "plausibility considerations." Nevertheless, it could be claimed that his contribution was in trying to step outside the framework of deductive or inductive logical approaches to discovery, and to suggest the need for a new look at the problem.

Another approach is possible by applying Kaplan's distinction between "logic-in-use" and "reconstructed logic." He writes:

Now the word "logic" is one of those, like "physiology" and "history," which is used both for a certain discipline and for its subject matter. We all have physiologies and histories, and some of us also think and write about these things. Similarly, scientists and philosophers use a logic--they have a cognitive style which is more or less logical--and some of them formulate it explicitly. I call the former the *logic-in-use*, and the latter the *reconstructed logic*.<sup>45</sup>

Yet, is it necessary to view standard deductive and inductive logics as the only acceptable ones, notwithstanding the undeniable success of the former as manifested in scientific inquiry by the so-called "hypothetical-deductive method?" Kaplan goes on:

The myth of a "natural logic," defining a universal rationality, has been penetratingly analyzed by Benjamin Lee Whorf and his successors among linguists and anthropologists. Not only language and culture affect the logic-in-use, but also the state of knowledge, the stage of inquiry, and the special condition of the particular problem.<sup>46</sup>

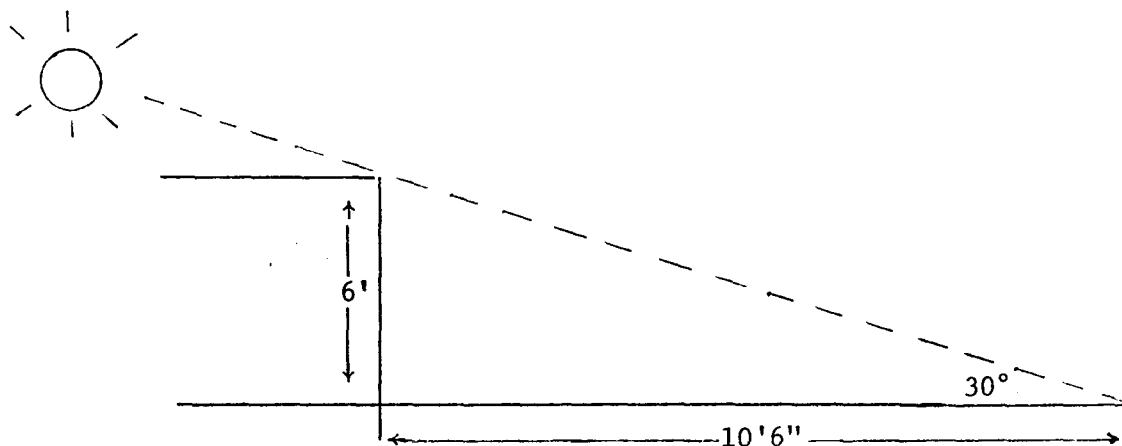
Against the background of this notion of "reconstructed logic," then, one may wish to focus on the heuristic value of models. Logics function as inference techniques, and if one also agrees that models are metaphors plus a calculus of inference, then models may be viewed as (reconstructed) logics. Moreover, if they function significantly in the context of discovery, then perhaps a workable notion of a logic of discovery is possible.

Consider a specific example to illustrate this heuristic role of models. Suppose the following is observed (following Toulmin)<sup>47</sup>

- (i) there is a wall six feet high;
- (ii) the sun is shining;
- (iii) the length of the shadow cast by the wall is ten feet,  
six inches;
- (iv) the angle of elevation is measured at 30°

As discussed in Chapter Two, the aim of inquiry is explanation. The question that comes to mind, when confronted with these data, is

how can one explain why the shadow is 10 feet 6 inches long. A diagram such as the following can be drawn:



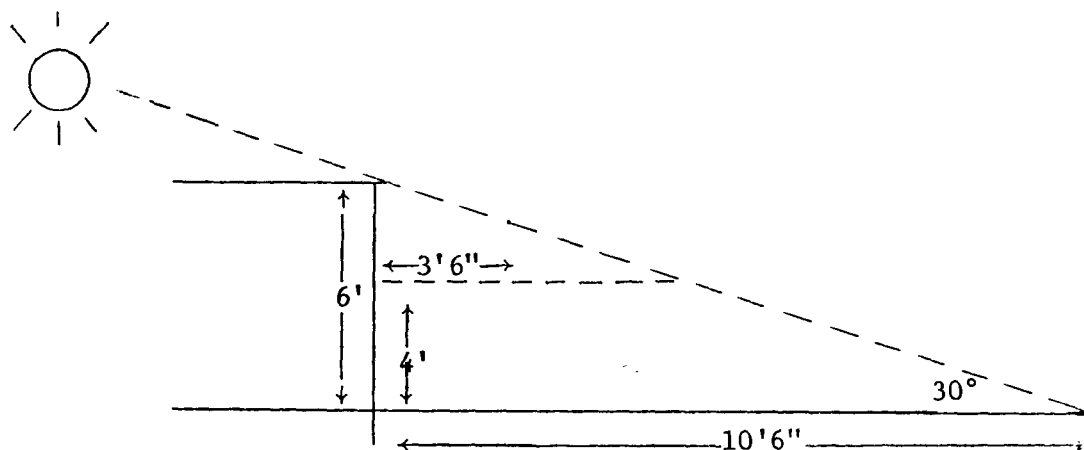
Now, if it is assumed that light travels in straight lines, and given that the wall is 6 feet high, and that the angle of elevation of the sun is  $30^{\circ}$ ; then the shadow must be ten and a half feet deep, and this conclusion follows from the way in which light has been represented. This representation is a model of the way in which light does travel (and hence may be called a Model of the Rectilinear Propagation of Light). This may not be the only explanatory model for light (and it is not) but it does explain the phenomenon presented, and its acceptance marks the introduction of the explanatory techniques which go to make up geometrical optics, viz., the model of light as something travelling in straight lines from a source, and the use of geometrical optical diagrams to infer what phenomena are to be expected in any given circumstance. It is in fact, a conceptual discovery (as opposed to an empirical one), or, as Blackwell would say, a "discovery why" rather than a "discovery that." One is led to look at a familiar phenomenon in a new way, not at new



phenomena in a familiar way. Moreover, a vital part of the model is the drawing of "pictures" (the inferring technique) so that they fit the facts, but the model need not fit all the time. For example, if under some circumstances, such as refraction and diffraction, one needs to use a model of light travelling as waves, or even as particles, these phenomena will not destroy the range to which the rectilinear model is applicable. One simply will have two additional, equally useful models for explaining phenomena not included within the class for which the first model serves. (See Appendix G). The core of discovery as represented in our present example, seems to be (a) the development of a technique for representing optical phenomena which was found to fit a wide range of facts, and (b) the adoption, along with this technique, of a new model, or a new way of regarding these phenomena, and of understanding why they are as they are. In the rectilinear model, one does not necessarily expect to find light atomized into individual rays; one simply represents (i.e., re-presents) it as consisting of such rays, and the diagram we draw, as Toulmin would argue, plays an indispensable part in the explanation.

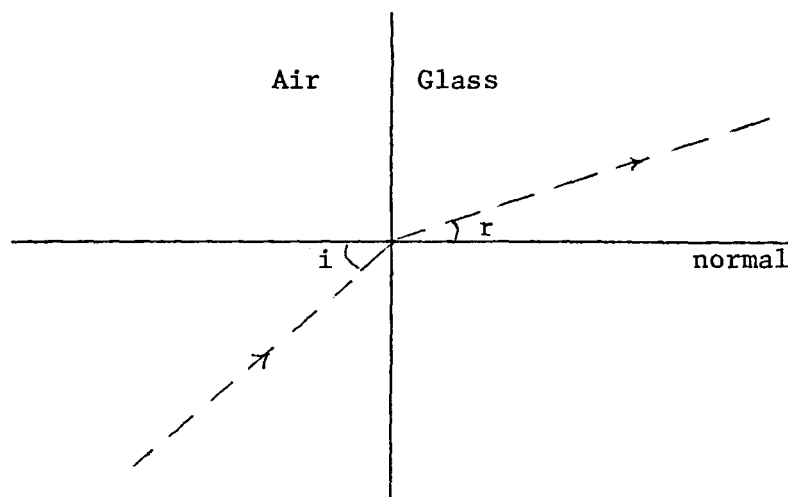
However, as mentioned earlier, the model (of rectilinear propagation of light) should suggest further test implications; and, in fact, it does, for one may then go on to ask what depth the shadow would be at the height of four feet. The calculus of inference, in this case, plane geometry, would give the answer to be three feet, six inches.

Again, if one were to assume the angle of elevation to be  $15^{\circ}$ , then the depth of the shadow is calculated as 22 feet, 4 inches. It



appears, then, that there is no limit to the number of such questions which this inferring technique (i.e., ray diagram) and the model of the rectilinear propagation of light can be used to answer.

However, further evidence of the explanatory power of this model can be seen in its ability to explain higher-level phenomena which may be stated in more general terms than the rather specific example above. For example, it may be observed that, when light travels from one medium to another, it bends, a phenomenon that is called refraction. This is diagrammed below:



Now, representing the phenomenon as the bending of rays of light, a relationship obtains between the two angles which is stated by Snell's Law:

Whenever any ray of light is incident at the surface which separates two media, it is bent in such a way that the ratio of the sine of the angle of incidence to the sine of the angle of refraction is always a constant quantity for those two media.

This law may be stated mathematically as:

$$\frac{\sin i}{\sin r} = c$$

where "c" is the constant quantity. Although in a few cases where this relationship appears anomalous, it is general enough to be called a law, and the constant quantity for refraction out of air into the substance is called the refractive index of the substance. Furthermore, it can be seen that the lines drawn in the diagram do not necessarily stand for individual "things" or, as Blackwell would probably say "physical facts," in the state of affairs represented, for the model derives its meaning as much from our diagram as from the phenomenon represented.

A further point comes to the surface here, and this concerns the language, for in the model of the rectilinear propagation of light, the key words, "light" and "travelling" are given new uses in the very statement of the discovery. As Toulmin says:

A crucial part of the step we are examining is, then, simply this: coming to think about shadows and light-patches in a new way and in consequence coming to ask new questions about the questions like "Where from?," "Where to?," and "How fast?," which are intelligible only if one thinks of the phenomena in this new way.<sup>48</sup>

In essence, then, one is required to extend the notions of "light" and "travelling."

"Pictorial" models are not the only type used in the conduct of inquiry, for mathematical models appear to be playing an increasingly prominent role. As a matter of fact, the logical role played in geometrical optics by "diagram-models" is taken over by less primitive kinds of mathematics, but which often are, nevertheless, very sophisticated and complex when juxtaposed to the former type. They play a comparable role, for the mathematical models still serve as techniques of drawing inferences. In dynamics, for example, the equations of motion of the system of bodies under investigation correspond to the geometrical optical diagram, and these equations, as Toulmin claims, can then be thought of as providing in a mathematical form, a 'picture' of the motions of the system, logically parallel to that which our diagram gives of optical phenomena.

But what is meant when one speaks of a mathematical model or theory? For example, the mathematical theory of free falling bodies under the influence of gravity is represented by:

$$\frac{dv}{dt} = g \text{ where } v \text{ is the velocity}$$

t is time

g is the gravitational constant,

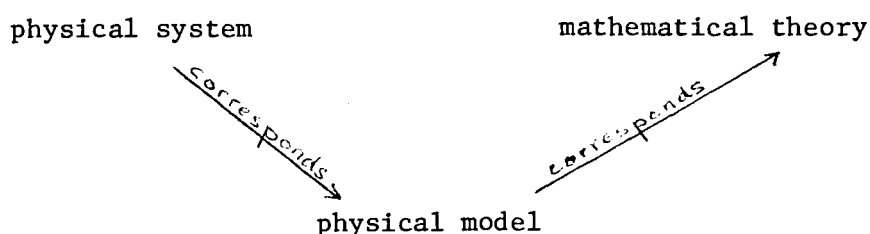
of which integration of this equation gives:

$$v = gt = v_0 \quad \text{or} \quad s = \frac{gt^2}{2} + v_0 t + s_0$$

where  $v_0$  is original velocity, and s and  $s_0$  refer to distance.

Now, what is meant by saying that these equations contain a theory of the falling body is that the numbers obtainable by the physical manipulations satisfy the equations when substituted into it. In general, then, one might say that a mathematical theory enables us to set up correspondence between the mathematical quantities (symbols) of the equation and the measured quantities of experience.

Traditionally, mathematical models have had a physical model "understood," as, for example, in the kinetic-molecular theory of gases which, in the background, "relied" on a model consisting of idealized molecules even though such simple idealizations (as revealed by spectrum analysis) were known to be false. Nevertheless, the molecules were simplified in order to make them amenable to mathematical treatment; but, at the sake of redundancy. In fact, what one has (for the model is still in use today) is a type of double theory: a mathematical theory of the idealized model, and also a physical theory consisting of the statement that there is a corresponding relationship between the idealized model and the actual physical system. The reason, of course, in constructing the idealized model is that it does possess a mathematical theory simple enough to be manipulated. Thus one has:



Yet, there appears to be a redundancy here, since a correspondence to a correspondence is also a correspondence. It seems that the

"intermediate" step, the physical model, is a technique so deeply embedded in the conduct of inquiry that the model tends to become identified with the actual physical system.

However, this intermediate step involving the physical model is becoming a less frequent manoeuvre for just such an absence of a physical model has led to, particularly in the work of Dirac, what seems to be brilliant success in the mathematical treatment of wave mechanics. What one now has are mathematical models with greater theoretical power and this is, in turn, due to the vastly greater wealth of possibilities among the structures of mathematics than in the physical models which one can visualize, and which have a simple enough mathematical theory.

Nevertheless, despite these advantages, something appears to be missing when mathematical models are employed. Upon analysis, one is likely to discover that it is the explanatory power that seems to be lost. A mathematical model, it can be argued, cannot be visualized in the same sense as a physical model can be, especially if one conceives of explanation-understanding as the analysis of a complex system into simpler systems so that one may recognize in the larger system the interplay of elements already so familiar that one does not consider them in need of explanation. A theory using a physical model is at the same time an "explanation" if one can find in the physical system the counterpart of assumed elements in the physical model. For example, if one considers the kinetic-molecular theory of gases, it might be argued that its "superiority" over the mathematical model (consisting of the first and second laws of thermodynamics and a characteristic function or two)

lies in the fact, from the point of view of explanation of the behaviour of a gas, that there is evidence from other phenomena of the existence in the gas of molecules as postulated in the physical model. This brings one back to the problems of discovery, ontology, experience, measurement, and probability, to name only a few.

The use of mathematical models has also been taken up by social scientists, but with, as yet, only limited success. In the various disciplines of social science, one characteristically encounters, as Mas Black states, such expressions as "mapping" an "object system" upon one or several so-called "mathematical systems," or "mathematical models." He goes on to say:

When used unemphatically, "model" in such contexts is often no more than a pretentious substitute for "theory" or "mathematical treatment. Usually, however, there are at least the following three additional suggestions: The original field is "projected" upon the abstract domain of sets, functions, and the like that is the subject matter of the correlated mathematical theory; thus social forces are said to be "modeled" by relations between mathematical entities. The "model" is conceived to be *simpler* and *more abstract* than the original. Often there is a suggestion of the model's being a kind of ethereal analogue model, as if the mathematical equations referred to an invisible mechanism whose operation illustrates or even partially explains the operation of the original social system under investigation. This last suggestion must be rejected as an illusion.<sup>49</sup>

The advantages of the mathematical theory, allows one

- (1) some precision in the formulation of relationships;
- (2) ease of inference based upon mathematical calculation;
- (3) intuitive grasp of the structures revealed (for example, the emergence of, say, a "logistic function" or a "parabolic function" as an organizing device).

However, as referred to previously, there are some dangers involved. For example, the oftentimes drastic simplifications demanded for effecting a successful mathematical analysis entail a very serious risk of confusing accuracy of the mathematics with the strength of empirical investigation in the original field. As in the case of the previously mentioned example of the theory of free falling bodies, ( $dv/dt = g$ ), it is especially important to remember that the mathematical treatment really furnishes little by way of explanation, for as Black would contend:

Mathematics can be expected to do no more than draw consequences from the original empirical assumptions . . . We may say, if we like, that the pure mathematics provides the form of an explanation, by showing what kinds of functions would approximately fit known data . . . In their inability to suggest explanations, "mathematical models" differ markedly from the theoretical models . . . 50

Summarized below is the heuristic role of models--mathematical as well as pictorial--in the context of discovery:

- (i) models serve as techniques of inference-drawing, but generally not of the syllogistic type;
- (ii) the basis of all major discoveries, (in the physical sciences at least), is the discovery of novel methods of representation, and so of fresh techniques by which inferences can be drawn, and, moreover, drawn in ways which fit the data. Models are especially amenable to those criteria of representation;
- (iii) the guiding criterion for employing models in scientific inquiry is that of explanation. In addition, Toulmin says



that prediction and representation are further criteria that justify the use of models. Those fertile questions and hypotheses that a particular model suggests will, however, be found in the course of actual research.

- (iv) the model should suggest further questions, taking the researcher beyond the phenomena from which he began, and suggest hypotheses which may turn out to be experimentally fertile.

The use of models as a logic of discovery has limitations, for the choice of types of models is prior to the use, and there is no algorithm or deductive calculus that leads to the "correct" or "most suitable" one. This is similar to Hume's problem regarding induction, for, if a model  $X$  is chosen, there is no logical guarantee that model  $Y$  could not be a better or more suitable one. Or, to phrase the problem another way, if one chooses to investigate  $n$  number of models, and finds one that is suitable, there is no logical guarantee that the next model,  $n+1$  would not be a better or more suitable one. It appears, then, that the method for arriving at the model for a particular problem is nonexistent, although once a model is chosen it can function as a calculus of inference within the context of discovery by "forcing" the researcher to look for isomorphisms, if such are available.

Employment of models at any time entails a number of dangers arising from their intrinsic limitations. Some of these will now be discussed, and should be related particularly to the problem of discovery.

- (a) Overemphasis on symbols: perhaps based upon a certain mystique in the "magic" of symbols; the symbolic style is often only a manner of speaking, and, although it may confer on a proposition a certain scientific usefulness, with regard to form, it does not necessarily follow that the content is so rendered. "The essence of mathematics is not its symbolism, but its method of deduction."<sup>51</sup>
- (b) Overemphasis on form: models are easily constructed, although they may not always be useful in a given state of knowledge, and, indeed, may impose a premature closure on one's ideas, thereby limiting the awareness of unexplored possibilities of conceptualization. Indeed, one may allow the models to usurp the importance of the ends they are meant to serve. Incorporating subject-matter in a model does not automatically give such knowledge scientific status. It is easy to fall into the habit of building models for the sake of building them.

For example, important concepts such as "personality," "culture," and "ideology" may, by imposing an explicit definition, in fact limit their application. On the other hand, it is possible to fake a closure by using symbols (for example, "P," "C," "I,") but their openness of meaning will become apparent in their interpretation, no matter how sophisticated the mathematics or other symbolic relations may become.

In the context of educational research, these same problems apply, and a stipulative definition for "achievement," which in itself may be

dependent upon somewhat arbitrary, but again stipulative definitions of "creativity," "originality," and "rote learning" may in fact prematurely close or limit their applicability. On the other hand, by using symbols such as "C," "Or" and "Lv" one does not have a sufficient guarantee that these same concepts have been "closed enough," and their vagueness cannot in the long run be masked by symbols and/or sophisticated equations.

This raises the problem of artificial languages, which in behavioural sciences is often doubly artificial. Kaplan says:

Like other devices contrived to fill what is thought to be a deficiency of nature, it may look even better than the real thing, but it is usually disappointing in its performance.<sup>52</sup>

Kaplan then goes on to state that one must not lose sight of the value of empirical materials, and that behavioural scientists should perhaps be more concerned with subject-matter, and less concerned with whether or not one is building a "Science."

- (c) Oversimplification: When models are virtually useless, the failing which is chiefly responsible is likely to be oversimplification, for one may in fact be mistaking the notion of "simplicity" in science (i.e., to formulate only what is essential for understanding, prediction, or control). It is possible that one may have simplified in the wrong way or in the wrong places. Also, one may have neglected something important for the purposes of the model ("under-complication.") Kaplan uses the example of drawing

colored balls from an urn, which, while despite the simplification involved, may serve as a useful model in genetics. However, it may be an inappropriate model for an actual game of cards. As Kaplan states: "In short, a crude but more realistic set of hypotheses may serve the purposes of inquiry in particular cases far better than a refined but oversimplified model."<sup>53</sup>

- (d) Overemphasis on rigor: Models may be improperly exact, calling for measures that one cannot in fact obtain, or that one would not know how to use even if one did obtain them, (e.g., Hedonic calculi vs. mathematically cruder but empirically more subtle and sophisticated accounts of motivation). Kaplan states that many postulational and formal models have no remarkable deductive fertility. He also maintains that where there is deductive fertility, there is not necessarily a corresponding heuristic fertility (i.e., of being rich in implications for further observation, experiment, or conceptualization). As Kaplan says: "Careful observation and shrewd even if unformalized inference have by no means outlived their day."<sup>54</sup>
- (e) Map reading: This results from failure to realize that not all features of a model correspond to some characteristic of its subject-matter. In a sense, this is the obverse of the error of oversimplification. A model always has some irrelevant features in virtue of the isomorph, (for

example--supposing that the British Empire is everywhere pink because the map so depicts it).

Within the context of social science research, and probably at least equally, or even more so, within the perspective of educational research, formulations of theories using such concepts as "learning," "teaching," and "education," are often effected with only a sketchy idea of the logical structure of what Rudner would call "the empirical theory of primary concern." Without, at this point, dwelling on possible heuristic import of such educational models, Dewey's growth model or the "activity movement" model in education fall into precisely this same error; that is, the assumption of complete isomorphism when it is, in fact, only partial. When employing such models in education one must always be aware that, given an empirical or mathematical theory, no matter how beautifully it seems "to fit," one must, at the same time be cautious enough to admit that parts of the logical structure may be ignorant to him. In other words, one's confidence in employing the model varies without knowledge of its isomorphism with the theory of primary concern. When one is so ignorant, to translate from the theorems of some system that one is guessing to be a model, such as the growth model or the moulding model, is to risk attributing to the theory of teaching/learning implications that, quite literally, the theory does not have. This danger is present just as long as the required isomorphism between model and theory remains undemonstrated.

- (f) Pictorial realism: Consider the distinction between exogenous and endogenous variables. Those variables irrelevant

to the structure of the model are termed exogenous (for example, coloring in the countries of the British Empire pink on a map--this relates to the error of map reading, previously discussed, which latter error is the failure to distinguish between the two variables, and further, treating as relevant to the model some properties that are irrelevant). Moreover, even if the endogenous properties are properly identified as such, one may still misconceive them as constituting an image or likeness of what is being modeled (for example, Mercator map projections yield good maps, though they distort the polar regions, and the distortion, endogenous to the model, should not be conceived as an image of what is being modeled). Especially if one thinks of a model as a scientific metaphor, one must avoid "that's what it is" instead of "that's what it's like."

Braithwaite also discusses this problem, especially in the light of the earlier discussion of "model" as an alternative interpretation of a calculus. All that is required in the relationship of model to theory is similarity of formal structure. In chemistry, models for theories often depict molecules as linked systems of atoms, whereas in physics, models for theories often depict atoms as "solar systems" of separate elementary particles such as electrons and protons. The danger lies in supposing that atoms or electrons share characteristics of the model other than those which make the model an appropriate one, although, in this particular case, the danger is less prevalent now because of new

conceptions of the atom. Nevertheless, it lies latent whenever one may choose, as perhaps in science teaching to use this model. To quote Braithwaite:

Thinking of scientific theories by means of models is always *as-if* thinking; hydrogen atoms behave (in certain respects) as if they were solar systems each with an electronic planet revolving around a protonic sun. But hydrogen atoms are not solar systems; it is only useful to think of them as if they were such systems if one remembers all the time that they are not. The price of the employment of models is eternal vigilance.<sup>55</sup>

The imposing of empirical features onto the concepts of the model is only one danger, for an even more subtle one is that of transferring the logical necessity of some feature of the chosen model onto the theory, and thus of supposing, that the theory, either in whole or in part, has a logical necessity which is, in fact, fallacious.

Rudner also discusses this danger, for when one uses such empirical models as the hydraulic model for electrical current flow, or the pigment-mixture model for the humors theory of personality, one need not assume that the axioms and theorems of the model are exhaustive, for, indeed, one does know very much more about water than merely how it behaves in conduits. Similarly, one knows much more about paint pigments than about the colours they yield in certain combinations. As Rudner writes:

But in employing the water-conduit theory, or the pigment-mixing theory as models, it would be quite unjustified to translate any information not actually deducible in those models into the theory of concern, no matter how familiar with, or how confident of, this extrinsic information we might be. To take a final example of this point, if we are employing some theory about body-temperature homeostasis in whales as a model for a theory about some homeostatic characteristic of modern political states, it would

be fallacious to translate our extrinsic knowledge of the way in which whales care for their young into any corresponding hypothesis about the way in which modern states deal with their colonies (no matter how well-confirmed our knowledge of, say, suckling behaviour of whales may be).<sup>56</sup>

He then goes on to say:

When the model relationship does obtain, we are, so to speak, guaranteed that implications of the model have corresponding implications in the theory of concern. But this guarantee extends only to implications *in the model* and not to any exterior knowledge of the model's subject matter.<sup>57</sup>

Rudner terms this error the fallacy of unwarranted exploitation of extrinsic substantive knowledge, and, he warns that, in the case of formal models, the danger results from an unwarranted transfer of certain formal characteristics of the model's statements to presumed corresponding statements in the theory of primary concern. He says:

In particular, the a priori characteristics (for which the term 'analyticity' or 'logical necessity' are generally used) of statements of an analytic model are not routinely transferable to their corresponding statements in the theory of concern. While the temptation to make this kind of transfer to *observation* statements (those referring to observable features of the world) of the theory might be slight, the temptation to make the transfer to *theoretical* statements of the theory must be, perhaps, more vigilantly guarded against.<sup>58</sup>

- (g) Accord with data: Kaplan points out that models in behavioural science cannot be expected to fit the data exactly because some of the relevant variables are likely to have been omitted, and the variables which are brought into the analysis are not likely to be measured with great



exactness. Probabilistic considerations thus assume considerable importance. One must strive to maintain the "integrity of the data"--to make the models fit the data rather than assuming that behaviour has just the properties needed for the models to be mathematically workable (e.g., assumptions such as normal distributions, linear functions, additive operations and forces independent of action).

## SECTION VI

*(Philosophical II) Exegesis of Blackwell's theory of discovery. Introduction. Where is the process of discovery? Presentation of related difficulties. A theory of discovery could be a more fruitful approach, rather than the traditional emphasis on a logic of discovery. Discovery is normative. Two conditions required for establishing a scientific discovery. "Discovery that" and "discovery why." Discovery and explanation. Routine, revolutionary, and accidental discovery. Four "levels" of a theory of discovery. The adaptation theory of discovery: presuppositions and the mind-nature dualism. Adaptation. Brief recapitulation. Adaptation as elaboration of structures. Elaboration. The doctrine of structures. Elaboration and "discovery that." Adaptation as transformation of structures. Transformations vis-à-vis conceptual frameworks. Theoretical entities. Elements of transformation. Theoretical discovery. Recapitulation.*

*Analysis of Blackwell's theory of discovery. Symmetrical and asymmetrical binary relationships. The asymmetry in Blackwell's theory. Blackwell's treatment of the mind-nature dualism. The Cartesian and Spinozistic stands. A second look at Blackwell's asymmetry. Blackwell's theory: some further considerations. Sources of weakness in the theory. Concluding remarks.*

### Exegesis:

The problem of discovery has been a traditionally neglected one, and yet the process seems to be potentially significant for the conduct of inquiry. In his recent work, *Discovery in the Physical Sciences* (1969) Professor Richard Blackwell argues that in order to get the program off the ground one must postpone for the time being the traditional formulation of the problem, with its emphasis on the "logic" of discovery, and develop, instead, a "theory" of discovery that will enable at least a partial understanding of this vital process.

This section begins with an exegesis of Blackwell's theory after which a logical analysis is performed which establishes an asymmetry between the two basic theoretical constructs, "discovery that" and "discovery why," and this serves as the genesis for a subsequent examination of the reason(s) that give rise to this relationship. This leads into a discussion of the epistemology of discovery, and an argument in favour of Blackwell's stand as a possible solution to the problems as posed by the Cartesian and Spinozistic positions on the mind-matter dualism. In addition, Blackwell's refutation of Hume's epistemology is also supported.

Having argued for the conceptual foundation of Blackwell's theory, i.e., his treatment of the mind-matter dualism, the thesis returns to a second look at the asymmetrical relationships of this theory as thereby generated, with the argument that this kind of relationship, implicit though it may be, allows one to gain some special insights into the process, such as the viewing of scientific discovery as a form of creativity.

Finally, the section concludes with an evaluation of the theory as a whole.

Blackwell begins by locating the process of discovery as somewhere along the transition from data to the generation of a hypothesis. As has already been discussed in Section IV, he also notes that there appears to be no discernible formal pattern(s) of logical inference in going from data to hypothesis, and this has led most philosophers to conclude there is no logic of discovery. A quotation from Hans Reichenbach supports this claim:

The act of 'discovery escapes logical analysis; there are no logical rules in terms of which a 'discovery machine' could be constructed that would take over the creative function of the genius. But it is not the logician's task to account for scientific discoveries; all he can do is to analyze the relation between given facts and a theory presented to him with the claim that it explains these facts. In other words, logic is concerned only with the context of justification.<sup>59</sup>

Karl Popper also is in agreement, for he says:

The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis, nor to be susceptible to it. The question how it happens that a new idea occurs to a man--whether it is a musical theme, a dramatic conflict, or a scientific theory--may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.<sup>60</sup>

Many of the difficulties of the present problem reside in the great inexactitude with which it is approached. For example, what does one mean by "logic" in this case? Does one mean only deductive, formal logic, or may one also include inductive logic and probability inferences also? Does one simply mean reasonable and understandable explanation? It might

be suggested that discovery is purely a psychological matter, or is this only a convenient way out of discussing it? Finally, what are the objects of discoveries--facts, empirical laws, theoretical entities and their interrelations?

It is not expected that all these related problems can be solved with finality, although many of them will be answered in the course of the discussion. However, Blackwell does conclude these preliminary remarks by cautioning against using the term "logic of discovery" in the sense of a formal algorithm, and he then goes on to assert that a theory of discovery may be another, perhaps more fruitful approach to the problem.

Blackwell contends that "discovery" carries an honorific connotation. Also, two conditions which *X* must satisfy if it is to be counted as a discoverer are:

- (i) *X* must become part of the accepted body of reliable scientific knowledge, and
- (ii) *X* must be of relatively permanent value.

There are two basic types of discovery, viz., "discovering that" and discovery why." The former type refers more to the determination of the physical state of affairs, and contains fewer conceptual implications; the latter presupposes the former, is primarily interpretive, requires integration into an over-all structure of human knowledge, and implies that an explanation is sought. For example, it might be said that Kepler discovered

that the planets move in certain ways, while it was Newton who discovered why they so do.

It seems that both these fundamental types of discovery are, in fact, two sides of the same coin, although one or the other may be the more prominent in a given instance. Nevertheless, in each case new knowledge is produced that is judged to be a discovery in terms of its acceptability into the current body of scientific knowledge.

As a process, "discovery that" leads to the statement of the explanandum, whereas "discovery why" is that process which uncovers the explanans needed for the explanandum. However, if one is considering the problem of discovery, one is still left, as it were, in the cold, for there is still not a satisfactory answer to the question of the origin of the explanans and the explanandum. What one does have is simply a logical relationship between the components of explanation and the process of discovery.

Going still further, one might say that, epistemologically, "discovering that" is empirically oriented, whereas "discovering why" is theoretically directed. For example, if one observes that the pressure of a given volume of gas under standard conditions of temperature and pressure (i.e., not extremes of these variables) vary directly and indirectly respectively, the following relationships may be formulated:

$$(i) \quad v \propto \frac{1}{P} \quad \text{and} \quad (ii) \quad V \propto T,$$

which may be converted into:

$$(iii) \quad V = \frac{k}{P} \quad \text{and} \quad (iv) \quad V = kT.$$

The former equation is Boyle's Law, and the latter is Charles' law which laws may be combined, and commonly labelled "The Perfect-Gas Law":

$$(v) \quad V = \left(\frac{1}{P}\right)(T)(n) \quad \text{or} \quad PV = nRT,$$

where  $n$  is the number of moles, and  $R$  is the gas constant.

However, the explanation of the behaviour of gases according to these laws is effected by employing non-observable small particles as postulated in the kinetic-molecular theory. In this sense, then, "discovery why" is more theoretically oriented.

Consider another way of classifying types of discoveries. The history of the discovery of the neutrino is an example of what may be termed routine discovery. The electrons emitted in the  $\beta$ -ray decay of a nucleus presented two problems to physicists:

- (i) If a comparison of energies is made before and after emission, and in those cases where the energy of the emitted electron is less than the predicted value, the difference represents an unaccounted for loss of energy, a situation which would tend to refute the law of conservation of energy.

- (ii) According to quantum theory, the emitted electron is the product of the transformation of a neutron into a proton, and this transformation requires a change of the angular momentum (or spin quantum number) equal to unity. However, in  $\beta$ -decay, the spin quantum number equals only  $\frac{1}{2}$ , a situation which would tend to refute the law of conservation of angular momentum.

Fermi and Pauli, in order "to save" the conservation principles, postulated a new particle explicitly defined to make up the difference in energy and angular momentum, and in 1956, the existence of this particle, called the "neutrino," was verified for the first time.

Another example (and many more are available) will suffice to illustrate this type of discovery. The discovery of the planet Neptune resulted from observed irregularities in the motion of the planet Uranus which observations meant that the laws of Newtonian mechanics were in need of modification, or that there exists an as yet unobserved planet beyond Uranus which could account for the irregularities precisely according to these same Newtonian laws. In 1848, Adams and Leverrier, working independently, made the required calculations from Newtonian principles, and Galle in Berlin discovered the planet on his very first attempt. Routine discoveries, then, may be said to result from deductive application of established principles to new empirical evidence, or the extension of an established set of principles to new areas, or the mathematical

reformulation of a prior theory. They are made within the confines of an established conceptual framework, and may be considered extensions of, or additions to, this structure.

Revolutionary discoveries, on the other hand, involve a major conceptual revolution, a deliberate break from earlier views. For example, in 1900, Max Planck encountered the so-called "ultra-violet catastrophe" which logically follows from the explanation of black-body radiation according to classical electromagnetic theory, and which predicted that the intensity of radiation becomes infinitely large as the frequency becomes higher and higher. Planck, realizing that this catastrophe could not be averted merely by an extension of, or an addition to, the classical theory, formulated two postulates:

- (i) Radiation can be emitted from a source only with energies which are integral multiples of the frequency multiplied by Planck's constant, and
- (ii) at thermal equilibrium only a few atoms within the black body are capable of high-frequency emissions and the rate of these emissions is low.

It should be emphasized that in formulating these postulates, Planck was breaking completely from the classical assumption that radiation occurs over a continuous range of energies (for it was this continuity of the radiated energy which allowed the intensity of the radiation from a black body to build up gradually to infinite values as the frequency increased). With Planck's stipulations,



the "ultra-violet catastrophe" could be avoided, but the price, as Planck was well aware, was high, for the whole conception of the nature of electromagnetic radiation had to be changed. Essentially, what Planck formulated was a weak version of the new quantum assumption, viz., that energy is quantized at the time and place of emission, but later merges into a continuous field as depicted in the wave theory of light. Einstein's subsequent discovery and interpretation of the photoelectric effect led to an acceptance of a stronger version of this assumption, whereby electromagnetic energy is not quantized during emission only, but also remains quantized afterward. The quantum theory, then, was a deliberate break from earlier views, and resulted in a major conceptual revolution in physics. There are many other such examples, such as the special and general theories of relativity, Harvey's discovery of the functions of the heart, Pasteur's germ theory, and Newton's laborious struggle in finding the principles of classical mechanics, and the consequent rejection of, first, the Aristotelian and, later, the Cartesian views of matter and motion.

Revolutionary discoveries do not occur in isolation, but within a conceptual scheme from which they then break away. The appearance of what Kuhn calls a "scientific anomaly" is the immediate occasion for a revolutionary discovery, and the result of such a revolution is scientific growth.

Accidental discoveries (e.g., Goodyear's discovery of vulcanized rubber) are related more to conditions of observation rather than to understanding and insight which are the focal points of routine,

and especially revolutionary, discovery. Accidental discoveries are usually of the "discovery that" type, with an emphasis on fortuitous events, rather than insight.

In order to explain the process of discovery Blackwell suggests four approaches:

- (a) The Logical Approach--the traditional way, i.e., formulated as the problem of a "logic of discovery";
- (b) The Psychological Approach;
- (c) The Historical Approach;
- (d) The Epistemological Approach--this is Blackwell's approach, and the springboard of his "theory of discovery."

The logical approach is mainly represented by N. R. Hanson whose program has already been discussed. Blackwell's criticism of Hanson is essentially the same, although he goes on to differentiate two senses of "hypothesis" in accordance with his distinction between "discovering that" and "discovering why."

- (i) hypothesis (first sense)--a general statement of explanatory power. Blackwell agrees with Hanson that these kinds of statements are not produced by the methods of enumerative induction. This sense of the term is linked to "discovery why."
- (ii) hypothesis (second sense)--a general statement expressing empirical regularity, such as "all copper conducts electricity."

Blackwell argues that here the methods of enumerative induction, linked to "discovery that" are relevant.

Two additional distinctions can be made, viz., "rule-centered inference" and "content-centered inference." The former type focuses primarily on the formal structure of propositions independently of their meaning. For example, in the earlier example concerning the routine discoveries of the neutrino and of the planet Neptune, these discoveries could be classified under rule-centered inference because, as mentioned before, (and taking the Neptune example) they resulted from a deductive application of Newtonian mechanics to account for the irregularities of motion of the planet Uranus. The second type of inference (content-centered type) finds its natural home in the context of revolutionary discoveries, and the focus is primarily on the meaning-content of the statements involved. For example, relativity theory is not simply an extension of Newtonian mechanics, nor is the latter any kind of rule-centered development of Aristotelian physics. In short, a fundamental re-alignment of concepts and meaning is involved in revolutionary discovery.

The psychological approach centers on delineating how the mind operates in the formation of new ideas and although no comprehensive psychological theory has been advanced, as yet, to explain the act of discovery, this is neither to deny nor to affirm the possibility of the eventual construction of such a theory (refer to Section II). Arthur Koestler in *The Act of Creation* (1964)

describes a pattern which he calls "the bisociative act." Essentially, it is the recognition of similarities between two previously unrelated matrices of thought. Using Newton once again, an example of this bisociative act would be in the recognition that terrestrial and celestial motions, which required separate accounts in Aristotelian physics, are governed by the same laws. Koestler also differentiates between the unconscious and conscious levels of awareness, and claims that bisociation, and hence discovery, occurs through the interplay of these levels of awareness.

The chief proponent of the historical approach is T. S. Kuhn. In his fascinating book *The Structure of Scientific Revolutions* (1962) he traces the history, birth, and death of conceptual paradigms, and elucidates a similar general pattern common to all scientific revolutions. To quote Kuhn himself:

Discovery commences with the discovery of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected. Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed--until the scientist has learned to see nature in a new way--the new fact is not quite a scientific fact at all.<sup>61</sup>

When the crisis is at its peak, there is a period of freedom from older patterns of thought, and theorizing in all sorts of new directions is not only accepted, but encouraged. It is at this juncture that the act of discovery occurs. The thrust of Kuhn's

presentation, then, is the social nature of scientific thought, and the historical explanation of discovery consists in viewing the individual act in its proper place within the wider historical processes which surround it. Although one may argue that, in fact, no theory of discovery is thereby effected, Kuhn nevertheless offers at least a partial explication. (However, it may be claimed that he probably never intended to develop a theory of discovery as such.)

The epistemological approach is the basis of Blackwell's theory, and involves the investigation of the process of discovery from the point of view of the meaning-content of the knowledge involved. For example, the creative scientist freely introduces non-observable entities and events as explanations of empirical laws. Blackwell then wishes to ask: "How is this transmission made?", "what residue, if any, of physical meaning remains in such theoretical constructs?" and "what does this reveal about the process of theoretical discovery?"

Blackwell's presupposition is that the human mind and physical nature are somehow appropriate for each other, and that the act of discovery is located at the point of conjunction between these two realms of nature. The distinction between empiricism and rationalism is not as clear cut as Bacon claimed, and science does not merely passively arise from what Blackwell terms "the dictates of nature", with the mind an unobtrusive "observer." However, Blackwell would also wish to claim that science is not simply the product of the creative

mind operating as an unrestricted legislator. In terms of a mind-nature dualism, the epistemology of discovery becomes the problem of explaining how science arises from the merging of the dictates of nature and the creative functions of the mind. In the last analysis, neither physical nature nor the human mind have any meaning in isolation from each other. Furthermore, mind and nature, though not identical, exhibit distinctly different properties, and are intrinsically relational entities which possess reality and meaning only within the over-all matrix of these relations.

Blackwell's theory of discovery is founded, in turn, upon his theory of adaptation, or the process of going from nature to mind. This adaptation is comprised of two parts: elaboration and transformation. In order to facilitate understanding of these parts he draws several analogies, scattered throughout the text, and drawn together here for convenience:

- (i) *space-time analogy* to explain the dualism of mind and nature: the topological properties of space and time are different; for example, the former is unidirectional while the latter possesses causal aspects of temporal relation. Nevertheless, according to relativity theory, the reality and meaning of space and time are interwoven and interdependent. Neither is any longer viewed as a complete and self-contained entity.
- (ii) *ontogeny--phylogeny analogy* to explain the dualism of his theory: elaboration is like the gestation and maturation

of the individual organism, and transformation is likened to the process of evolution.

- (iii) *anthropological or sociological analogy* also to explain the dualism of adaptation: the study of man may be carried out at the level of the individual or at the level of community, although each separately gives only a partial picture of man. Similarly, this is true of the dictates of nature and the functions of the mind with respect to the epistemology of discovery.

Brief recapitulation: Blackwell argues that if both mind and nature are viewed as being essentially relational entities, then there is at least a foundation for an epistemologically-based approach to an understanding of discovery.

Blackwell emphasizes the interaction of mind and nature, and claims that at certain times any one side may be pre-eminent in some aspect of discovery. Also, discovery may come in a "flash of insight," but the preparation for it is long and extensive, and forms an integral part of its story (these points have been discussed previously). Two further distinctions can then be made:

- (i) complexity of the dictates of nature;
- (ii) complexity of pre-established conceptual frameworks.

Both complexities interplay with each other and discovery emerges

from this interaction. This is simply another statement of Blackwell's treatment of the mind-nature dualism.

The first stage of the theory of discovery is termed elaboration, which is defined as the process of progressive specification of physical meaning from the amorphous mind-nature confrontation resulting in the emergence of a discovered structure. This latter term refers to an individual physical fact, or a generalized, empirical law. Elaboration comprises four stages:

- (a) The sorting-out process. This is simply the identification of relevant physical factors, presupposing, of course, an anomaly, the latter of which presupposes a prior conceptual framework.

- (b) Interrelating recognized components of a structure.

Elaboration has, as its goal, the development of physical meaning which is attained in the discernment of the relational structure in nature. A scientific law is not a mere catalogue of relevant factors with a statement of their degrees of relevance. Rather, it is the interrelation of these factors into a meaningful pattern that is required, and that brings the first level of scientific discovery to successful completion.

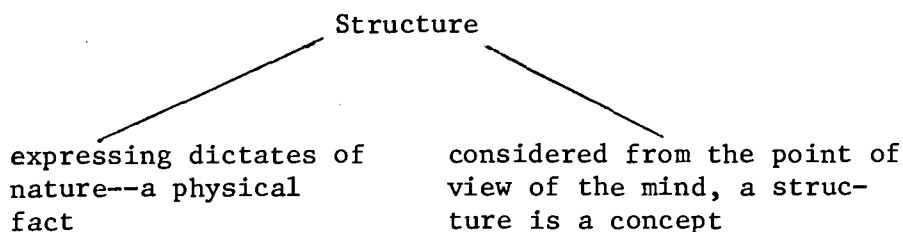
- (c) Integration with other knowledge. After a structure is discovered, it must be related to other structures already available to human knowledge. For example, mass



and force, space and time are concepts which have meanings extended far beyond their original contexts. They become elements in a complex, conceptual whole, and these positions they then hold contribute significantly, in turn, to the fullness of their own meanings. Furthermore, they influence the course of inquiry by suggesting new problematics.

- (d) Transcending actual experience. This is essentially the process of generalization, a shorthand convenience to avoid listing all actually examined instances. However, there is also a built-in proviso that exceptions may arise, and, as such, these generalizations are understood as provisional universals.

Although there is a temporal duration to the process of elaboration, culminating in a discovery, the elements in elaboration are not tempoally distinct steps. In elaboration, the dictates of nature exert predominating influence, and the discovered structure provides adequate understanding of the problem and, furhter, suggests new problems for the continuation of inquiry. Consider the following diagram:



A distinction may be drawn between empirical laws (which are discovered structures) and theories, the former relating primarily to determination of facts, and resulting from the elaboration of structures; the latter referring primarily to the interpretation of facts. Examples of a discovered structure as "fact" include Galileo's discovery of sunspots or the discovery of Neptune by Adams and Leverrier. Examples of such discovered structures as generalized empirical laws include Galileo's law of free falling bodies or Kepler's laws of planetary motion.

"Transformation" is defined as that type of mind-nature adaptation which is the process of the development of theoretical meaning. A theory is a deductively organized system of explanation in which the explanandum is a structure previously discovered by elaboration, and in which the explanans contains both other discovered structures and mentally created, non-observable theoretical entities. However, this is not quite accurate, for the explanandum is sometimes itself a theory, as in, for example, the case of the electromagnetic theory of light which is at least partially explained by the quantum theory of the atom.

As individual physical facts and generalized empirical laws proliferate, the drive for order and unity becomes more pronounced. Furthermore, when the scientist seeks the reasons why a certain physical state of affairs occurs he converts a discovered structure into an explanandum, and, as is also true in the process of elaboration, the articulation of the problem at this point is frequently a matter of genius and gifted insight.

The process of transformation also has a dual nature, for it is productive of both explanatory systems as well as theoretical entities, the latter in which the main burden of explanation falls. Furthermore, these theoretical entities always bear some residue of physical meaning.

Transformation is the occasion for the creative functions of the mind. It is comprised of three parts:

- (a) Idealization. This is perhaps the most fundamental step. It is the conversion of discovered structures into forms which are not, and often cannot be, actual physical facts. They should not be confused with the discovered structures (i.e., Whitehead's fallacy of misplaced concreteness must be avoided. See Appendix H) Examples of these theoretical entities include "point locations" and "instants of time." Another example of idealization is the "thought experiment," such as Einstein's elevator experiment to illustrate the principle of equivalence. Moreover, theoretical entities are explanatory insofar as they are placed in systematic relation to the discovered structures from which they arose.
- (b) Creative postulation. This is defined as the formulation of theoretical entities by the stipulation of a set of components which, if assumed, will enable the deduction and prediction of observed physical facts. Furthermore, the components stipulated may be taken from virtually

any available source in prior knowledge, whether scientific or not, as long as it is supposed that such components will contribute to the generation of an explanation.

This process again allows for a wide range of genius and gifted insight. The atomic theory of matter and the neutrino are examples of such postulation.

- (c) Substitution through analogy. This process is the recognition of similarities between two or more conceptual frameworks, and the subsequent transference of a theoretical explanation from one domain to another. The essential point is that prior knowledge can uncover resemblances which are suggestive of explanatory hypotheses. Another important term is "theoretical discovery," and this is defined as the grasping of an explanation of a previously discovered structure in terms of its deductive derivation from a set of theoretical entities.

#### Recapitulation:

Blackwell's arguments center around the mind-nature confrontation, the common matrix out of which all natural human knowledge arises.

He writes:

The act of discovery bears no distinctive logical structure. It does not occur in a step-by-step fashion. One cannot lay down explicit rules which, if faithfully followed, will lead to a discovery. The element of genius and gifted insight is a constant attendant of the discovery process. . . . The problem of how scientific knowledge is originally generated from experience is the task of the theory of discovery, which, even if only partially realizable, adds considerably to our understanding of the nature of science.<sup>62</sup>

Blackwell claims that a scientific object is ontologically real insofar as it exists in nature, or expresses a dictate of nature. He contrasts this with intentional reality, the mode of existence constituted by the functioning of the mind in thinking about an object. As concepts, scientific objects exist intentionally. Indeed, it may be more accurate to speak of degrees of ontological status, which would be correlated with the degree to which the scientific object expresses the dictates of nature. For example, the electron is ontologically real insofar as it expresses the dictates of nature involved in the phenomena of electrical conductivity, chemical composition, ionization, spectographic evidence, and so on.

#### Analysis:

##### Symmetrical and Asymmetrical Binary Relationships:

If  $x$  is equal to  $a$ , and  $y$  is also equal to  $a$ ,  
then

$$(i) \quad x = y, \text{ and also } y = x$$

Now, to generalize this relationship one may write:

$$(ii) \quad xRy \text{ and also } yRx$$

and such a relationship between  $x$  and  $y$  is said to be symmetrical.

If  $x$  is the brother of  $y$ , and both  $x$  and  $y$  are male, and it is true that  $x$  and  $y$  are brothers, then the relationship may be written as:

$$(iii) \quad xBy \text{ and } yBx$$

The general form of symmetry as presented in (ii) in the notation of symbolic logic as:

$$(iv) \quad (x)(y)(x \in A \cdot y \in A \cdot xRy \rightarrow yRx)$$

where  $(x)$ ,  $(y)$  are the universal quantifiers, and  $A$  is the set of which  $x$  and  $y$  are both members.

If, however,  $x$  is greater than  $a$  and  $y$  is equal to  $a$  then

$$(v) \quad x > y, \text{ and also } y < x$$

To generalize these relationships one would obtain

$$(vi) \quad xRy \text{ and } \sim yRx$$

and such a relationship between  $x$  and  $y$  is said to be asymmetrical.

If  $x$  is the father of  $y$ , and  $x$  is male and it is true that  $x$  is the father of  $y$ , then one may write the following generalized relationships:

$$(vii) \quad xFy \text{ and } \sim yFx$$

In the notation of symbolic logic then, asymmetry may be written as:

$$(viii) \quad (x)(y)(x \in A \cdot y \in A \cdot xRy \rightarrow \sim yRx)$$

Formula (viii) is applicable to an analysis of Blackwell's theory in which there is a logical asymmetry between the two fundamental constructs of the theory, "discovering that" and "discovering why." The following passages show how Blackwell uses these notions, either explicitly or implicitly.

- (a) Merely knowing that such-and-such is the case does not fully satisfy human curiosity. We would also like to know why. Consequently, a new problematic is formulated, requiring a new type of inquiry, which will result in scientific discoveries at another level.<sup>63</sup>
- (b) Nevertheless, the explanation of discovered structures themselves is the ultimate objective of all theories in science. . . .

The net result of this analysis is that there are two levels of discovery in physical science, those dealing with the articulation of structures at the level of elaboration and those dealing with the grasping of explanations at the level of theoretical transformation. The former are concerned with the determination of facts, the latter with the interpretation of facts. The former are an understanding of individual physical facts and empirical laws, the latter are an understanding of explanatory theoretical entities. The former are instances of "discovering that," the latter are instances of "discovering why."<sup>64</sup>

- (c) In fact, we have argued for a continuous gradation of such objects, from the level of common-sense knowledge through elaborated structures to the theoretical entities constructed by the mind for explanatory purposes.<sup>65</sup>

As mentioned in the exegesis, Blackwell argues that his dualistic theory, arising out of the mind-nature confrontation, gives at least an understanding of the process of discovery. If transformation of discovered structures into conceptual interpretations represents a higher step of development towards what may be termed the external evaluative criterion of explanation-understanding (and, indeed, "discovery why" presupposes "discovery that") then these two fundamental theoretical constructs are asymmetrical. One could then write:

- (i) "discovery that" = x and
- (ii) "discovery why" = y

such that, in terms of "greater explanatory power,"  $E$

$$(iii) \quad yEx \quad \text{and} \quad \sim xEy,$$

and this relationship is a more specific case of

$$(iv) \quad yRx \quad \text{and} \quad \sim xRy$$

mentioned above. Using  $A$  to represent the class of which  $x$  and  $y$  are both members, one can write the general formula for asymmetry again:

$$(v) \quad (x)(y)(x \in A \cdot y \in A \cdot yEx \rightarrow \sim xEy)$$

where  $(x)$ ,  $(y)$  represent "for all 'discoveries that'" and "for all 'discoveries why,'" respectively, and  $A$  is the class of discoveries in general.

However, "greater explanatory power"  $E$  need not be the only external evaluative criterion, for one may also consider "greater ontological reality,"  $O$  as the criterion. Consider what Blackwell says:

- (a) Of the two, discovered structures clearly possess the higher degree of ontological reality. They are much closer to the realm of nature as immediately experienced; they are much more explicit expressions of physical meaning; they are much more directly descriptive of the dictates of nature.<sup>66</sup>
- (b) In short, the closer is the descriptive empirical base of science, the less pressing is the issue of the ontological value of scientific assertions.<sup>67</sup>

One may then write  $xOy$  and  $\sim yOx$ . However, if one wishes to speak of "greater intentional reality,"  $I$  then  $y > x$  in terms of  $I$ , and the relationship is still one of asymmetry. Similarly, in terms of "greater



creative power required,"  $C$ , then  $yCx$  and  $\sim xCy$ . The task now is to investigate the reasons that give rise to these relationships.

Blackwell's treatment of the mind-nature dualism not only gives rise to the asymmetry discussed earlier, but also is the foundation stone of his entire theory. Although one may contend that his treatment is "middle of the road," and, hence, an easy way of avoiding the epistemological problems this dualism effects, it may nevertheless be claimed that his treatment is extremely viable just because of this very "neutral" position. In fact, this neutrality is one of the strongest points of the theory, for it seems to give enough ground upon which to build without having to resort to any metaphysical notions.

Descartes, as is well known, claimed that the universe is composed of only two basic constituents, or "substances," and, in using this latter term was following historical usage in which "substance" means "that which requires nothing but itself in order to exist." The defining characteristic of body (or matter)--that is, its essential characteristic without which it would not be what it is--is extension, the possession of dimensions; the defining characteristic of mind (mental substance) is thought, or activity of thinking. Descartes also maintained that only body and mind are substances, and everything in the universe is reducible to one or the other, but--and this is the crucial point--body and mind are not reducible to each other--they are separate and self-sustaining. Whereas body has none of the properties of mind, so too does mind have none of the properties of body. However, this self-sustaining power being discussed here is perhaps a little misleading

for, strictly speaking, Descartes also claimed that God (or the divine substance) is alone ultimately self-sustaining, such that the other two substances are substances by virtue of his power.

The problem with Cartesian dualism is well known, for Descartes was never able to explain satisfactorily the problem of interaction, of the relation of mind and body in the human organism. He suggested that the mind or soul might operate from a localized seat in the brain, and he suggested the pineal gland. However, this implies that the "seat" is located in space, or extended, and, as has been noted Descartes was at pains to emphasize that extension is the property of matter alone. Mind is defined solely as thought and has none of the properties of body. However, even if mind could somehow function without being located, the manner of its interaction with the body remains necessarily mysterious, for, how can two substances without any properties in common interact in any way at all? Descartes himself recognized the difficulty, and ultimately appealed to the hypothesis of God to "explain" the interaction.

Consider briefly the position of Spinoza. All that the contemporary naturalist would include in nature is, for Spinoza, "substance" the order of nature, and this includes both an order of extended things as well as a logical order of their "ideas" or intelligible aspects. Mind and matter are thus two "attributes" or ways in which the order of nature is illustrated. They are no longer, as in Descartes, independent substances (in addition to the divine) but are, rather, exemplifications of the one all-embracing substance. Furthermore, according to Spinoza, this substance (as the inclusive order of nature comprises the unity and

interrelation of all that exists), also contains within itself all happenings. It is, therefore "eternal," "infinite," and "omnipotent," and for this reason he calls it "God" as well as "nature." As Spinoza himself said: "Whatever is, is in God, and without God nothing can be, or be conceived." This is the basis, it seems, of Spinoza's Pantheism and Metaphysical Monism.

Now, in the light of some understanding (albeit sketchy) of the Cartesian and Spinozistic stands of dualism, the point here is to argue that Blackwell is justified in taking what seems to be a "middle road" for, both Descartes and Spinoza, in the final analysis, had to make recourse to metaphysical notions; Blackwell, by his relational and interactional treatment of this dualism, does not need to make such a recourse. He simply cannot be pushed into these "extreme" cases. As Blackwell himself writes:

It is not my intention here to dissolve all distinction between mind and nature and lapse into an indefensible monism. Both mind and nature have distinctive features. Rather, although mind and nature are not identical, each possesses a reality which also includes an essential relation to the other. The alternatives to be considered are not simply the extremes of a mind and nature as disparate realities bearing no relational dependencies or as identical entities with no distinguishing characteristics. We have lived too long in the shadow of the Cartesian dualism of mind and matter and the false problems which it generates. . . . If we insist in thinking in these categories, our only options are to accept Descartes' total separation or Spinoza's total identification of mind and matter. . . .

Although mind and matter are distinctively different realities, it does not follow that either is a complete and self-contained reality. . . . The intention is to argue that the mental and physical poles of reality have each within them an essential relation to the other. . . .<sup>68</sup>

Nevertheless, Blackwell does not want to claim that his approach is so very new, although one may still agree with him that the problems created by the Cartesian and Spinozistic stands have been primarily responsible for the neglect in the examination of the discovery aspect of scientific inquiry. Traditionally, the emphasis has been shifted to what now appears to be an adequate account of the hypothetico-deductive method. But this account, as illustrated by Braithwaite, Reichenbach, and Popper is limited to the logical analysis of the finished scientific system, and does not take into consideration the reasons which originally suggested the principles. Central to the theory of discovery are these two phases which have already been termed "the context of discovery" and "the context of validation." Blackwell makes this distinction clear; the former context is more closely associated with the "nature" aspect of the dualism, and the latter more nearly associated with the "mind" aspect.

However, as mentioned above, Blackwell does not claim that this difference has never before been specified. He refers to Newton's distinction between the analytic and synthetic phases, the former being the stage of going from data to principles--the discovery phase--and the latter, the going from principles to conclusions in the order of explanation.

Blackwell's argument against the Humean epistemological influence in contemporary philosophy of science seems valid, for a strong charge may be laid against Hume to the effect that he leaves no room for the act of discovery in the usual sense of the term. According to Hume, all

human knowledge ultimately reduces to what he calls "impressions," the central feature of which is that they are atomistic and do not bear within themselves internal relationships to each other. In Humean epistemology the development of human knowledge consists in the mind's actively making up for this deficiency of impressions (i.e., the absence of relatedness) by constructing relationships between them and others to form the objects of the experienced world. For Hume there is simply nothing about the impressions that can be discovered. The mosaic of human knowledge is ultimately composed of minute pieces which have no internal, pregiven relationships to each other, not even, it would seem similar shades of, say, the colour blue. What can be done within such an epistemology, especially with respect to discovery? It should come as little surprise that there is, in fact, little *take* about discovery, but a good deal more is said with respect to constructionism.

The problems with Hume's epistemology have been encountered much earlier, for although it may seem at first a little peculiar, it can be claimed that Hume's impressions are in the same epistemological class as the Platonic Forms. It is well known that Plato introduced his Forms in order to have an appropriate set of objects for the universe of intellectual discourse, but, as shown in his later dialogues he came to realize that if each Form exists in isolation from the others, as he apparently originally thought, the whole argument comes to nought. It seems that interrelatedness--as Blackwell is at pains to show within his own theory of discovery--is an essential condition for meaning and intelligibility.

Blackwell's further charge against Hume also seems valid, this

charge being that Hume's epistemology is untrue to living human experience. As Blackwell says:

Direct experience is of a complex world composed of a huge variety of interrelated objects and events that any viable epistemology must take as its starting point and can deny only at its own peril. This point has been made over and over again, often with great eloquence, by various philosophers. Yet the Humean view still strongly persists. Perhaps this is because it is so reductionistic and mechanical, attributes which appeal strongly to the modern mind.<sup>69</sup>

It can now be said that, in addition to the Cartesian and Spinozistic stands on dualism, the Humean stand on epistemology also creates unviable (as well as untrue) artificial situations that Blackwell's approach (emphasizing the relationship and interaction of mind and nature)--avoids. It seems that Hume's emphasis on the atomistic nature of sense impressions, and the further development of this notion in later sense data theories have resulted in a progressive depletion of meaningful content from immediate sense experience, leading logically, in the end to a complete vacuity of meaning. ✓

Another virtue of this particular treatment of the dualism is simply that by positing relatedness and interaction one also avoids objectifying abstractions. For example, if one were to take the Cartesian notion of total separation, and attempt to discuss an epistemology in terms of it, one would most likely find oneself dealing in such abstractions as "nature-in-itself" and "mind-in-itself." But these are abstractions, and the danger for understanding any epistemology is the tendency to objectify such abstractions and to take them as the primary concrete realities under discussion. This is Whitehead's fallacy of misplaced concreteness, and this fallacy Blackwell avoids.

The central feature of Blackwell's treatment of the present dualism is based then on two basic characteristics; viz., relationship and interaction. The asymmetry between "discovery that" and "discovery why," the two fundamental theoretical constructs, arises from these two characteristics.

And it is this asymmetry that helps one to focus on other features of discovery, such as the view of scientific discovery as a form of creativity. Consider the asymmetry formulas in the following order:

- (i)  $xOy$  and  $\sim yOx$      $O$  is "greater ontological reality";
- (ii)  $yIx$  and  $\sim xIy$      $I$  is "greater intentional reality";
- (iii)  $yEx$  and  $\sim xEy$      $E$  is "greater explanatory potential."

Formulas (i) to (iii) involve an increasingly greater emphasis on mind, such that formula (iv) below represents a viable consequence:

- (iv)  $yCx$  and  $\sim xCy$      $C$  is "more creative than."

There are several passages in respect of (iv):

- (a) On the other hand theoretical knowledge is contained in statements which express the relations among non-observable entities and events and which are produced by creative assumptions of the mind.<sup>70</sup>
- (b) . . . it follows that creative postulation offers a wider range for genius and gifted insight. Knowing how to postulate theoretical entities of explanatory value is a matter of considerable ingenuity, and it defies logical analysis. But this should not lead us to underrate creative postulation; it is one of the chief types of transformation in the genesis of a scientific theory.<sup>71</sup>

- (c) The theoretical entities which result are to a large extent the products of the creative functions of the mind. To this extent they are not ontologically real. They are mentally created symbols which, as such, are intentional entities.<sup>72</sup>

One of the strongest charges against Blackwell's theory could come from those who claim that the generation of hypotheses must be independent of experience, and, moreover, that they must be arrived at deductively. These conditions are intended to maximize the possibility of obtaining an hypothesis that is free of the investigator's prior dispositions, values, attitudes, and so on. It may be held that Blackwell's interaction treatment of the mind-nature dualism may preclude this conceptual objectivity. In fact, however, there is no inconsistency, for Blackwell is talking about the influence of the universe of experience in the origin of the idea, the logically and temporally prior stage before any attempt at the formulation of even a crude, tentative hypothesis. The fact that he makes such a strong case for the transformational processes of idealization and, especially, creative postulation, indicates that he too is aware of the problems of "objective" (inter-subjective) hypothesis generation. Perhaps because of this same problem he speaks of the first adaptive stage as "elaboration of experience," and introduce the new term "transformation" to refer to that stage culminating in the formulation of an hypothesis.

Certain areas of Blackwell's discussion do, however, seem somewhat vague. For instance, in trying to justify his treatment of the mind-nature dualism he resorts to vague expressions such as "the human mind and physical nature are somehow appropriate for each other." One



may then ask for a more precise meaning of the phrase "somehow appropriate," and also his justification and/or empirical evidence, for this claim. One may also question his use of mind-nature confrontation; in what sense, if any, does "interaction" differ from "confrontation," or is the latter term simply a label for a temporally prior situation? In addition, his ideas of "elaboration of nature," "dictates of nature," and "progressive specification" are all left somewhat unsatisfactorily explained, and it seems Blackwell relies too heavily on a quasi-intuitionist, perhaps "common-sense" approach; nevertheless these (and perhaps other) terms are vital to his proposed theory, and it might be expected that he would delineate them more explicitly. These appear to be some of the more obvious weaknesses of the theory as presently offered; perhaps others are present also.

However, the theory does seem to succeed in its basic purposes of:

- (1) calling attention to this important aspect of the scientific enterprise. Whether the success is complete will depend, it may be suggested, on whether his work stimulates further effort in the study of this process which perhaps, on the empirical level, could lead to additional insights and discoveries;
- (2) separation of the context of discovery from the context of validation, and to develop explicitly the former, which has not been done hitherto;
- (3) providing a treatment which may be corrective for those who may attribute a naively realistic status to scientific entities (as,

for example, by showing that theoretical entities are often very "creative" in nature).

In addition this thesis has argued in support of Blackwell's theory in terms of:

- (1) his treatment of the mind-matter dualism with respect to the problems raised by Plato, Descartes, Spinoza, Hume, and Whitehead;
- (2) the implicit asymmetry that allows further understanding of, and insights into, the nature of this process.

Finally, it may be worth repeating, especially for those who feel a further analysis of a logic of discovery has been forfeited: Blackwell's theory is not to preclude any such further study of the "logic" involved, if, indeed, it exists. The outstanding feature of Blackwell's contribution is that it allows one to get on with the attempt to understand the discovery process simply by approaching the problem from a new perspective, and without becoming stifled by trying to see "how many angels can dance on the head of a pin"; that is, without being stifled by trying to determine whether or not a logic of discovery does in fact exist.

#### SUMMARY

This chapter has been an intensive examination of scientific discovery with the aim of acquiring some understanding of the process, as well as identifying some of its characteristic features. Traditionally

this has been a neglected area, and philosophers have simply referred the matter to psychologists. Some of the psychological work done to date have been cited, and, as witnessed by Simon's articles, psychological theories of scientific discovery are being developed. However, there is not as yet a single, complete theory.

The philosophical approach has focused on elucidating a logic of discovery (Bacon, Mill, Hanson) but analysis of their treatments refute the existence of such a logic. Other methods have been tried. Peirce's "abduction" fails because it simply reduces to a term describing the mental processes by which hypotheses are formulated. Polya's logic of heuristic plausibility inferences fares better. Models are perhaps the closest technique to such a "logic," but there is no algorithm generating the model from which the most fruitful hypotheses may be deduced. As well, models possess many intrinsic limitations.

Empirically (by what psychologists report and by what scientists themselves say) as well as conceptually, discovery appears to be non-logical and "creative." But this is not to say that further study, especially further philosophical study is impossible. Blackwell's work does throw more light on discovery and basically succeeds in explaining the process. Moreover, analysis of his arguments again shows up the creativity of scientific discovery. The following model can then be constructed.



## FOOTNOTES

<sup>1</sup>F. C. McLean, "The Happy Accident" in *Scientific Monthly*, LIII (1941), pp. 61-70, referred to in A. Reza Arasteh, *Creativity in the Life Cycle*, Vol. I - An Annotated Bibliography. Copyright by E. J. Brill; Linden, Netherlands, 1962.

<sup>2</sup>Herbert A. Simon, "Scientific Discovery and the Psychology of Problem Solving" in R. G. Colodny (ed.), *Mind and Cosmos*. U.S.A.: University of Pittsburgh Press, 1966.

<sup>3</sup>Israel Scheffler, *Science and Subjectivity*. New York: The Bobbs-Merrill Company Inc., 1967, p. 2.

<sup>4</sup>Albert Szent-Gyorgyi, *The Way of the Scientist*. Interviews from the World of Science and Technology, Selected and Annotated by the Editors of International Science and Technology, New York: Simon and Schuster, Inc., 1966.

<sup>5</sup>R. R. Wilson, *The Way of the Scientist*, pp. 52-53.

<sup>6</sup>*Ibid.*, p. 52.

<sup>7</sup>*Ibid.*, p. 52.

<sup>8</sup>Scheffler, *Science and Subjectivity*, p. 69.

<sup>9</sup>Simon, "Scientific Discovery and the Psychology of Problem Solving" in *Mind and Cosmos*, p. 22.

<sup>10</sup>Brewster Ghiselin, *The Creative Process*, Berkeley: University of California Press, Mentor paperback, 1967, p. 38.

<sup>11</sup>*Ibid.*, p. 42.

<sup>12</sup>*Ibid.*, p. 40.

<sup>13</sup>W. I. B. Beveridge, *The Art of Scientific Investigation*. New York: Vintage Books, Inc., 1957, p. 102.

<sup>14</sup>*Ibid.*, p. 76.

<sup>15</sup>*Ibid.*, p. 76.

<sup>16</sup>R. B. Lindsay, *The Nature of Physics: A Physicist's Views on the History and Philosophy of His Science*. Providence, Rhode Island: Brown University Press, 1968, p. 34.

- <sup>17</sup>Beveridge, *The Art of Scientific Investigation*, p. 78.
- <sup>18</sup>*Ibid.*, p. 72.
- <sup>19</sup>*Ibid.*, p. 77.
- <sup>20</sup>*Ibid.*, p. 94.
- <sup>21</sup>*Ibid.*, p. 94.
- <sup>22</sup>*Ibid.*, p. 95.
- <sup>23</sup>Ghiselin, *The Creative Process*, p. 25.
- <sup>24</sup>*Ibid.*, p. 25.
- <sup>25</sup>*Ibid.*, p. 25.
- <sup>26</sup>Beveridge, *The Art of Scientific Investigation*, p. 89.
- <sup>27</sup>James Watson, *The Double Helix*. Toronto: The New American Library of Canada Limited, 1969, p. 136.
- <sup>28</sup>*Ibid.*, p. 123.
- <sup>29</sup>*Ibid.*, p. 123.
- <sup>30</sup>Lindsay, *The Nature of Physics*, p. 87.
- <sup>31</sup>M. Turner, *Philosophy and the Science of Behaviour*. New York: Appleton-Century-Crofts, 1965, p. 94.
- <sup>32</sup>W. C. Salmon, *The Foundations of Scientific Inference*. U.S.A.: University of Pittsburgh Press, 1967, p. 111.
- <sup>33</sup>N. R. Hanson, "Is There A Logic of Discovery?" in *Current Issues in The Philosophy of Science*, H. Feigl and G. Maxwell (eds.), New York: Holt, Reinhardt and Winston, 1961, p. 22.
- <sup>34</sup>Salmon, *The Foundations of Scientific Inference*, p. 112-113.
- <sup>35</sup>G. Holton, "Presupposition in the Construction of Theories," in Harry Wolf, *Science as a Cultural Force*, Baltimore: The John Hopkins Press, 1964, p. 62.
- <sup>36</sup>R. Rudner, *Philosophy of Social Science*, Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1966, p. 6.
- <sup>37</sup>I. Scheffler, *Science and Subjectivity*, p. 68.

<sup>38</sup>*Ibid.*, p. 2.

<sup>39</sup>K. Popper, *The Logic of Scientific Discovery*, New York: Basic Books, Inc., 1969, pp. 279-80.

<sup>40</sup>C. Hempel, *Philosophy of Natural Science*, Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1966, p. 15.

<sup>41</sup>*Ibid.*, p. 17.

<sup>42</sup>Salmon, *The Foundations of Scientific Inference*, p. 140.

<sup>43</sup>A. N. Whitehead, *Science and the Modern World*, New York: The New American Library of World Literature, Inc., 1964, p. 25.

<sup>44</sup>Scheffler, *Science and Subjectivity*, p. 69.

<sup>45</sup>A. Kaplan, *The Conduct of Inquiry*, San Francisco: Chandler Publishing Co., 1964, p. 8.

<sup>46</sup>*Ibid.*, p. 27.

<sup>47</sup>S. Toulmin, *The Philosophy of Science*, New York: Harper and Row, 1960, Chapter Two.

<sup>48</sup>*Ibid.*, p. 32.

<sup>49</sup>Max Black, *Models and Metaphors*, p. 226.

<sup>50</sup>*Ibid.*, p. 226.

<sup>51</sup>Kaplan, *The Conduct of Inquiry*, p. 219.

<sup>52</sup>*Ibid.*, p. 194.

<sup>53</sup>*Ibid.*, p. 289.

<sup>54</sup>*Ibid.*, p. 282.

<sup>55</sup>R. B. Braithwaite, *Scientific Explanation*, London: Cambridge University Press, 1953, p. 93.

<sup>56</sup>Rudner, *Philosophy of Social Science*, p. 27.

<sup>57</sup>*Ibid.*, p. 27.

<sup>58</sup>*Ibid.*, p. 27.

<sup>59</sup>H. Reichenbach, *The Rise of Scientific Philosophy*, Berkeley and Los Angeles: University of California Press, 1958, p. 231.

<sup>60</sup>Popper, *The Logic of Scientific Discovery*, p. 31.

<sup>61</sup>T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago: The University of Chicago Press, 1962, p. 87.

<sup>62</sup>R. Blackwell, *Discovery in the Physical Sciences*, Saint Louis: Notre Dame University Press, 1969, p. 185.

<sup>63</sup>*Ibid.*, p. 159.

<sup>64</sup>*Ibid.*, p. 183.

<sup>65</sup>*Ibid.*, p. 200.

<sup>66</sup>*Ibid.*, p. 202.

<sup>67</sup>*Ibid.*, p. 203.

<sup>68</sup>*Ibid.*, pp. 111-12.

<sup>69</sup>*Ibid.*, p. 91.

<sup>70</sup>*Ibid.*, p. 51.

<sup>71</sup>*Ibid.*, p. 175.

<sup>72</sup>*Ibid.*, p. 205.



## CHAPTER FOUR

### MUSICAL CREATIVITY

#### SECTION I    *Introduction*

The previous chapter established two phases of scientific discovery (general sense), namely, the subjective and the objective. The former is associated with, in psychological terms, divergent thinking and creativity and with the context of discovery; the latter is associated with convergent thinking, with adherence to rules of valid inference, and with the context of validation. This present chapter will be devoted to an examination of musical creativity, but, as this activity and concept may be considered a standard case, or paradigm, of creativity (as the term itself implies) the emphasis will be an attempt to establish an objective base such that a relationship may be formulated between scientific discovery (general sense) and musical creativity (general sense). That is, the relationship need not be restricted to the establishment of a creative-subjective phase, but can be extended to include also a rule-centered, objective one. This will then allow a relationship between both the special and the general senses of each term.

The general plan of the chapter will be as follows:

(A) empirical (I), being brief summaries of what psychologists have written about the subject;

(B) empirical (II), being a correlation of what composers themselves have said about musical creativity. These quotations will be categorized as in Chapter Three, namely:

- (1) speculation and the humanistic dimension;
- (2) (creative) imagination;
- (3) inventiveness, ingenuity, intuition, and the role of prior knowledge;
- (4) selectivity and critical evaluation.

Again (1) and (2) will be closely related.

(C) conceptual (I): an argument to establish an objective base through an attempt to clarify the notion of "music theory";

(D) conceptual (II): an exposition of the logic underlying a basic theory of musical structure ("the Proto-theory of Music") which can be seen as supplying a fundamental conceptual framework analogous to, for example, the atomic theory of matter.

## SECTION II

*Some psychological approaches that have been effected to attempt to understand musical creativity, which studies are intended to support the conceptual claims to be made later in the thesis.*

*empirical (I) approach: psychological studies. R. H. Wilman: emphasizes the principle of selectivity. A. R. Arasteh supports this principle and suggests that it applies to all types of creativity. Evelyn Benham's auditory images comprise the first phase of musical creativity, but are associated with trained observation.*

R. H. Wilman relates ("An Experimental Investigation of the Creative Process in Music")<sup>1</sup> how he chose twenty-two "standard" and ten "popular" composers, with varying degrees of musical training. One part of the experiment consisted of interviews with the composers themselves, and a significant finding was that selectivity was observed to enter the creative process, involving choice among a large number of possibilities, and perception of the relationships of the choice in regard to specific situations. A. Reza Arasteh (*Creativity in The Life Style*) agrees with Wilman and suggests that this principle of selectivity not only applies to creative musicians but to every creative person (presumably, including scientists). He goes on to say (and later elaborates in some detail) that all creative processes seem similar to one another, although this is not to say that they are identical. Each type of creativity is uniquely determined by its creator (artist, musician), and also by the form and design of the work. However, in music, the techniques of the instruments and voices and technical skill in actual composition play a greater role in determining what has been called musical creativity (general sense). In other words, of all the arts, it seems that music is the most highly technical, both in its creation and in its performance.

Peter Racine Fricker says:

Perhaps the art of composing music needs a very clear head. I may be being bumptious about this, but, to my mind, there are so many more technical questions involved in composing music than in painting or literature that you need a clearer mind to cope with them . . . <sup>2</sup>

Evelyn Benham ("The Creative Activity: Introspective Experiments in Musical Composition")<sup>3</sup> presents observations based on her own experiences

prior to and during the process of composing music. Amongst other results, she concludes that auditory images comprise the initial stage (cf. later Copland's term, "sound image") but that trained observation is needed to get at it. This suggests that prior training (cf. "role of prior knowledge") is also essential.

This concludes this brief survey of empirical results which have been presented in various psychological journals (see bibliography). Again, the thesis makes no pretense that the investigations in this area are complete, or that further study is not wanting. These works are cited here in support of claims to be made in subsequent sections.

### SECTION III (empirical II)

*An attempt is made to understand further the nature of musical creativity by examining some quotations of composers themselves. As was the case with scientific discovery, these quotations have not been systematized; they are gathered here in four categories which categories have already been introduced in Chapter Three. For review purposes, these categories are:*

- (1) *Speculation and the humanistic dimension:*  
I. Stravinsky. H. Searle. P. R. Fricker.  
Sir Michael Tippett.
- (2) *Creative imagination:*  
I. Stravinsky. A. Copland. H. Searle.
- (3) *Inventiveness, ingenuity, intuition, and the role of prior knowledge:*  
I. Stravinsky. A. Benjamin. P. R. Fricker.
- (4) *Selectivity and critical evaluation:*  
I. Stravinsky. A. Copland. E. Welles.

*Summary and indication of the subsequent direction of the discussion.*

(1) *Speculation and the humanistic dimension:*

One of the "themes" upon which a relationship between scientific discovery and musical creativity (general senses) can be built is that of speculation, and the sketching out of a basic framework before filling in the details. The late Igor Stravinsky has discussed music as a phenomenon of innate complexities and possibilities that require speculation, where the composer's will, first moving in an abstract realm eventually results in something concrete, the final musical composition. In his *Poetics of Music* he writes:

For the phenomenon of music is nothing other than a phenomenon of speculation. There is nothing in this expression that should frighten you. It simply presupposes that the basis of musical creation is a preliminary feeling-out . . . <sup>4</sup>

This preliminary speculation is also confirmed by Humphrey Searle, who says:

. . . I sketch out a composition very quickly and then go back and fill in the details. If you looked at some of the unfinished scores of Schubert you would get an idea of what mine look like after this first stage--just the briefest indications for later reworking. I like to have a framework before me and I like to get it erected as quickly as possible. I sketched my third symphony in three weeks; then I went back and spent three months on the details.<sup>5</sup>

and this is further supported by Peter Racine Fricker who, in the following passage also makes reference to Beethoven:

. . . Beethoven, I believe said that he had a 'picture' of a work before he wrote it--not a programme, but a conception about how the piece will work out. He meant, I think,

that he saw the outlines of the work clearly and knew how and where the different characteristics of the work would emerge. I feel much the same - the work begins to take shape before I write a note of it.<sup>6</sup>

Finally, Sir Michael Tippett says:

. . . I begin by first becoming aware of the overall length of the work, then of how it will divide itself into sections (perhaps movements), and then of the kind of texture or instruments that will perform it. I prefer not to look for the actual notes of the composition until this process has gone as far as possible. Finally the notes appear and, in general, I find that during the purely mental process of articulating my imagination the precise material has been forming itself subconsciously, so that I never have to struggle to find it.<sup>7</sup>

(2) *Creative imagination:*

Stravinsky speaks of creative imagination in his composing in that any invention presupposes it. Moreover, it is this imaginative faculty that helps the composer to pass from the conceptual realm to that of realization. Also, he suggests that creative imagination goes hand-in-hand with observation. He writes:

The faculty of creating is never given to us all by itself. It always goes hand in hand with the gift of observation.<sup>8</sup>

Aaron Copland, another prominent 20th century composer, and perhaps the leader of the American school of composers, also writes on the important role of the imagination when he says that the free and imaginative mind is at the base of all vital music making. He goes on to add that in music, a highly creative, imaginative mind is especially important because music provides the broadest possible vista for the imagination

since it is the freest, most abstract, and least hindered of all the arts. It is interesting to observe in connection with these notions of creation and abstraction what A. N. Whitehead has written (cited first in Chapter Three, and here quoted in part).

The Science of Pure Mathematics, in its modern developments, may claim to be the most original creation of the human spirit. Another claimant for this position is music.<sup>9</sup>

Also, Humphrey Searle, in connection with his preliminary sketching-out (speculation) also mentions the notion of imagination:

I try to have a good idea of the formal shape of the work before I begin; the series is simply my material. I don't have any organized scheme as to how this material in its various forms and transpositions will make its appearance in the work but draw on it as my imagination requires. What I sometimes do is to make the last two notes of one form of the series the same as the first two of the next so that I have a logical continuation from one to another and this automatically creates transpositions with each repetition.<sup>10</sup>

(By the term "series," he is referring to the twelve-tone method.)

### (3) *Inventiveness, ingenuity, intuition:*

Stravinsky also makes reference to this point of inventiveness when, speaking of composers, he says composers have a duty toward music, namely to invent it. Moreover, the study of the phenomenon of music requires, as Stravinsky would have it, an "integral man," armed with all the resources of his senses, his psychological faculties and his intellectual equipment. He goes on to say that, although inventiveness presupposes creative imagination, it should not be confused with it, for "inventiveness" implies a lucky find (cf. Hempel's "happy guesses") and

the achievement or full realization of that find. However, he maintains that creative imagination does not necessarily take on a concrete form, such as a symphony or string quartet, and indeed may simply remain in a state of virtuality.

Arthur Benjamin also speaks of this intuition, although in the following it is referred to as an "instinct":

When I begin a work I never know what form it's going to take. I never say: 'This will be my second subject, here I'm going to build a bridge, or it's now time for the recapitulation.' I feel these things instinctively when the proper time arrives. Practice too, has helped me to lose all sensations of adapting myself to the formal necessities of different works. I will say though, that large works are always more difficult. They require more mental planning; one's vision must be broader and one's memory must be stronger.<sup>11</sup>

He also refers to the role of prior knowledge and its relation to "instinct."

It simply must become second nature to the composer to be able to build large movements. In his youth he has to practise by writing in the strict forms, first the minuet and then the sonata. To give a convincing shape to a fugue in which three or four voices are continually involved with one another is one of the greatest problems that could be set for the student composer. Working with these strict forms is only a means of training the student composer for the liberties he will take later in order to give his mature works a building power that will distinguish his musical personality. Then form becomes instinct.<sup>12</sup>

Finally, reference to intuition is again made by Peter Racine Fricker:

For me the 'vision' of a work is quite a different thing from experiencing a mental performance of it. It is, ideally, an instantaneous picture or image of the complete piece. Not all the details are clear but the general outlines will be quite definite. There seems to be no duration to this image or at least I don't sense it consciously this way. Not at first. I don't think this really comes until I begin writing



it down. Then I begin to balance the sections and space the climaxes. It is impossible to speak about this. It has to be sensed intuitively.<sup>13</sup>

(4) *Selectivity and critical evaluation:*

In discussing his work, Stravinsky says:

Thus, it is not a question of my private feelings and tastes, nor is it a question of a theory of music projected through a subjective prism. My experiences and investigations are entirely objective . . . <sup>14</sup>

Also, he dismisses the popular notion of "musical inspiration" that leads to great compositions by saying:

Most music-lovers believe that what sets the composer's creative imagination in motion is a certain emotive disturbance generally designated by the name of *inspiration*.<sup>15</sup>

He maintains that in no way is inspiration a prescribed condition of the creative act, but is, rather, a temporally secondary manifestation.

His ideas are clearly stated when he writes:

*Inspiration, art, artist* - so many words, hazy at least, that keep us from seeing clearly in a field where everything is balance and calculation through which the breath of the speculative spirit blows. It is afterwards, and only afterwards, that the emotive disturbance which is at the root of inspiration may arise . . . <sup>16</sup>

He goes on to say:

All creation presupposes at its origin a sort of appetite that is brought on by the foretaste of discovery. This foretaste of the creative act accompanies the intuitive grasp of an unknown entity already possessed but not yet intelligible, an entity that will not take definite shape except by the action of a constantly vigilant technique.

This appetite that is aroused in me at the mere thought of putting in order musical elements that have attracted my attention is not at all a fortuitous thing like inspiration, but as habitual and periodic, if not as constant, as a natural need.

This premonition of an obligation, this foretaste of a pleasure, this conditioned reflex, as a modern physiologist would say, shows clearly that the idea of discovery and hard work is what attracts me.<sup>17</sup>

He maintains that this selectivity and ordering of materials, of relating intervals rhythmically, comes long before any ideas are born, and this exploration of possibilities, for him, is always conducted at the piano. Thus, only *after* he has established his melodic, harmonic, and rhythmical relationships does he pass on to composition, which is a subsequent expansion and organization of material. Moreover, he says that the form of his compositions is the "logical discussion" of selected musical materials, and that the creative phenomenon cannot be observed independently of the form in which the phenomenon is made manifest. Furthermore, every formal process proceeds from one principle, viz., the need to bring order out of chaos.

This objective manipulating and ordering of musical materials is expressed by his description of music as organized tonal elements. He says: "All music is nothing more than a succession of impulses that converge toward a definite point of repose"<sup>18</sup> and that composing, for him, is

. . . putting into an order a certain number of these sounds according to certain interval-relationships. This activity leads to a search for the center upon which the series of sounds involved in my undertaking should converge. Thus, if a center is given, I shall have to find a combination that

converges upon it. If, on the other hand, an as yet un-oriented combination has been found, I shall have to determine the center toward which it should lead. The discovery of this center suggests to me the solution of my problem.<sup>19</sup>

This reference to problem solving is especially interesting, for, in conversation with the conductor Robert Craft, Stravinsky has said (in reply to a question of whether he regarded musical form as being in some degree mathematical):

It is at any rate far closer to mathematics than to literature - not perhaps to mathematics itself, but certainly to something like mathematical thinking and mathematical relationships. (How misleading are all literary descriptions of musical form!) I am not saying that composers think in equations or charts of numbers, nor are those things more able to symbolize music. But the way the composers think, the way I think, is, it seems to me, not very different from mathematical thinking. I was aware of the similarity of these two modes while I was still a student; and, incidentally, mathematics was the subject that most interested me in school. Musical form is mathematical because it is ideal, and form is always ideal . . . <sup>20</sup>

It has been indicated that the selectivity and ordering of materials is not completely left to the composer's abandon. As Copland states, the creative mind must also be a *critical* one, and Stravinsky writes that, after the creator has sifted the elements available, he must then impose limits, for, "the more art is controlled, limited, worked over, the more it is free."<sup>21</sup>

Finally, Egon Wellesz describes the manner in which he was taught composition by Arnold Schoenberg in which the notion of "selectivity" is again highlighted:

When he saw our work and criticized it he always gave us four or five different solutions to the problems that confronted

us. That is the difference between him and many other teachers; he taught us that every artistic problem can have a variety of solutions. But he insisted that one must always strive for integrity of thought. He taught that if the melody is simple you need a simple harmony; if the melody is complicated you need complicated harmony. I remember he once told a pupil: 'Didn't you invent a simple harmony to this simple melody first, and then make it complicated?' The pupil admitted he had. 'Well then, please write it that way.'<sup>22</sup>

Inventiveness and ingenuity, even creative imagination and speculation already discussed, seem to be related, in some way, and to varying degrees, with prior knowledge. Moreover this knowledge need not be understood in a narrow sense, i.e., knowledge of only one field in depth, but more in terms of a broad but secure grasp of several fields. Stravinsky touches on this idea when he writes:

Art in the true sense is a way of fashioning works according to certain methods acquired either by apprenticeship or by inventiveness. And methods are the straight and predetermined channels that insure the rightness of our operation.<sup>23</sup>

Summary: Sections II and III have been concerned with elucidating the special sense of musical creativity. In Section II the emphasis has been on the way psychologists have to date reported on this particular phenomenon; the section does not pretend to be comprehensive. Section III has established the relationships between scientific discovery and musical creativity in the special sense and with respect to four mutual categories. The thesis will now be directed at establishing the relationship in the general sense.

#### SECTION IV

*The "objective phase" of musical creativity is concerned with conceptual frameworks (theories) and testing (Copland: testing of "sound image." A discussion of the concept of "musical creativity." Musical styles as conceptual frameworks. Theories as conceptual frameworks. Musical styles as musical theories. The notions of "explanation and prediction." Detailed treatment of this argument with examples:*

- (1) Medieval and Renaissance music;*
- (2) Baroque music;*
- (3) Classical-romantic music;*
- (4) Schoenberg's Twelve-Tone Method (Modern).*

*(The emphasis is on rules of composition within these styles, and on form.)*

*Recapitulation of the argument. Empirical foundations of musical theory. Schoenberg's method close (if not equivalent) to a formal deductive system. The notion of testing.*

The notion of "conceptual frameworks" plays a substantial role in scientific research (and probably in other disciplines as well). The theories accepted in science, provisional though they may be, exert a profound effect upon the way in which scientists--theoreticians and experimentalists alike--interpret the world. One has only to investigate some of the major scientific revolutions in order to appreciate the influence of conceptual paradigms. The revolutions effected by the theories of Copernicus, Galileo, Newton, Harvey, Pasteur, Planck, and Einstein speak for themselves in this regard. It is also interesting to note the social context which influences the development of intellectual thought, and T. S. Kuhn's book *The Structure of Scientific Revolutions* should not be overlooked when scientific conceptual frameworks are being

considered. For example, the tremendous influence of the church on society in Galileo's day also pervaded scientific thought. (This has been well documented in G. de Santillana's book, *The Crime of Galileo*. Particular notice should be given to his trial by the "aristocracy" of the Church.) Perhaps such excruciating controls are less forceful today; at any rate, they are less obvious. But the point remains that, social and cultural interaction in the development of theories notwithstanding, the effect of these theories, once formulated, does profoundly influence the view of the world that the researcher sees. Such is also the case, this thesis holds, with music and musicians.

Traditionally, the whole of music (performance and composition) has shunned any attempt at developing, formalizing, and systematizing its materials. Music is an art, and such intellectualization, it was (and is) held, is anathema to its nature, let alone its growth. But as early as the 14th century Philippe de Vitry (1291-1361) bishop, poet, and composer wrote a treatise, *Ars Nova*, in which he set down the first rules of polyphonic composition whereby he:

- (1) established duple time where only a triple time had formerly been accepted (and this was due once again to the influence of the church and the reference to the Holy Trinity);
- (2) introduced new rhythmic schemes;
- (3) forbade consecutive octaves and fifths;
- (4) emphasized *musica ficta*, (the free chromatic alteration of music, not only to avoid the succession of the tritone, but to decorate the approach to a cadence).

(More will be said of musical "rules" subsequently.) But it was not

until the Romantic era in music that writings about music--its performance, history, and theory--began to flourish. Today there has been a rapid growth of journals and treatises (not to mention university courses) dealing with this aspect. Thus, at least three major areas of music are now recognized: composition, performance, and musicology.

Can a more precise treatment be given to the notion of "music theory?" This present chapter will be an attempt to do so, and to relate it to the proposed "objective phase" of musical creativity.

Traditionally, musicians have recognized several general styles such as the baroque, the classical, the modern, and so on. Each of these styles bears its individual stamp because of underlying rules that permit certain compositional "moves" to occur, and it is the totality of these rules within a certain musical style that may be called the theory or conceptual framework. But what about the notion of "explanation" that is the aim of scientific theories? Although there is a dearth of systematized discussion, if one were to ask "Why does a symphony by Mozart sound as it does, eliciting in the listener a certain emotional response  $X$ , whereas a symphony by Brahms sounds different, eliciting in the listener an emotional response  $Y$ , it can be seen that the vocabulary, formation rules, transformation rules (to borrow terms from the logical structure of scientific theories) are of two different types. (This idea was briefly touched upon in Chapter Two in the context of "explanation why," "understanding why" and "learning why," although the emphasis at that time was to the performance aspect; also, compare the "performer" and "composer" in music to the "experimentalist" and

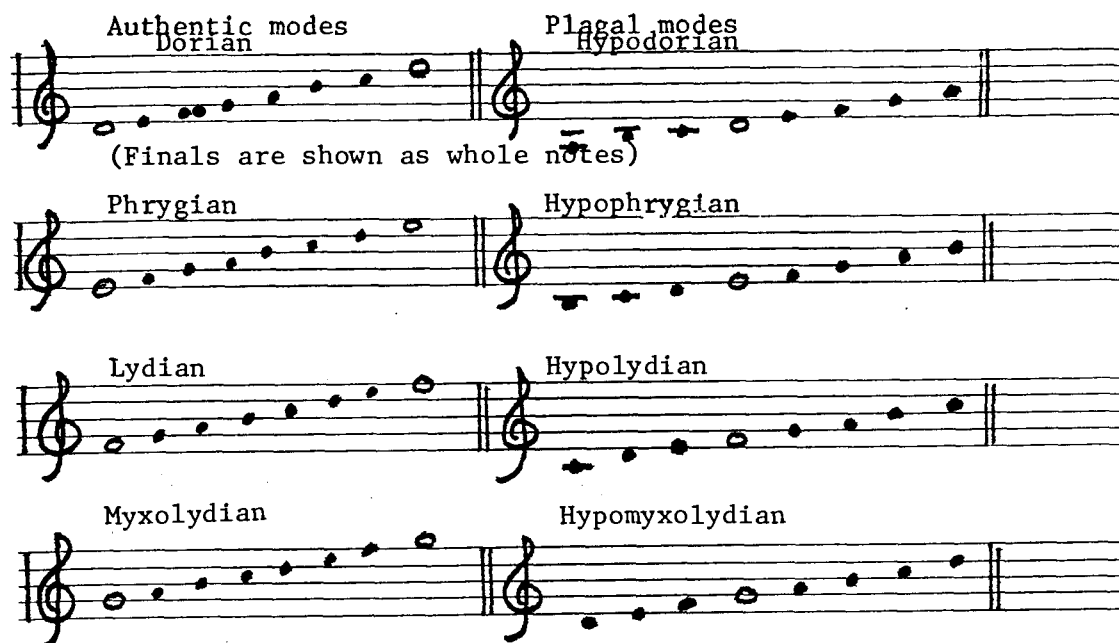
and "theoretician" in science.) It is time now to develop this point in more detail. Four distinct styles will be considered.

(1) Medieval and Renaissance Music:

In the 10th to the 12th centuries, the influence of the Church on western music was considerable. Indeed, it considered music to be, in a very definitive way, its own specific right to be used only in devotional services. Music outside the church, (i.e., secular music) was banned. It can be said then that this influence curtailed the development of original music in Europe since all music (all "legitimate music," that is) had to be founded on the old, immutable melodies of Gregorian Chant. In the language of creativity, however, we could state that this particular conceptual framework, with all its associated rules, determined in large part the unique character of the music.

Early church music was written in unison based on four patterns or modes, (which have their roots in the heritage of Greek music). It is believed that Pope Gregory (d. 604) (notice again the influence of the church) added the Plagal (derived) modes. The patterns, then, to which composers were "limited" were:





In the sixteenth century, the following were added:



Now, a composer of this era would, straight off, be limited to these ("church-determined") modes, and his melody could make use of a particular mode in one of two forms: either the melody could be confined to an octave range (plus a tone or two) between the lower and upper finals (as in the examples below), or the melody could descend to a fourth below and ascend to a fifth above the final (again, plus a note or two). These two forms were called the authentic and the plagal,

respectively. These patterns are exemplified in the following passages by Lasso (1532-1594) and Okeghem (1430-1495).

(a) Lasso  
A - - sper - ges - - me - - 1 2 3 4 5 6 7 8

(b) Lasso  
Qui - a - per - se - cu - tus est in - i - mi - - 1 2 3 4 5 6 7 8

(c) Okeghem  
A - - - gnus - De - - - i - 1 2 3 4 5 6 7 8

In the 11th century, a second part sung at the interval of a 4th or 5th was added to these unchangeable melodies of Gregorian chant. This method of composition, having a stationary tenor part with an added voice in parallel 4th and 5th, was called *organum*. The following is an example:

Dies Irae

During the 12th and 13th centuries, six rhythmic modes were added as follows:

I		III		V	
II		IV		VI	

Also, up to the 17th century, the chief forms of vocal music were also somewhat restricted in number to:

- (a) motet: music in which the tenor part, set with words and music from Gregorian Chant, was decorated by one, two, or three other parts each with different words and music (some representative composers would be Josquin, Lasso, Taverner, Byrd);
- (b) madrigal: secular vocal music for two or more voices of a polyphonic texture (Gibbons, Morley, Lasso, Monteverdi);
- (c) mass: a choral setting of, usually, six portions of the Roman Catholic Service: Kyrie, Gloria, Credo, Sanctus, Benedictus, Agnus Dei (Machaut, Palestrina).

These appear to be the chief forms, certainly in medieval and renaissance music. Also, by the 17th century two other large scale forms, opera and oratorio had been added. (For the sake of simplicity, no mention will be made of other important forms, such as conductus, fancy, and several dance forms such as the galliard).

## (2) Baroque Music:

Contrapuntal music of Palestrina's time was very tightly controlled (hence the term "strict counterpoint") and it was not unusual for older texts to refer to permissible progressions as the laws of progressions. For example, the following table gives the complete list of chords available for strict counterpoint, both in major and minor keys, excepting those which are asterisked which are unavailable in minor:

Symbol	Chords	Positions	
I	Tonic	Root	1st Inversion
II	Supertonic	* Root	1st Inversion
III	Mediant	* Root	* 1st Inversion
IV	Subdominant	Root	1st Inversion
V	Dominant	Root	1st Inversion
VI	Submediant	Root	1st Inversion
VII	Leading Note		1st Inversion

Thus, there are only thirteen possible positions of harmony in a major key; ten in minor. Appendix I contains a chart giving all possible progressions (the "laws" of progressions which describe (not prescribe) what is considered to be the best "sound" within this framework).

The five species of Strict Counterpoint are:

- Species I: note against note;
- II: two (sometimes three) notes in the added voice(s) against one in the canto fermo (C.F.);
- III: four (sometimes six or eight) against one in the C.F.;
- IV: two against one, but the second note of each bar is tied over into the following measure, so producing syncopation;
- V: devices of all other species are used from time to time, with a few others, so producing florid counterpoint.

It is this florid counterpoint that is emphasized today. It contains a number of rules of varying generality, but the following is one cast into a conditional hypothetical (which, it will be remembered, is the logical form of scientific laws):

If double counterpoint at the octave is written then it is invertible at the fifteenth (but the converse is not true).

The following is an example of counterpoint at the octave (a) inverted both at the octave (b) and the fifteenth (c):

(a) C.F.



Cpt.

(b) Inversion at the octave:



(c) Inversion at the fifteenth:



One further such "law" should suffice:

If double counterpoint at the octave is being written, then the parts must not be wider than an octave apart (or they will not invert) and these parts must not cross.

This is exemplified by the following example of counterpoint at the octave (a), and the inversion (b):

(a)

C.F.

Key: B-  
Cpt.

I IV V<sub>7</sub> I- VI IIb V Ib IV (D+)II Vb I (F#-) V<sub>7</sub> IIb V Ib

(b) Inversion:

(Same harmonies, with the positions changed.)

The principal form of polyphonic music became the fugue. (The thesis always assumes that two necessary conditions for the notion of "music theory" developed here are compositional rules [as traditionally treated) and form.] Once again there is fairly standard plan (which, of course, is not to say that it is an immutable one). For the purpose, a basic outline of fugal structure is given in Appendix J.

### (3) Classical - Romantic Music:

The rules are founded on the "laws" of harmonic progression.  
 (Harmony and counterpoint are inextricably blended - indeed, the laws of counterpoint are derived from harmonic principles.) Using the notion of "compositional law" as given in the section on the counterpoint, examples will now be given:

If two voices move in the same direction, then they may not move by parallel fifths or octaves.

The following then, are examples of "incorrect progressions":

(a) (b) (c) (d) (e) (f)

Consecutive:  
 fifths    octaves    unisons    fifths & 8ves    Exposed Octave    Exposed Fifth

If chords in second inversion are to be used, then the following (a), (b), and (c) are the basic patterns.

(a) Cadential  $\frac{6}{4}$     (b) Pedal  $\frac{6}{4}$     (c) Passing  $\frac{6}{4}$

Key: A

C+

Ic  $\frac{6}{4}$     Va  $\frac{5}{\#}$     IVc  $\frac{6}{4}$     Ia  $\frac{5}{3}$     Ia  $\frac{5}{3}$     IVc  $\frac{6}{4}$     Ia  $\frac{5}{3}$     Va  $\frac{5}{3}$     Ic  $\frac{6}{4}$     Va  $\frac{5}{3}$     Ia  $\frac{5}{3}$     Vc  $\frac{6}{4}$     Ib  $\frac{6}{6}$     IVa  $\frac{5}{3}$     Ic  $\frac{6}{4}$     IVb  $\frac{6}{6}$

If the dominant seventh chord is used, then it must resolve according to the following rules:

Position	Symbol	Figuring	Abbreviation	Resolution	Remarks
Root	V7a	7	7 or 7 3	Ia	7 falls, 3 rises
		5			5 omitted from one chord
		3		VIa IVb	7 falls, 3 rises 7 stationary
1st inversion	V7b	6	6 5 3	Ia	7 falls, 3 rises
		5			
		3			
2nd inversion	V7c	6	4 3 3	Ia Ib	7 falls, 3 rises
		4			7 rises, 3 rises
		3			
3rd inversion	V7d	6	4 2 2	Ib	7 falls, 3 rises
		4			
		2			

The chief form of the classical style is called "Sonata - Allegro" (there are other equivalent names). A brief description is given in Appendix K.

#### (4) Schoenberg's Twelve-tone Method:

Schoenberg's method may be termed atonal (although he himself disliked the term), in that it rejects the framework of key, and in addition, by excluding consonance, starts from dissonance, moving from one level to another (maximum tension). According to this technique, every dodecaphonic composition is based on an arbitrary arrangement of the twelve chromatic tones, the tone row. This fundamental set is the unifying idea that is the basis of that particular composition, serving as the source of all the musical ideas that occur in it. The twelve tones of the row (or series) are regarded as equally important, and no



one of them is permitted to appear more than once (this, of course is to avoid any notion of a "tonic"). The row may be inverted, presented backward (retrograde) or upside down and backward (retrograde-inversion). Each of these four versions--the original row and its three variants--may begin on any one of the twelve notes of the chromatic scale, thus giving forty-eight possibilities. This row generates not only the melody, but also the contrapuntal lines that unfold against it. In addition, it also generates the harmony, since segments of the row may appear in vertical formations as chords. The following example shows the four versions of the basic set from Schoenberg's *Wind Quintet*, Op. 26.

(E $\flat$  is enharmonic D $\sharp$ ). Schoenberg, Op. 26

Original Inversion Retrograde Retrograde-Inversion

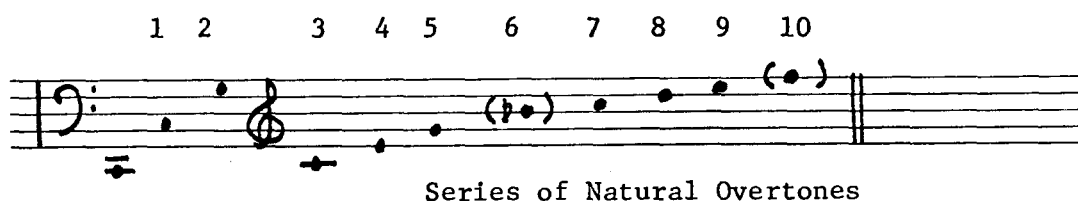
Consider a further example, Schoenberg's *Piano Concerto*, Op. 42.

The row is



It is not intended that the above discussions are in any way comprehensive; indeed, this would not be appropriate. However, enough support can be adduced from the survey undertaken to establish the notion of music theory (considered as collective noun) as intimately bound up with what has been traditionally called musical styles. The thesis argues that these "styles" may be equated with the notion of "conceptual frameworks," and as a theory is such framework, one may then have "style" equivalent to "theory." Hence, it may be appropriate to speak, in this context, of "baroque music theory," "classical music theory," and so on. Moreover, such a conception allows for explanation of why certain pieces of different styles sound the way they do; that is, why a fugue by Bach is different in sound from a symphony by Mozart. By symmetry, if one knows that composition *X* is baroque and *Y* is classical, then one can predict the over-all sound and stylistic characteristics of both works even before hearing them. But, one may legitimately ask: "How were these rules formulated?"

The laws of progressions in harmony and counterpoint (Schoenberg's method is excluded here; it will be dealt with presently) were not arbitrarily stipulated. In fact, they have an empirical basis, founded on the notions of "consonance" (intersubjectively agreeable sound) and "dissonance." Consider the following, called in physics the harmonic series:



(Overtones 6 is actually flatter than B-flat; overtone 10 is about half-way between F and F-sharp.)

In the earliest attempts at harmony, the only intervals considered consonant were the octave and the perfect fifth, as well as the inversion of the fifth, the perfect fourth. From the above, it can be seen that these intervals correspond to the first three natural overtones. (These intervals have already been encountered in the previous discussion of *organum* in medieval and renaissance music.) It is extraordinary that these crude beginnings at harmony should have the only three intervals considered consonant corresponding to the first two overtones, (or first three if the inversion of the fifth is taken separately). Indeed, these medieval composers (and theoreticians) discovered by ear and by instinct

what science was to elucidate many centuries later. Moreover, the historical order in which these tones have entered the primitive vocabulary corresponds again almost identically to the harmonic series. This is the basis of the empirical foundations of the laws of harmony and also, of course, of counterpoint. However, this empirical foundation is less in evidence (if at all) in considering Schoenberg's Method, for his is analogous to a formal, deductive system such as logic or mathematics. Here it seems that, with the four postulates stipulated, it is then simply a matter of drawing out consequences. This method (notwithstanding its almost universal employment in some manner or other by contemporary composers) has been criticized as "intellectual" and "arbitrary." But its wide use, in the long run, attests to its value. Indeed Schoenberg himself preferred the term "method" rather than "system" just because he felt that the former term implied something less rigorous and dogmatic. Furthermore, from the point of view of "conceptual frameworks," one may even speak of Schoenberg's serial technique as a major musical revolution.

It can be seen, then, that this notion of musical theory as framework leads a composer to consider musical materials available before actual composition, and this is argued to be analogous to the stage of "plausibility considerations" in the earlier discussion of scientific discovery. Furthermore, the notion of "test" also plays a role in composition, for once the composer has freely created an idea, then he must test in order to determine how well what he has written accords (or fails to accord) with the original idea. Beethoven's famous sketch-books

provide a wealth of support for the notion, and Allen Forte's analysis of the Op. 109 piano sonata also gives valuable clues. Another good example of this type of musical experimentation can be seen in the composing of the first Piano Concerto, Op. 15, by Johannes Brahms. Originally written as a symphony, the thematic material was revamped and the work was then converted into a sonata for two pianos. However, according to the composer, the material still seemed over-rich, and he eventually compromised between the two forms and decided to use the material as a concerto for piano and orchestra. It is also interesting that Brahms, in a letter to Clara Schumann, mentioned that the idea of writing the concerto first came to him in a dream. It is also interesting, from the perspective of our present problem, that, after the poor initial reception of this work, Brahms, in a letter to the celebrated violinist and conductor, Joachim, wrote that he was, with this composition, only experimenting and feeling out his way. Also, consider the revisions of the opening measures of the Valse, Op. 69, by Chopin:

OPENING MEASURES, FIRST MS.



## OPENING MEASURES, SECOND MS.



## OPENING MEASURES, FINAL MS.



Finally, Aaron Copland has said that composers, in general, are not so concerned with technical adequacy or quality of tonal performance as with the character and specific expressive nature of the interpretation. To this purpose, he writes: "Whatever else happens he doesn't want his basic conception to be falsified."<sup>24</sup> And falsification, it will be remembered, is central to Popper's program.

## SECTION V

*An attempt to establish a fundamental theory of musical structure analogous to, say, Dalton's Atomic Theory (of the structure of matter) or the Cell Theory in Biology. Exegesis of R. M. Martin's approach to formulating such a theory (as yet incomplete): the Proto-theory of Musical Structure. (Martin expounds three alternative logics for such a theory.) Determination of the ontology and primitive predicates. The distinction between pitch-class and pitch-event. A music theory based on pitch-classes and time. Cardinal couples. Chording based on pitch-class summation and on a class of dyadic, triadic (etc.) cardinal couples. A theory of musical structure based on virtual classes. A theory of musical structure based on event-logic. Discussion of Martin's theory and its implications.*

Traditionally, musicologists and music theorists have been more concerned with elucidating, describing, and relating "theory" more with respect to the practical aspects of composition and performance than with ascertaining the logic of musical structure in itself. Perhaps this is only right, for one's being a theorist does not presuppose one's being also a philosopher. Yet, for those whose concern are with conceptual foundations of art, music presents a special problem because

its language (notational and theoretical) is highly technical. Recently, however, there has been much more interest in this area, and journals are now issued that are primarily devoted to the investigation of the theoretical and philosophical aspects *per se* (and this has led to the claim that the art of music has now been "intellectualized." This is a related point, but will not be pursued at this time).

What purpose, if any, could be served by developing a basic theory of musical structure? The claim can be made that such a theory could unify various other theories; that is, there would be a hierarchy ranging from general to particular. Such is the case with physics and chemistry. In chemistry, for example, the basic theory of the structure of matter is Dalton's Atomic Theory which can account for such laws as conservation of mass, definite proportions, and multiple proportions. One of the corollaries of Dalton's theory is that atoms can "link" to form molecules, and this concept is presumed in the Kinetic-Molecular Theory of Gases (refer to Chapter Two). Thus, the Atomic Theory is more basic.

A corresponding, complete and generally acceptable theory to explain (and hence to bring one to understand) the nature of musical structure is as yet unavailable. However, contemporary interest has fostered a rapport between musicians and logicians, and one may expect a basic theory to be forthcoming. The thesis will now turn to an exegesis of R. M. Martin's recent attempt to develop such a theory.

There can be many alternative, underlying logics of any theory and, presumably, this is the case with music. (In fact, Martin presents



three.) In developing a theory it is of first importance to establish (1) the ontology or special domain of objects allowed as values for variables and (2) the non-logical primitive predicates or the basic classes into which these objects may be grouped as well as the basic relations by which they may be related. Martin then proceeds to discuss (1) and (2) which he claims is logically prior to the establishment of axioms; hence, he emphasizes that his theory, at this stage, is a "proto-theory" of music.

Martin does sketch out three frameworks, all of which could conceivably lay claim to be the proto-theory (although it is perfectly legitimate to have three alternative theories and models, such as required for the explanation of light-- refer to Chapter Two), and although one of these alternative theories involves at least a second-order logic, it is not altogether certain that such complexities, together with other complexities such as modal logic or three- or many-valued logics are required in a theory of musical structure. It is quite possible that the elementary logic of first order can be sufficient.

Martin draws an analogy in musical syntax with the distinction, in logical syntax (of language) between sign-designs and sign-events. In music, the corresponding terms would be note-class (or -shape) and note-event. The first is an abstract shape, or concept, outside of space and time. In contrast, a note-event is a particular ink-mark on a particular page of a particular musical text. It is a phenomenal occurrence within both space and time and, presumably, within some human experience. Following the treatment of logical syntax and semantics

to date, Martin suggests that initially it would be easier to base a musical theory on pitch-classes rather than pitch-events. In other words, as logical syntax has concerned itself primarily with sign-shapes rather than sign-events, Martin wishes to use this treatment as a heuristic model for the possible development of his own (musical) theory.

Within the ontogony of traditional musical structure there is a finite number of distinct pitch classes, regarded as individuals, and these are precisely the twelve pitch-classes of the chromatic scale. (Electronic music, microtonal music, and so on would probably have extended domains.) Thus, 'C' would designate all these classes of pitch with such-and-such vibrations per second (or multiplied/divided by 2). Probably a null pitch-class could handle rests, cesuras, and so on.

The second theoretical entity is time. Martin uses the epochal or pulse theory which holds that time flows on in little pulses, the shortest of which would be determined by the limits of human discrimination during which musical sound would be produced. It appears, then, that the simplest description of musical structure is pitch-classes combined with times in certain ways -- or, for the composer especially -- the right pitch at the right time. Moreover, these pulses ( $t$ ) would be transitive and irreflexive, and, hence, connected.

Consider the time impulses  $t_1$  and  $t_2$ , and the relationship  $R$ , which states that no two time impulses can be in the same time class. Thus,  $t_1$  and  $t_2$  are serial and the irreflexive relationship can be symbolized as:

$$t_1 R t_2$$

Also, consider the relationship  $S$ , which states that two time impulses are serially *related*. Then, for the *serial* time impulses  $t_1$ ,  $t_2$  and  $t_3$ , if

$$t_1 S_{t_2} \text{ and } t_2 S_{t_3}$$

then

$$t_1 S_{t_3}$$

or, in slightly more precise form:

$$(t_1) (t_2) (t_3) ((t_1 \in T \cdot t_2 \in T \cdot t_3 \in T \cdot t_1 S_{t_2} \cdot t_2 S_{t_3}) \supset t_1 S_{t_3}),$$

where  $T$  is the class of all time impulses.

The ontology now has two individual variables, pitch-class and time. Using the notion of cardinal couples (*Principia Mathematica*) one can write the pitch "middle c" with time  $t$  as

$$(c, t).$$

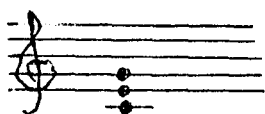
In more general terms, where  $C$  stands for the pitch-class and  $t$  stands for time, one of the twelve basic cardinal couples would be:

$$(C, t)$$

Others would be  $(C\#, t)$ ,  $(D, t)$ ,  $(E, t)$ , and so on. These are the "ultimate constituents of music," just as atoms are the "ultimate constituents of matter." It should also be noted that it is not "electrons" or "protons," and other subatomic particles that are so described, the basic unit is the atom. So too with the cardinal couples, for it is not pitch-class alone, but the cardinal couple of these two that are the basic units. However, one should distinguish between pitch-classes and classes of

cardinal couples. That is, the  $C$  in  $(C,t)$  represents a class (a pitch-class) but the whole couple,  $(C,t)$  itself also repeats a class, of all pitches  $C$  at time  $t$ . Thus, this theory would require an underlying logic of at least second order (for variables and quantifiers over classes).

How, then, are chords handled? One way to handle them would be by simple pitch summation such that for any two pitch-class  $x, y$ , there is a pitch-class  $(x+y)$  as their sum. Thus, for



the cardinal couple  $[(C + E + G), t]$  pertains. The weakness (if it may be so termed) is that such a treatment allows of many more combinations that are actually used. This is what Martin calls "overpopulation," but his case, from a composer's point of view, (particularly contemporary composition) is perhaps innocuous. Another way to handle chords would be to form cardinal couples of the dyad or triad, or whatever. For the chord above, one would then obtain:

$$[(C,t), (E,t), (G,t)]$$

and these three couples, taken together, also form a class (i.e., a class of a triad of cardinal couples, each couple of which is in itself a class). Slight re-adjustment allows this summation to become clear:

$$(C,t), (E,t), (G,t)$$

It can be claimed that this method does not have the redundancy or "overpopulation" of the first. Furthermore, in this type of chord

treatment at least a third-order logic is required for relationships between individuals and also between classes.

The second framework that is sketched involves the notion of virtual classes which differ from the real classes dealt with above in not being values for variables. With only slight alteration, the first treatment can be reduced to virtual cardinal couples, and hence of virtual classes of virtual cardinal couples. Using this notion of virtual classes, whether simultaneous or sequent, means that a musical composition may be described simply as a virtual class of virtual couples. If this be valid, then this would be a description of the ontological status of the musical composition within proto-theory. Only a first-order logical would be needed.

The third alternative that Martin suggests centers on event-logic whereby events only are allowed as values for variables. Instead of (C,t) one gets:

$$C_e$$

which indicates that event  $e$  is an "event of pitch C" which may be simul-taneous with  $E_e$  and  $G_e$  (i.e. the C-major chord) or sequential to  $E_e$ , etc. Such events may be of varying durations, a numerical measure of which may develop into a theory of rhythm. In other words, the effect of time is achieved by the temporal ordering of events. Thus, musical structure becomes wholly absorbed in event-logic if to the latter are added pitch predicates forming event-variables. Again, only a first-order logic is involved. Presumably, with any of these three methods,

the further development of theory would simply be a matter of introducing definitions to the various notions of harmony, counterpoint, and so on, and drawing out their logical consequences. What has been undertaken here is the identification of the primitive symbols. A further assumption is that additional primitive predicates can suitably handle other aspects of musical structure such as dynamics, timbre, and phrasing.

It is not altogether clear what Martin means by his reference to the finite number of pitch-classes of the chromatic scale "from the lowest to the highest." If he is simply referring to the chromatic scale (concept), then that finite number is twelve which is repeated over various octave ranges, and therefore there are only twelve cardinal couples, but which may be repeated depending on whether the vibrations per second are at double or half of any given reference pitch (say, "middle C"). Thus, "middle C" could be represented by the cardinal couple

$$(C, t)$$

and the "C" one octave above as

$$(C', t)$$

and the "C" one octave below as

$$('C, t)$$

and so on. This would mean that a pitch-class can be broken up into sub-pitch-classes (cf. sub-atomic particles). This is the position that the present exegesis has taken.

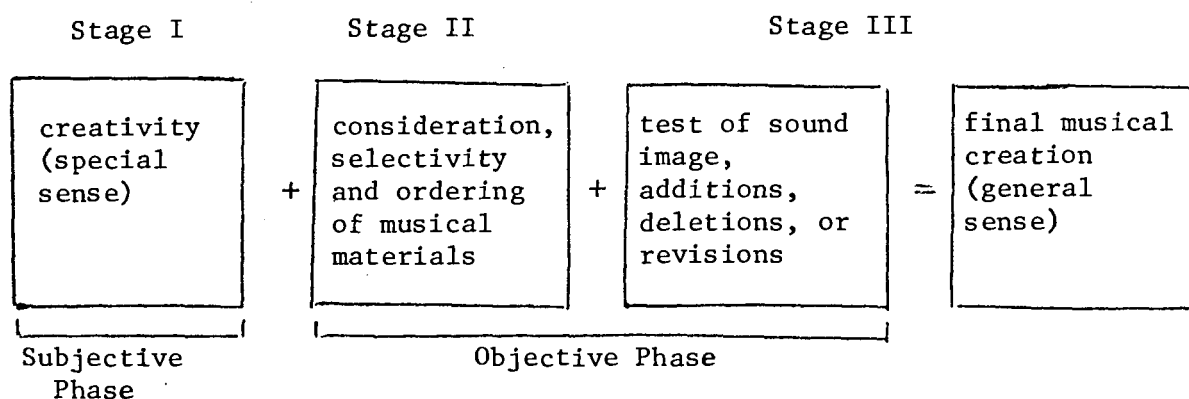
However, Martin's consistent reference to "middle c" throughout his whole discussion suggest that what he means by "from the lowest to the highest" is from the lowest "A" on the piano keyboard (for all western music is "keyboard-based") to the highest "C." Thus, the total number of pitch-classes will now be eighty-eight, but some sort of indication will still be required to show in which octave a given pitch-class occurs. For example, octaves may be numbered from the bass end, so that  $(D\sharp_2t)$  is the cardinal couple associated with the second octave range. Again the analogy, albeit somewhat forced, is between these subdivisions and the subatomic particles.

The thesis supports Martin's theory, even though it is incomplete, for it is one of the first serious attempts to systematize the primitives of musical structure into a fully-fledged explanatory theory. It has the status of what has been termed "basic theory" similar to that which Dalton's atomic theory has with respect to the structure of matter. The concept of "music theory" as presented in the immediately preceding section is also accommodated (cf. kinetic-molecular theory "subsumed under" the atomic theory). When such a theory has been more fully worked out, presented, and accepted by musicians and philosophers alike, then its location will be also under the objective phase category of "consideration of musical materials." (See summary of this Chapter and Chapter Five.)

## SUMMARY

This chapter has been an examination of musical creativity, focusing first on what both psychologists and musicians themselves have said (sections II and III). This established some of the characteristics of what has been termed the subjective aspect of musical creativity, and it has been found that these could be fitted into the same categories as used in Chapter Three. Sections IV and V were aimed at trying to make some sense of the notion of an "objective phase" of musical creativity. Section IV aimed at elucidating the concept of "music theory" *vis-à-vis* the concepts of "musical style," conceptual frameworks," "explanation," and "prediction." Section V was on exegesis of a recent attempt to formulate a logic underlying a basic theory of musical structure, and an analogy was drawn between this "Proto-theory" and a corresponding basic theory in the physical sciences, namely Dalton's Atomic Theory. Based on the arguments of this chapter, the following model (MC Model) may be diagrammed as follows:

### Musical Creativity





## FOOTNOTES

<sup>1</sup>M. I. Stein and S. J. Heinze, *Creativity and the Individual*, Chicago: Free Press of Glencoe, University of Chicago, 2nd ed., 1964, p. 76.

<sup>2</sup>R. M. Schafer, *British Composers in Interview*, London: Faber and Faber, 1963, p. 141.

<sup>3</sup>Stein and Heinze, *Creativity and the Individual*, p. 84.

<sup>4</sup>I. Stravinsky, *Poetics of Music in the Form of Six Lessons*, Translated by Arthur Knodel and Ingolf Dahl, New York: Vintage Books, 1947, p. 28.

<sup>5</sup>Schafer, *British Composers in Interview*, p. 132.

<sup>6</sup>*Ibid.*, p. 138.

<sup>7</sup>*Ibid.*, pp. 97-8.

<sup>8</sup>Stravinsky, *Poetics of Music*, p. 56.

<sup>9</sup>A. N. Whitehead, *Science and the Modern World*, New York: The New American Library of World Literature, Inc., 1964, p. 25.

<sup>10</sup>Schafer, *British Composers in Interview*, p. 132.

<sup>11</sup>*Ibid.*, p. 50.

<sup>12</sup>*Ibid.*, p. 42.

<sup>13</sup>*Ibid.*, p. 146.

<sup>14</sup>Stravinsky, *Poetics of Music*, p. 8.

<sup>15</sup>*Ibid.*, pp. 50-1.

<sup>16</sup>*Ibid.*, p. 51.

<sup>17</sup>*Ibid.*, p. 52.

<sup>18</sup>*Ibid.*, pp. 37-8.

<sup>19</sup>Stravinsky, *Poetics of Music*, pp. 39-40.

<sup>20</sup>I. Stravinsky and R. Craft, *Stravinsky in Conversation with Robert Craft*, Middlesex: Penguin Books Ltd., 1960, p. 34.

<sup>21</sup>Stravinsky, *Poetics of Music*, p. 66.

<sup>22</sup>Schafer, *British Composers in Interview*, p. 40.

<sup>23</sup>Stravinsky, *Poetics of Music*, pp. 24-5.

<sup>24</sup>A. Copland, *Music and Imagination*, New York: The New American Library of World Literature, Inc., 1964, p. 58.

## CHAPTER FIVE

### RELATIONSHIPS, CONCLUSIONS AND IMPLICATIONS

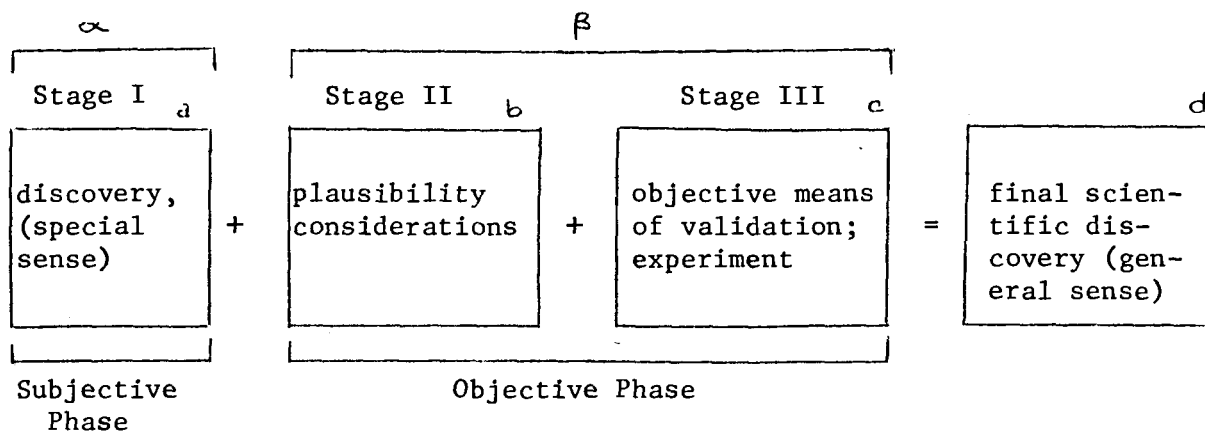
#### SECTION I

*The relationship of scientific discovery and musical creativity: "SD-MC model." Discussion of the relationship. Formal analysis of the SD-MC model: Symmetries and asymmetries. The formal analysis involves symbolization, but the important point is that these formulae can be interpreted in the context of the arguments of the thesis; hence, also the distinction between relevant and irrelevant symmetry and asymmetry formulae.*

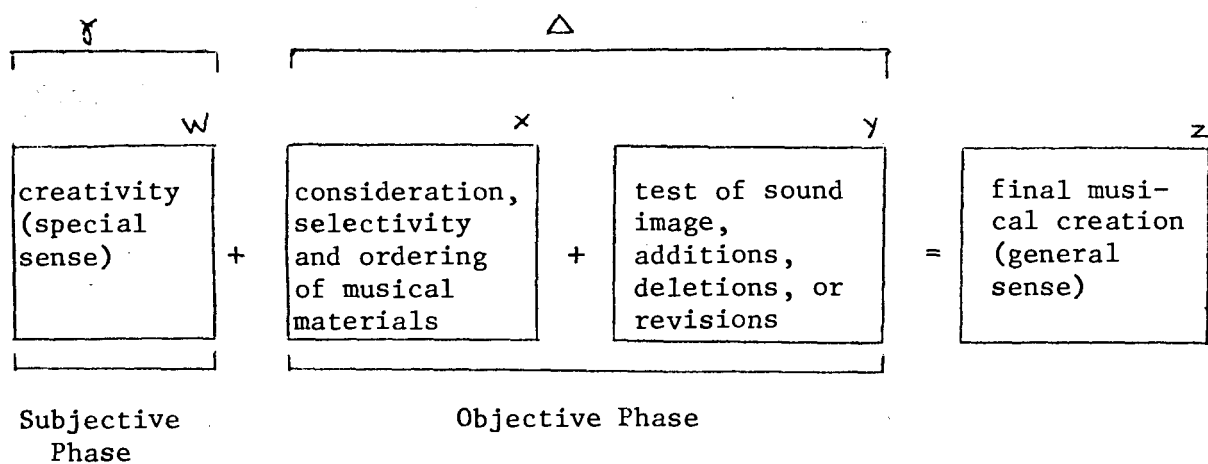
At the conclusions of each of Chapter Three and Four a model was presented to describe scientific discovery and musical creativity, respectively. When these models are juxtaposed, the relationship between the processes becomes apparent (see page 225).

It should be noted that the model for scientific discovery represents, essentially, the hypothetico-deductive method. There is no corresponding term in music to describe the process of musical composition. Thus, it is necessary to shift, in each case, to a more general description such that "scientific discovery" and "musical creativity" (called "general sense" in each case) are on the same levels of discourse. Furthermore, the boxes marked "discovery (special sense)" and

### Scientific Discovery

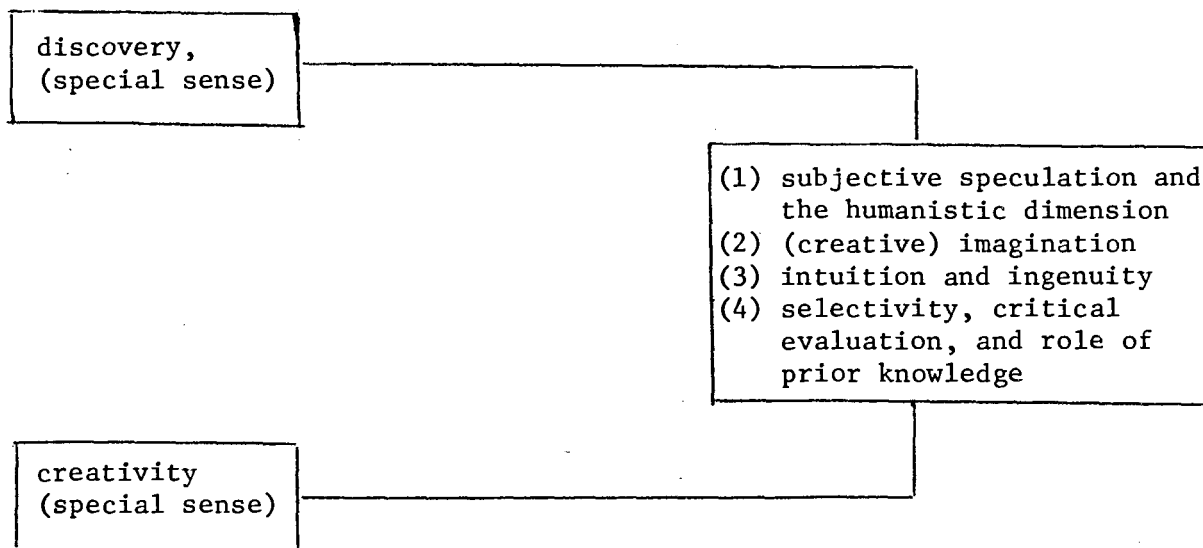


### Musical Creativity



### *SD-MC Model*

"creativity (special sense)" are not only equivalent temporally, but also equivalent characteristically; that is, the same categories are used in both instances. Also from the SD-MC Model, the following modifications of boxes  $a$  and  $w$  can be written:



Consider again the SD-MC Model. The following symmetrical relationships may be determined when  $R$  is read as "is symmetrical to"):

- (i)  $aRw$  and  $wRa$
- (ii)  $bRx$  and  $xRb$
- (iii)  $cRy$  and  $yRc$
- (iv)  $dRz$  and  $zRd$

It should be pointed out that (iv) is less "accurate" than the others. Temporally, it holds true, but the aim of the final musical composition is different from the aim of scientific research. In one, a musical composition is the end product; in the other, it is a scientific theory. Thus, as one goes in direction from left to right, the symmetry is less precise. Additional symmetry formulae are:

- (v)  $\alpha R\gamma$  and  $\gamma R\alpha$ , which is equivalent to  $aRn$  and  $wRa$ , and
- (vi)  $\beta R\Delta$  and  $\Delta R\beta$ , which is equivalent to  $(b+c)R(x+y)$  and

$(x+y)R(b+c)$ , and

(vii)  $(\alpha+\beta)R(\gamma+\Delta)$  and  $(\gamma+\Delta)R(\alpha+\beta)$ , which is equivalent to  $(a+b+c)R(w+x+y)$  and  $(w+x+y)R(a+b+c)$ .

All the formulae from (i) to (vii) can then be more precisely symbolized in the following series where  $D$  represents the class of all instances of the process of scientific discovery and  $C$  represents the class of all instances of the process of musical creativity, and  $R$  is the relationship "is symmetrical to":

- (i)'  $(a)(w) (a \in D \cdot w \in C \cdot aRw \supset wRa)$
- (ii)'  $(b)(x) (b \in D \cdot x \in C \cdot bRx \supset xRb)$
- (iii)'  $(c)(y) (c \in D \cdot y \in C \cdot cRy \supset yRc)$
- (iv)'  $(d)(z) (d \in D \cdot z \in C \cdot dRz \supset zRd)$
- (v)'  $(\alpha)(\gamma) (\alpha \in D \cdot \gamma \in C \cdot \alpha R\gamma \supset \gamma R\alpha)$
- (vi)'  $(\beta)(\Delta) (\beta \in D \cdot \Delta \in C \cdot \beta R\Delta \supset \Delta R\beta)$
- (vii)'  $(\alpha+\beta)(\gamma+\Delta) [(\alpha+\beta) \in D \cdot (\gamma+\Delta) \in C \cdot (\alpha+\beta)R(\gamma+\Delta) \supset (\gamma+\Delta)R(\alpha+\beta)]$

These, then are the relevant symmetries. (An example of an irrelevant one would be  $aRx$  and  $xRa$ .)

There are also some interesting asymmetries. In terms of greater explanatory power ( $E$ ), one obtains:

(viii)  $dEa$  and  $\sim aEd$

and this is just another formulation of the earlier asymmetry introduced in Chapter Three where  $x$  = "discovery that" and  $y$  = "discovery why."

These formulae from Chapter Three were:

$$xEy \quad \text{and} \quad \sim yEx, \quad \text{and} \\ (x)(y) (x \in A \leftrightarrow y \in A \leftrightarrow yEx \rightarrow \sim xEy)$$

where " $\rightarrow$ " (arrow) is to be treated, as equivalent to " $\supset$ " (hook) and where  $A$  in Chapter Three was taken to mean the class of all discoveries. To avoid confusion with the terminology of this chapter, let  $D$  replace  $A$  (of Chapter Three) as the class of all discoveries, so that (viii) above may be rewritten as:

$$(viii)' \quad (w)(z) (w \in D \leftrightarrow z \in D \leftrightarrow wEz \supset \sim zEw)$$

The other asymmetry formulae presented in Chapter Three were:

$$xOy \quad \text{and} \quad \sim yOx \\ yIx \quad \text{and} \quad \sim xIy \\ yCx \quad \text{and} \quad \sim xCy$$

In terms of the present model, these may be written as:

$$(ix) \quad wOz \quad \text{and} \quad \sim zOw \\ (x) \quad zIw \quad \text{and} \quad \sim wIz \\ (xi) \quad xCw \quad \text{and} \quad \sim wCz,$$

and these can all be rewritten as:

$$(xi)' \quad (w)(z) (w \in D \leftrightarrow z \in D \leftrightarrow wOz \supset \sim zOw)$$

where  $O$  is the relationship of "greater ontological reality";

$$(x)' \quad (w)(z) (w \in D \cdot z \in D \cdot zIw \supset \sim wIz)$$

where  $I$  is the relationship of "greater intensional reality";

$$(xi)' \quad (w)(z) (w \in D \cdot z \in D \cdot wCz \supset \sim zCw)$$

where  $C$  is the relationship of "more creative than."

For the musical creativity model, one can obtain:

$$(xii) \quad aSc \quad \text{and} \quad \sim cSa, \text{ and}$$

$$(xiii) \quad aSb \quad \text{and} \quad \sim bSa$$

where  $S$  is the relationship of "more subjective than"; formulae (xii) and (xiii) can be rewritten as

$$(xii)' \quad (a)(c) (a \in C \cdot c \in C \cdot aSc \supset \sim cSa) \quad \text{and}$$

$$(xiii)' \quad (a)(b) (a \in C \cdot b \in C \cdot aSb \supset \sim bSa)$$

where  $C$  is the class of all musical creations (i.e., compositions). With respect to emphasizing an objective phase of musical creativity, perhaps the following formulae may be more appropriate:

$$(xiv) \quad cOba \quad \text{and} \quad \sim aObc, \quad \text{and}$$

$$(xv) \quad bOba \quad \text{and} \quad \sim aObc, \quad \text{where}$$

$Ob$  is the relationship of "more objective than," and these can be rewritten as:

$$(xiv)' \quad (a)(c) (a \in C \cdot c \in C \cdot cOba \supset \sim aObc)$$

$$(xv)' \quad (a)(b) (a \in C \cdot b \in C \cdot bOba \supset \sim aObb).$$



As in the case of the symmetry formulae, the distinction between relevant asymmetries and irrelevant ones, *vis-à-vis* the arguments of this thesis, also pertains. An example of such an irrelevant formulae would be:

$$aRy \quad \text{and} \quad \sim yRa.$$

However, the formulae given, when taken by themselves, simply describe the model. It is important that they can also be interpreted within the context of the arguments in the thesis. For example, from the asymmetry formula

$$dEa \quad \text{and} \quad \sim aEd,$$

it can be seen that  $dEa$  represents the stage of the recognition of an unexplained phenomenon. It is also the stage at which the explandum of the deductive model is associated, and the level of what has been termed, in Chapter Three, "discovery that." The formula  $aEd$  represents, after successful testing for corroboration, the formulation of a theory. The transition from  $dEa$  to  $dEa$  within the discovery process represents scientific research, and is concerned with the explanans and with "discovery why." If one then writes:

$$aEd \rightarrow \sim dEa,$$

the arrow represents "testing," and scientific research can be viewed as a continuum at which some point Blackwell's notion of transformation occurs. This is just one example, but it is worth repeating that the

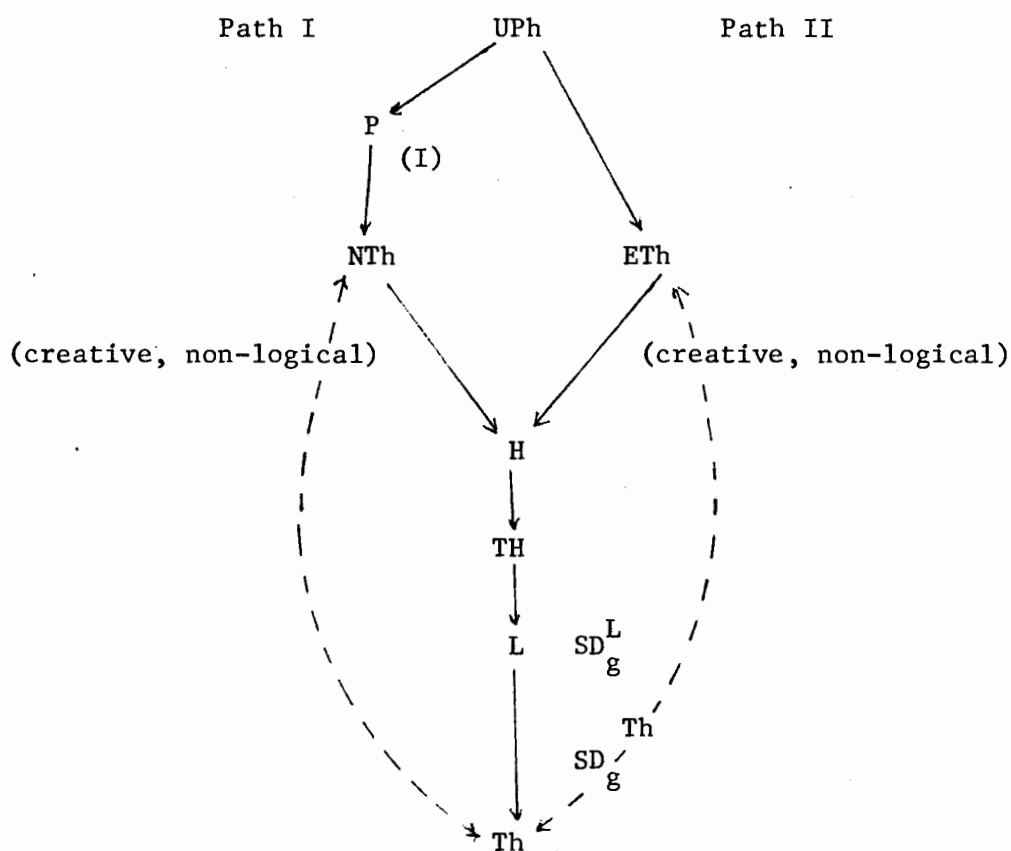
formulation of the model and the generation of formulae should not be seen as ends in themselves, but rather as convenient summaries of certain aspects of the arguments presented in this thesis. (See also Appendix L)

## SECTION II    *Conclusions and Implications*

- (1) The thesis supports the claim that there is no logic of discovery. The inductive techniques of Bacon and Mill, the plausibility considerations of Hanson, and the logic of heuristic plausibility inferences of Polya cannot be supported as such a logic. Models appear to be the best technique for the generation of fruitful hypotheses but again there is no logic by which the most fruitful model may be deduced. Moreover, models possess many intrinsic limitations which may arise from (a) overemphasis on symbols; (b) overemphasis on form; (c) oversimplification; (d) overemphasis on rigor; (e) map reading (incomplete isomorphism); (f) pictorial realism; (g) failure to accord with data. However, the failure of models to qualify as a logic of discovery does not imply that they are useless. Indeed, one of their distinct advantages is their powerful heuristic function in the context of discovery.
- (2) Scientific discovery (special sense), the generation of fruitful hypotheses, may be described as non-logical and creative. Psychologically, the process is characterized by divergent

thinking. The thesis also supports Blackwell's theory of discovery which, upon analysis, also reveals the creative nature (e.g., "creative postulation") involved.

- (3) The Scientific Research model (*SR Model C*) introduced in Chapter Two must now be revised as follows:



*SR Model D*

- (4) It cannot be assumed or argued, on *a priori* grounds, that scientific methodology is objective. To ascertain its nature, one must observe the manner in which scientists work, and, when this is done, a subjective, creative, and non-logical aspect of

discovery emerges. This leads to a distinction between the subjective and objective phases of discovery (general sense), the former of which is concerned with the context of discovery and the latter with the context of validation. It is also recognized that the term "objective phase" may be more accurately described by the term "intersubjective phase," since each investigator who attempts to duplicate the results of an experiment brings to that experiment his own private conceptual framework of predispositions, past experiences, predilections, and so on.

- (5) The thesis also supports Popper's criterion of falsification as the central criterion of demarcating scientific research from the non-scientific. Hypotheses should be as freely and imaginatively generated as long as they are subjected to rigorous test. This maximizes the researcher's ability to stand outside his framework in generating hypotheses, yet guards against the wanton formulation of hypotheses and theories that do not accord with the facts of experience. This has special implications for less developed sciences, such as some of the social sciences and education. The problem of value freedom is partially reconciled when one considers that research in the natural sciences also is not value free. Creative generation of hypotheses, conceptual frameworks with respect to theories, as well as "private" frameworks of investigators destroy the myth of "objectivity." It seems that when, for example, sociologists treat Marx's theory of society and history as "immutable" and

"objective" that the theory degenerates into dogma. Lack of understanding of the conceptual and philosophical foundations of inquiry often leads social sciences to misconstrue the nature of scientific activity, including this very point: scientific theories, though aiming at universality, are in fact held as provisional. To quote Popper again:

From the point of view here developed all laws, all theories, remain essentially tentative, or conjectural, or hypothetical, even when we feel unable to doubt them any longer.<sup>1</sup>

Popper claims that one of the tenets of social science is to propound historical prophecies; that is, to prophesy (historicism), and he goes on to state the impossibility of such:

Many of my fellow-rationalists are Marxists; in England, for example, a considerable number of excellent physicists and biologists emphasize their allegiance to the Marxist doctrine. They are attracted to Marxism by its claims: (a) that it is a science, (b) that it is progressive, and (c) that it adopts the methods of prediction which the natural science practise. Of course, everything depends upon this third claim. I shall therefore try to show that this claim is not justified . . .<sup>2</sup>

Basically, Popper's approach centers on the characteristics of social systems that have been discussed earlier--that society is not stationary nor well-isolated, that it is in constant change, and the development is not, in the main, repetitive. However, his argument is levelled principally at the historicists who fail to distinguish between what he calls scientific

prediction and unconditional historical prophesies. In fact most scientific predictions are conditional, for they assert that certain changes, such as the temperature of a copper wire, will be accompanied by other changes, such as expansion. Social sciences are similar in this respect, for Popper is not denying that social science cannot predict in an altogether similar manner (that is, conditional manner). He cites the case of the economist who can predict, that, under certain social conditions such as shortage of commodities, controlled prices, and the absence of an effective punitive system, a black market will develop. However, what Popper is emphasizing is that the historicist does not, and cannot derive his (unconditional) historical prophesies from what are in fact conditional predictions simply because such long-term prophesies can only be derived from scientific conditional predictions if (and this is very important) they apply to systems that are well-isolated, stationary, and recurrent. The solar system, for example, is just such a system, and eclipse prophesies are possible. This is but one of the very few such systems in nature (apart from the cyclic systems in biology--but, in a sense, the life cycles of organisms are only semi-stationary with respect to slow evolutionary changes). One does not need to despair, however, for Popper, by a series of arguments too detailed to be taken up here, goes on to say:

The main usefulness of the physical sciences does not lie in the prediction of eclipses; and similarly, the practical usefulness of the social sciences does not depend upon their power to prophesy historical or political developments.<sup>3</sup>

However, by viewing "prediction" from a different angle, viz., as the discovery of unintended consequences of one's actions, of the formulation of practical rules of what one cannot do, he writes:

The second law of thermodynamics can be expressed as the technological warning. 'You cannot build a machine which is 100 per cent efficient.' A similar rule of the social sciences would be, 'You cannot, without increasing productivity, raise the real income of the working population' and 'You cannot equalize real incomes and at the same time raise productivity.' An example of a promising hypothesis in this field which is by no means generally accepted--or, in other words, a problem that is still open--is the following: 'You cannot have a full employment policy without inflation.' These examples may show the way in which the social sciences are practically important. They do not allow us to make historical prophecies, but they may give us an idea of what can, and what cannot, be done in the political field.

We have seen that the historicist doctrine is untenable, but this fact does not lead us to lose faith in science or in reason. On the contrary, we now see that it gives rise to a clearer insight into the role of science in social life. Its practical role is the modest one of helping us to understand even the more remote consequences of possible actions, and thus of helping us to choose our actions more wisely.<sup>4</sup>

Social sciences, then, can make scientific predictions in the same way as the physical sciences, but they cannot make unconditional historical prophecies.

- (6) An important consequence of the results of this thesis, taken together with Popper's criterion is that of not only the

conditional nature of scientific predictions, but also the provisionality of scientific theories (this was touched upon in (5) above). Here again, in less developed sciences, the thesis holds that this latter feature is too often over-looked. Not only is it fallacious to treat theories as immutable, objective, and unconditional, but it is equally fallacious to cling on to any theory, even well-established ones, without regard to its possible refutation in whole or in part. Scientific knowledge is never complete, and the moment any researcher adopts any one or more of the above related fallacies, scientific knowledge will lose that dynamic quality essential to its existence. As Pei Sung Tang says, specifically in relationship to biological knowledge:

. . . new tools may have to be invented and new concepts introduced as the existing ones prove inadequate. And out of the observations and interpretations made with the aid of these new tools and new concepts, newer systems of knowledge may be constructed which govern the behaviour of the living world.<sup>5</sup>

This is further supported by Niels Bohr who writes:

The task of science is both to extend the range of our experience and to reduce it to order, and this task presents various aspects, inseparably connected with each other. Only by experience itself do we come to recognize those laws which grant us a comprehensive view of the diversity of phenomena. As our knowledge becomes wider, we must always be prepared, therefore, to expect alterations in the points of view best suited for the ordering of our experience. In this connection we must remember, above all, that as a matter of course, all new experience makes its appearance within the frame of our customary points of view and forms of perception.<sup>6</sup>



- (7) The relationship between scientific discovery and musical creativity should elucidate to some extent the psychological nature of these thought processes. As well, some additional insights on the concept of creativity can be gained. Musical creativity may then be said to share some scientific characteristics through the notions of "theory," "conceptual framework," "test," "explanation" "prediction," "subjectivity" and "objectivity" ("intersubjectivity"). However the relationship is not to be construed as a statement of the identity of one process with the other. The relationship supports the claim that the most successful scientists (social as well as natural) are those who combine subjectivity and objectivity in their work.
- (8) The relationship should dispel the myth of "inspiration" as the chief factor in musical composition. To be sure, such may occur in the initial subjective phase. It may be the sum of intuition, inventiveness, role of prior knowledge, and so forth. But at least equally important is the manipulation of musical materials and testing of the sound image. Indeed, this latter phase could be the one that plays the greater role.
- (9) The relationship should also dispel the myth of objectivity in science. In the crudest form, one must admit that hypotheses are guesses. There is no logically rigorous method for devising hypothesis; it is a common, fallacious, and vulgar error to speak of "deducing" them. Indeed, hypotheses are not deduced but are, rather, that which one deduces consequences from; and

if these predictions, after testing, are mistaken, then the hypothesis must be discarded, or at least modified. If, on the other hand, they are supported, then they are corroborated and are admitted on probation to the fund of scientific knowledge.

- (10) In view of the research of the present thesis, it might be suggested that the reporting of certain types of scientific research (such as some biological research) in journals be revamped as to format. It may be, as P. B. Medawar claims, "a fraud" for it misrepresents the processes of thought that accompanies or gave use to the work that is described in the paper. Too often the style used implies that discovery is an inductive process, and that the formulation of scientific theory begins with unbiased, "objective" observation. As Medawar himself says:

The conception underlying this style of scientific writing is an inductive process. What induction implies in its cruder form is roughly speaking this: scientific discovery, or the formulation of scientific theory, starts with the unvarnished and unembroidered evidence of the senses. It starts with simple observation--simple, unbiased, unprejudiced, naive, or innocent observation--and out of this sensory evidence, embodied in the form of simple propositions or declarations of fact, generalizations will grow up and take shape, almost as if some process of crystallization or condensation were taking place. Out of a disorderly array of facts, an orderly theory, an orderly general statement, will somehow emerge. This conception of scientific discovery in which the initiative comes from the unembroidered evidence of the senses was mainly the work of a great and wise, but in this context, I think, very mistaken man--John Stuart Mill.<sup>7</sup>

In the traditional format the general order is: introduction, review of literature, research design, results, discussion. In

the last section the investigator asks himself if the results are meaningful, and this is done with reference to specific questions which questions in fact, usually turn out to be the very ones that prompted the study. It may be suggested, then, that the introduction, usually a small section revealing the general area wherein one is going to work, should be replaced, or be supplemented, by the ideas that generated the study rather than pretending that these ideas, or truths, or laws have emerged from the contemplation of the results. This will then be in accord with the actual way scientists do act and hence contribute to the understanding of scientific discovery. Furthermore, this will not be inconsistent with the notion that unforeseen further implications and ideas may very well come out of the results. However, further testing will then be required.

## FOOTNOTES

<sup>1</sup>K. Popper, *Conjectures and Refutation: The Growth of Scientific Knowledge*, New York and Evanston: Harper and Row, 1968, p. 51.

<sup>2</sup>*Ibid.*, p. 337.

<sup>3</sup>*Ibid.*, p. 341.

<sup>4</sup>*Ibid.*, p. 343.

<sup>5</sup>P. S. Tang, *Green Thralldom: Essays of A Chinese Biologist*, London: George Allen and Unwin Ltd., 1949, p. 124.

<sup>6</sup>D. Hawkins, "The Creativity of Science" in H. Brown (ed.), *Science and the Creative Spirit*, Toronto: University of Toronto Press, 1958, p. 143.

<sup>7</sup>P. B. Medawa, "Is the Scientific Paper a Fraud?" in D. Edge (ed.), *Experiment*, London: British Broadcasting Corporation, 1964, p. 8.

**BIBLIOGRAPHY**

## BIBLIOGRAPHY

- Arasteh, A. Reza. *Creativity in the Life Cycle*. Vol. I. - An Annotated Bibliography, Copyright by E. J. Brill; Linden, Neth., 1962.
- Ayer, A. J. "The A Priori" in *Readings in the Theory of Knowledge*. Canfield and Donnell (eds.), New York: Appleton-Century-Crofts, 1964.
- Belth, Marc. "The Study of Education as The Study of Models" in *Alberta Journal of Educational Research*, Vol. XII, No. 3, Sept. 1966.
- \_\_\_\_\_. *Education as a Discipline*. Boston: Allyn and Bacon, 1965, *passim*.
- Bertalanffy, Ludwig von. "An Outline of General Theory," *British Journal of The Philosophy of Science*, 1:2:4, 1950.
- Beveridge, W. I. B. *The Art of Scientific Investigation*. New York: Vintage Books, Inc., 1957.
- Black, Max. *Models and Metaphors*. Ithaca: Cornell University Press, 1962.
- Blackwell, R. *Discovery in the Physical Sciences*. Saint Louis: Notre Dame University Press, 1969.
- Braithwaite, R. B. *Scientific Explanation*. London: Cambridge University Press, 1953.
- Bridgman, P. W., Nobel Laureate. *The Nature of Physical Theory*. New York: Princeton University Press, 1964.
- Brodbeck, May. "Models, Meaning and Theory," in L. Gross (ed.), *Symposium in Sociological Theory*. New York: Harper and Row, 1959.
- Brown, R. *Explanation in Social Science*. London: Routledge and Kegan Paul, 1963.
- Buckley, W. *Sociology and Modern Systems Theory*. Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1967.
- Copland, Aaron. *Music and Imagination*. New York: The New American Library of World Literature, Inc., 1964.
- Cregan, Robert F. "A Modelistic Educational Theory," *School and Society*. 87:511-512, 1959.
- de Santillana, G. *The Origins of Scientific Thought*. New York: Mentor paperback, 1961.

- Deutsch, Karl W., F. E. L. Priestley, Harcourt Brown, David Hawkins.  
"Essays on Humanistic Aspects of Science," *Science and the Creative Spirit*. Toronto: University of Toronto Press, 1958.
- Downey, Lawrence W. *The Secondary Phase of Education*. New York: Blaisdell Publishing Company, 1965.
- Dray, William H. *Laws and Explanation in History*. London, Oxford University Press, 1964.
- Durbin, Paul O. P. *Logic and Scientific Inquiry*. Milwaukee: Bruce Publishing Co., 1968.
- Duverger, M. *Introduction to the Social Sciences*. London: George Allen and Unwin Ltd., 1961.
- Eastwood, Gordon R. "Use of Models: Another Dimension," in *Alberta Journal of Educational Research*, Vol. XII, No. 3, Sept. 1966.
- \_\_\_\_\_. "Dimensions of Systematic Enquiry" (hand-out).
- Forte, Allen. *The Compositional Matrix*. New York. Music Teachers National Association, 1961.
- Furtado, C. *Development and Underdevelopment*. Berkeley: University of California Press, 1967.
- Geiringer, Karl. *Brahms His Life and Work*. New York: Doubleday & Company, Inc., 1961.
- George, J. H. "Models and Theories in Social Psychology," in L. Gross (ed.), *Symposium on Sociological Theory*. New York: Harper and Row, 1959.
- Ghiselin, Brewster. *The Creative Process*. Berkeley, University of California Press, Mentor paperback, 1967.
- Gibson, Q. *The Logic of Social Enquiry*. London, Routledge and Kegan Paul, 1960.
- Griffiths, Daniel E. *Administrative Theory*. New York: Appleton-Century-Crofts, 1959.
- Grout, Donald Jay. *A History of Western Music*. New York: W. W. Norton and Company, Inc., 1960.
- Halsey, A. H., Jean Floud, and C. Arnold Anderson (eds.). *Education, Economy and Society*. New York: The Free Press of Glencoe, Inc., 1961.
- Hanson, N. R. "Is There a Logic of Discovery?" in *Current Issues in The Philosophy of Science*, H. Feigl and G. Maxwell (eds.). New York: Holt, Reinhardt and Winston, 1961.

- Hanson, Norwood Russell. *Patterns of Discovery*. London: The University of Cambridge Press, 1961.
- Hawkins, David. "The Creativity of Science" in Harcourt Brown (ed.), *Science and the Creative Spirit*. Toronto: University of Toronto Press, 1958.
- Hempel, Carl G. *Philosophy of Natural Science*. Englewood Cliffs: Prentice-Hall, Inc., 1966.
- Hesse, Mary. "The Role of Models in Scientific Theory," in Dudley Shapere (ed.) *Philosophical Problems of Natural Science*. New York: Macmillan Co., 1965.
- \_\_\_\_\_. *Models and Analogies in Science*. South Bend, Indiana: University of Notre Dame Press, 1966.
- Holton, Gerald. "Presupposition in the Construction of Theories," Woolf, Harry. *Science as a Cultural Force*. Baltimore: The John Hopkins Press, 1964.
- Hospers, John. *An Introduction to Philosophical Analysis*. Englewood Cliffs, New Jersey, 2nd ed., Prentice-Hall Incorp., 1967.
- Howard, John Tasker and James Lyond. *Modern Music*. New York, Mentor paperback, 1962.
- Hume, D. *Enquiry Concerning Human Understanding*. London, 1748. Published as *Philosophical Essays Concerning Human Understanding*. Modern edition by C. W. Hendel, New York, 1955.
- Kaplan, Abraham. *The Conduct of Inquiry*. San Francisco: Chandler Publishing Co., 1964.
- Kluckhohn, C. *Mirror for Man*. New York: Fawcett World, February, 1957. Premier edition.
- Kuhn, Thomas S. *The Structure of Scientific Revolution*. Chicago: University of Chicago Press, 1962.
- Lindsay, Robert Bruce. *The Nature of Physics : A Physicist's Views on the History and Philosophy of His Science*. Providence, Rhode Island, Brown University Press, 1968.
- Maccia, Elizabeth S. and George S. Maccia. "Educational Theorizing Through Models," in *Alberta Journal of Educational Research*. Vol. XII, No. 3, Sept., 1966.



- Maccia, Elizabeth S, George S. Maccia, Robert E. Jewett (eds.). *Construction of Educational Theory Models*, Cooperative Research Project No. 1632. Columbus: Ohio State University, 1963.
- Macdonald, John. "A Psychologists Response," in *Alberta Journal of Educational Research*. Vol. XII, No. 3, Sept. 1966.
- Maciver, D. A. and F. A. Holdway. "An Analysis of the Use of Models in Education," *Alberta Journal of Educational Research*, Vol. XII, No. 3, Sept. 1966.
- Martin, R. J. "The Proto-theory of Music" in *Perspectives of New Music*. Princeton, New Jersey, Winter 1971.
- Marx, K. *Das Kapital*, 3 Vols., Hamburg, 1867-1894. Vols. II and III ed. by F. Engels; Vol. III in two parts. English translation, Chicago, 1915. Vol. I translated by S. Moore and E. Aveling, revised and amplified according to the fourth German edition by E. Untermann.
- Medawar, P. B. "Is the Scientific Paper a Fraud?", David Edge, *Experiment*. London, British Broadcasting Corporation, 1964.
- Merton, R. *Social Theory and Social Structure*, revised and enlarged edition. New York. Free Press of Glencoe, Inc., 1957.
- Mills, C. Wright. *The Power Elite*. New York, Oxford University Press, 1961.
- Myrdal, G. *Rich Lands and Poor*. New York and Evanston: Harper and Row, 1957.
- Nagel, Ernest. *The Structure of Science*. New York: Harcourt, Brace and World, 1961.
- \_\_\_\_\_, Patrick Suppes, and Alfred Tarski. *Logic, Methodology and the Philosophy of Science*. Stanford: Stanford University Press, 1962.
- Pap, Arthur. *An Introduction to the Philosophy of Science*. The Free Press of Glencoe, 1962.
- Peirce, Charles S. *Essays in the Philosophy of Science*. Indianapolis: Liberal Arts Press, Inc., 1957.
- Perle, George A. *Serial Composition and Atonality*. Berkeley and Los Angeles, University of California Press, 1963.
- Polya, George. *How to Solve It*. Princeton, N.J.: Princeton University Press, 1945.

Polya, George. *Mathematics and Plausible Reasoning*. 2 Vols. Princeton, N.J.: Princeton University Press, 1945.

\_\_\_\_\_. *Mathematical Discovery*. 2 Vols. New York: John Wiley and Sons, 1962, 1965.

Popper, Karl R. *Conjectures and Refutations: The Growth of Scientific Knowledge*. New York and Evanston, Harper and Row, 1968.

\_\_\_\_\_. *The Logic of Scientific Discovery*. New York: Basic Books, Inc., 1959.

\_\_\_\_\_. *The Poverty of Historicism*. New York, Harper & Row, 1964.

Rapoport, Anatol. "Uses and Limitations of Mathematical Models in Social Science," in Llewellyn Gross (ed.) *Symposium on Sociological Theory*. New York: Harper and Row, 1959.

Reichenbach, Hans. *The Rise of Scientific Philosophy*. Berkeley and Los Angeles: University of California Press, 1958.

Riseman, David, et al. *The Lonely Crowd: a Study of the Changing American Character*. New Haven, Yale University Press, 1950.

Rudner, Richard S. *Philosophy of Social Science*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966 (Foundations of Philosophy series).

Russell, B. *History of Western Philosophy*. London: George Allen and Unwin Limited, 1967.

Salmon, Wesley C. *The Foundations of Scientific Inference*. U.S.A.: University of Pittsburgh Press, 1967.

Salzman, Eric. *Twentieth Century Music: An Introduction*. Englewood Cliffs: Prentice-Hall, Inc., 1967.

Schafer, R. Murray. *British Composers in Interview*. London: Faber and Faber, 1963.

Scheffler, Israel. *Science and Subjectivity*. New York: The Bobbs-Merrill Company Inc., 1967.

Schrodinger, Erwin C. *Science, Theory and Man*. New York: Dover Publications, Inc., especially Chapter VII, "Conceptual models in Physics and their Philosophical Value."

Simon, Herbert A. "Scientific Discovery and the Psychology of Problem Solving," in Colodny, R. G., (ed.), *Mind and Cosmos*, U.S.A. University of Pittsburgh Press, 1966.

- Snow, Sir Charles Percy. *The Two Cultures and the Scientific Revolution*. New York: Cambridge University Press, 1961.
- Spinoza, B. *Ethics* (1677), translated by R. H. M. Elwes.
- Stein, M. I., and Stirling J. Heinze. *Creativity and the Individual*. Summaries of Selected Literature in Psychiatry and Psychology. Free Press of Glencoe, University of Chicago, 2nd ed. 1964.
- Stravinsky, Igor. *Poetics of Music in the Form of Six Lessons*. Trans. by Arthur Knodel and Ingolf Dahl, New York: Vintage Books, 1947.
- \_\_\_\_\_, and Robert Craft. *Stravinsky in Conversation with Robert Craft*. Middlesex: Penguin Books Ltd., 1960.
- Szent-Gyorgyi, Albert. *The Way of the Scientist*. Interviews from the World of Science and Technology, Selected and Annotated by the Editors of International Science and Technology. New York: Simon and Schuster, Inc., 1966.
- Taffel, A. *Physics : Its Methods and Meanings*. Boston, Allyn and Bacon, Inc., 1966.
- Tang, Pei-Sung. *Green Thralldom : Essays of a Chinese Biologist*. London, George Allen and Unwin Ltd., 1949.
- Theobald, D. W. *An Introduction to the Philosophy of Science*. London: Methuen and Co. Ltd., 1968 (also, University paperback).
- Toulmin, Stephen. *The Philosophy of Science*. New York: Harper and Row, 1960, especially Chapter II ("Discovery").
- Tovey, Donald Francis. *The Forms of Music*. Cleveland and New York. Meridian Books, 1961.
- \_\_\_\_\_, *A Companion to Beethoven's Pianoforte Sonatas*. London, The Associated Board of the Royal School of Music, 1931.
- Turner, Merle B. *Philosophy and the Science of Behaviour*. New York: Appleton-Century-Crofts, 1965.
- Ulrich, Homer. *Music : A Design for Listening*. New York, Harcourt, Brace and World, 1957.
- Wartoksky, Marx W. *Conceptual Foundations of Scientific Thought*. New York: The Macmillan Company, 1968.
- Watson, James D. *The Double Helix*. Toronto, The New American Library of Canada Limited, 1969.

Weinstock, Herbert. *Chopin : The Man and His Music*. New York, Alfred A. Knopf, Inc., 1965.

Whitehead, Alfred North. *Science and the Modern World*. New York: The New American Library of World Literature, Inc., 1964.

Woolf, Harry. *Science as a Cultural Force*. Baltimore: The Johns Hopkins Press, 1964.

## APPENDICES

## APPENDIX A

## APPENDIX A

The riskiness of Einstein's theory of gravity is brought out by his own admission that if one part of his theory proved wrong, the entire edifice would come tumbling down. In November of 1970 scientists at Pasadena's Jet Propulsion Laboratory beamed radio signals past the sun toward Mariners 6 and 7, a distance of some 250 million miles which were automatically amplified on board and transmitted back to earth. The process took 43 minutes. From analysis of data, new evidence was adduced in support of Einstein's 1916 General Theory of Relativity.

Although Einstein's theory offers the most comprehensive explanation of gravity since Newton, it has recently encountered its most serious challenge (refer to Popper's criterion). One consequence of Einstein's theory is that light and other electromagnetic waves should be measurably bent when passing through a strong gravitational field. Recently, however, Einstein's equations have been contested by Robert Dicke (Princeton) and Carl Brans (Loyola, New Orleans) who argue that such waves are bent to a lesser extent than Einstein had predicted.

By Einsteinian calculations, a radio signal traveling past the sun to the Mariner 6's position at the time of the test should take about 200 millionths of a second longer than if it did not pass through the sun's gravitational field; this is because the signal's path would be curved, not straight. The Brans-Dicke theory, on the other hand, predicted less curvature and a slowing down of 186 millionths of a second. Although measurements of bending have been done before, no firm case could be made for either theory. However, with error reduced to 4% the experimenters calculated that the signal to Mariner was slowed down by 204 millionths of a second on its round trip, only 4 millionths of a second off the Einsteinian prediction, but 18 millionths of a second off the Brans-Dicke figure. In Popperian language, Einstein's theory has again been corroborated.

**APPENDIX B**



## APPENDIX B

With respect to theory construction, the fundamental problem is that some analytic statements may be taken as synthetic statements, and vice versa (consider the statement: "All swans are white"). Confusion, in terms of theory construction, can result from a particular statement being "true by definition," yet not clearly indicated as such, or confusion can also result from a statement's being taken first as synthetic (and it is possible to empirically verify, either in practice and/or in principle, a synthetic statement) and later as analytic in the desire to avoid the inconvenience of subjecting a cherished hypothesis to the context of falsification. The danger is readily apparent, for it is possible to take a statement appearing to be an empirical proposition (when in fact it is not) and to conclude with some remarks on the implications such sentences may have not only in theory construction but also in theory application.

It is now possible to discuss some of the strengths and weaknesses inherent in analytic statements *vis-à-vis* theory construction and application. Their weakness is evident (as Ayer points out), in that

. . . none of them provide any information about any matter of fact. In other words, they are entirely devoid of factual content.<sup>1</sup>

In short, they tell us what we already know, and this is because of the very definitions of the words involved in the statements. However, the strength of analytic statements is that they cannot be refuted by experience because they do not make any assertion about the empirical world--they only appear to do this. One does not have to see a bald man to know that the statement: "All bald men are bald" is true. As Ayer points out, analytic statements simply record one's determination to use words

---

<sup>1</sup>A. J. Ayer, "The A Priori" in *Readings in the Theory of Knowledge*, Canfield and Donnell (eds.), New York: Appleton-Century-Crofts, 1964, p. 243.

in a certain manner. For these reasons, then, it can be seen why it is necessary to analyze analytic statements for they are often used as laws or lawlike generalizations in theory construction. Not only may they record one's determination to use words in a certain fashion, they may also lead one, or predispose one, to follow only certain lines of investigation which may, instead of revealing some new relationship, simply result in confirmation of some relationship already known to exist.

An hypothesis may be untestable for different reasons. The properties it refers to may be poorly defined or because the generalization may be true by definition, in which case there can be no question of submitting such an hypothesis to testing. (An hypothesis is a proposition which is assumed in order to draw out its logical consequences, and thus to test its accord with facts which are known or may be determined).

However, in the last analysis, it may be unimportant whether the statements that compose a theory are analytic, synthetic, or both. What is important is whether or not the lines of investigation are fruitful, and that no data or information are overlooked as unimportant or irrelevant when the decision as to what is important or relevant is based, or may be based, on an unwarranted inference possibly due to the "true by definition" nature of hypotheses.

**APPENDIX C**

## APPENDIX C

The question of "what is an electron" is still unanswered. The advances in physics only give the concept a slightly sharper image. Attempts to understand it have become more complex and it becomes apparent that its "existence" and properties are inextricably connected with other subatomic particles: photons (scattering of electrons); pairs of electrons and anti-electrons; neutrinos (particles with the same angular momentum as electrons, but with no charge, and, it is believed, no mass); muons (identical to an electron, but heavier in weight). The actual "existence" of these theoretical constructs, and the way they may fit into a hierarchy, escapes understanding. Furthermore, any explanation of the electron must include the muon (which particle particularly confounds physicists), and hence makes the understanding of the electron all the more difficult. To make matters even more complicated, there are transmutations of particles to be considered. For example, the muon, after existing about one millionth of a second decays into two neutrinos and an electron. To the physicist, the one constant invariance of these particles and transmutations is that they all conform to the laws of conservation. An electron may disappear, but one is confident that an electron-neutrino will appear in its place.

In this century, physics has incorporated these concepts into mathematical theories of their interactions. These theories do predict, with good approximation the behaviour of electrons in atoms, molecules, or high energy collisions; and herein lies their special value.

**APPENDIX D**

## APPENDIX D

It is now time to discuss the distinction between "categories" and "concepts," for the introduction of concepts in theory construction is of extreme importance.

The basic similarity between "concept" and "category" is that they are both composed of "parts" or "properties." However, that is apparently, where the similarity ends. The differences are now shown schematically:

<u>Concept</u>	<u>Category</u>
1. an "open" system--the <u>known</u> conceptual properties are <u>specifiable</u> , but are <u>indefinite</u> in number, and are <u>not arbitrarily</u> selected.	a "closed" system--the categorical properties are <u>specifiable</u> , <u>finite</u> in number, and may be <u>arbitrarily</u> selected.
2. conceptual properties <u>always</u> display conjunction, never disjunction. Membership to a concept must be on the basis of some entity's possession of all the known conceptual properties of any given concept. It may be that the different conceptual properties may vary in "weight," or importance, but they are <u>all</u> relevant.	categorical properties can display either conjunction <u>or</u> disjunction. Membership in a category can be specified on the basis of some entity's possession of <u>all</u> the categorical properties (conjunction) or, on the basis of some entity's possession of <u>any one or some</u> of the properties (disjunction).
3. concepts <u>can</u> stand for all time, or they <u>can</u> and <u>do</u> change with time.	a category, once formulated, can stand for all time, as there may never arise any reason to change it.
4. tends to be "natural" in that it allows for the discovery of many more properties than those originally recognized.	tends to be "artificial" in that one cannot do more with it than one first intended. If a category is found unsatisfactory, one can change it by adding or deleting properties.
5. addition of properties results, not in a new concept, but in a more <u>fully formed</u> one; if a	addition or deletion of properties results logically in a <u>new</u> category.

ConceptCategory

property is deletable one can say it was never a conceptual property in the first place or the property is kept, but is given less "weight."

6. concepts are limited in use to      categories are useful, but limited.  
the extent of our knowledge.

A few comments on this chart are needed. With respect to items 3 and 6, it may be argued that one's conception of what is possible, even of what is logically possible, grows with the growth of knowledge. The same is true for concepts, they grow and become more fully formed along with our knowledge. This is why conceptual properties can be said to be indefinite in number, for it would be difficult to conceive of knowledge having an absolute limit. A scientific concept has meaning only because scientists mean something by it, but this does not imply that it is possible to know everything about any given concept. By leaving concepts "open" in this way, conceptual meaning becomes more comprehensive as knowledge increases. Kaplan would argue that concepts in fact function as rules for judging or acting, as prescriptions for organizing the materials of experience. When seen in this light, then, as a concept becomes more comprehensive, so too does one's ability to judge and act. In addition, the context within which one uses a concept also becomes important, and, as such, concepts may be used as an heuristic technique in leading one to discover contextual relevance, a seemingly desirable complement to any theory.

It is now time to relate the above discussion to theory construction. Proper concepts are required to formulate good theories, but, in addition, adequate theories are needed to arrive at good concepts. The interaction of these two "levels" is important. Moreover, if one confuses "categories" with "concepts," if one fails to see the crucial distinctions between them, then the chances of formulating good theories will be reduced, for, by mistaking a "category" for a "concept," and

introducing it into the theory, that theory will be limited. That is to say, the very limitations of the category will be the very limitations of the theory. If the mistake is made, the theory becomes akin to an *ad hoc* hypothesis--explaining what it set out to explain, and nothing more.



## APPENDIX E

## APPENDIX E

Biologists and philosophers agree that the biological and behavioural sciences are fundamentally non-teleological, and that appropriate translation rules exist to transform such statements as:

"The function of x in z is to do y"

into

"z does y by using x"

"y is an adaptation."

(This approach was expounded by Nagel--and has recently been discussed by Michael E. Ruse in *Philosophy of Science*, Official Journal of the Philosophy of Science Association, Vol. 38, No. 1, March, 1971).

## APPENDIX F

## APPENDIX F

The Kinetic-Molecular Theory may be considered a standard scientific theory. An outline of it is presented here in order that some of the main ideas and concepts may be correlated. It may prove fruitful to closely read the text of Chapter Two and the following outline and "reconstruct" the theory imaginatively.

*A detailed investigation of a scientific theory in order to correlate the key terms of scientific research: The Kinetic Molecular Theory. Empirical observation suggests postulates. The theory as a model and the four basic assumptions. Justifications of these assumptions. Boyle's Law; Charles' Law; Dalton's Law; Brownian Motion; Graham's Law.*

One phenomenon that yields a clue to the observed behaviour of gases is suggested by Brownian motion, first observed by the Scottish botanist, Robert Brown, in 1827. It can be observed by focusing a microscope on a particle of cigarette smoke when illuminated from the side. The particle of cigarette smoke appears to be jostled by its neighbouring molecules so as indirectly to suggest the motion of the submicroscopic, invisible molecules of matter. This, then, is powerful, empirical support for the idea that matter consists of extremely minute particles which are constantly in motion. Indeed, the very existence of Brownian motion contradicts the idea of matter as a quiescent state. This "moving-molecule" theory is known as the kinetic theory of matter, and it is founded on basic postulates.

The kinetic theory represents a model which is proposed to account for an observed set of empirical facts. In order that the model be practical, certain simplifying assumptions must be made about its properties. The validity of each assumption and the reliability of the whole model can be ascertained by how well the facts are explained, and

by how well additional facts may be predicted.

- (1) Gases consist of tiny molecules, which are so small and so far apart on the average that the actual volume of the molecules is negligible compared to the empty space between them.
- (2) In the perfect gas, there are no attractive forces between molecules. The molecules are completely independent of each other.
- (3) The molecules of a gas are in rapid, random, straight-line motion, colliding with each other and with the walls of their container. In each collision, it is assumed that there is no net loss of kinetic energy (Law of Conservation of Energy), although there may be a transfer of energy between the partners in the collision.
- (4) At a particular instant in any collection of gas molecules, different molecules have different speeds and, therefore, different kinetic energies. However, the average kinetic energy of all the molecules is assumed to be directly proportional to the absolute temperature.

Before discussing each of these assumptions, it might be asked how the model is related to the observable quantities  $V$ ,  $P$ , and  $T$ , where  $V$  is the volume,  $P$  is the pressure, and  $T$  is the temperature. The accepted model of a gas is that it consists mostly of empty space in which billions of tiny points representing molecules move in violent motion, colliding with each other and with the walls of the container. The *volume* of a gas is mostly empty space but is *occupied*, in the sense that moving particles occupy the entire region in which they move. *Pressure*, defined as force per unit area, is exerted by gases because molecules collide with the walls of the container. Each collision produces a tiny push, and the sum of all the pushes on 1 sq. cm. of wall in 1 sec. is the pressure. *Temperature* gives a quantitative measure of the average motion of the molecules.

That the first of the four assumptions listed is reasonable can be supported by the fact that the compressibility of gases is so great. Calculations show that, in oxygen gas, for example, at S.T.P. (that is, at standard temperature and pressure, which is  $0^{\circ}\text{C}$  (or  $273^{\circ}\text{K}$ ) and 1 standard atmosphere (760 mm. Hg) of pressure, 99.96 per cent of the total volume is empty space at any instant. Since there are  $2.7 \times 10^{19}$  molecules per cubic centimeter of oxygen gas at S.T.P., it can be calculated that the average spacing between molecules is about  $37 \times 10^{-8}$  cm., which is about thirteen times the molecular diameter. When oxygen or any other gas is compressed, the average spacing between molecules is reduced such that the fraction of free space is reduced.

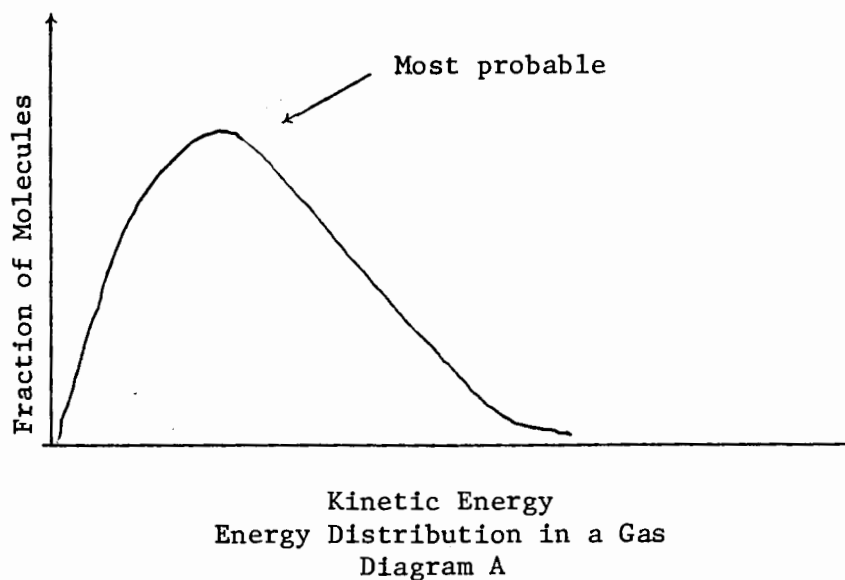
The validity of the second assumption can be supported by the observation that gases spontaneously expand to occupy all the volume accessible to them. This behaviour occurs even for a highly compressed gas, where the molecules are fairly close together and hence where any intermolecular forces should be greatest. This observation supports the claim that there is no appreciable binding of one molecule of a gas to its neighbours.

As already indicated, the observation of Brownian motion implies that molecules of a gas move, and in agreement with assumption 3. Like any moving body, molecules have an amount of kinetic energy equal to  $\frac{1}{2}ms^2$ , where  $m$  is the mass of the molecule and  $s$  is its speed. That molecules move in straight lines follows from the assumption of no attractive forces, for only if there were attractions between them could molecules be swerved from straight-line paths. Because there are so many molecules in a gas sample and because they are moving so rapidly (at  $0^{\circ}\text{C}$ . the average speed of oxygen molecules is about 1,000 miles per hour), there are frequent collisions between molecules. It is necessary to assume that the collisions are elastic (like those between billiard balls) for, otherwise, kinetic energy would be lost by conversion to potential energy (as by distorting molecules). If this were the case, motion of the molecules would eventually stop, and the molecules would settle to the bottom of the container. It might be noted that the distance a gas

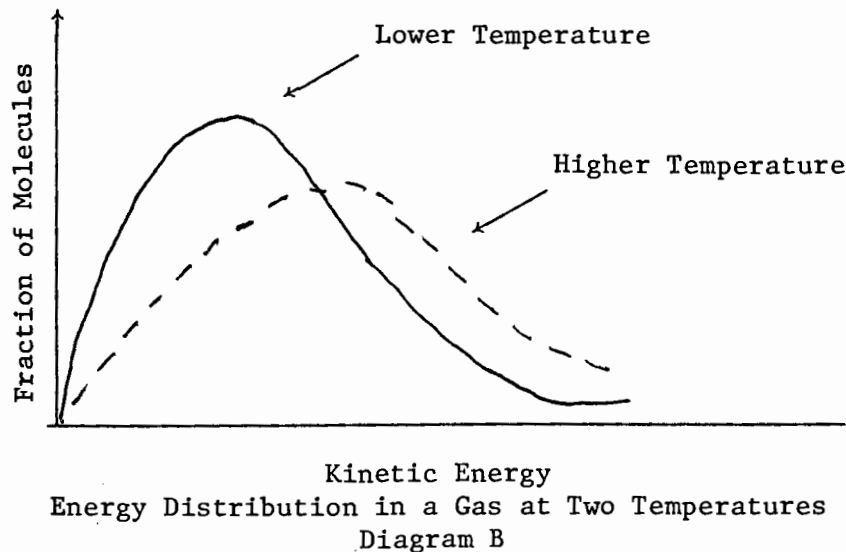
molecule has to travel before colliding elastically with another gas molecule is much greater than the average spacing between molecules, because the molecules have many near misses. In oxygen at S.T.P. the average distance between successive collisions, called the *mean free path*, is approximately 1,000 times the molecular diameter.

The fourth assumption consists of two parts: (1) that there is a distribution of kinetic energies and (2) that the average kinetic energy is proportional to the absolute temperature. The distribution, or range, of energies comes about as a result of molecular collisions, which continually change the speed of a particular molecule. A given molecule may move along with a certain speed until it hits another, to which it loses some of its kinetic energy; perhaps, later, it gets hit by a third and gains kinetic energy. This exchange of kinetic energy between neighbours occurs constantly, such that it is only the total kinetic energy of a gas sample that remains constant provided, of course, that no energy is added to the gas sample from the outside, as by heating. The total kinetic energy of a gas is made up of the contributions of all the molecules, each of which may be moving at a different speed. At a particular instant, a few molecules may be standing still with no kinetic energy; a few may have high kinetic energy; most will have kinetic energies near the average. The situation is summarized in the diagram below, which indicates the usual distribution of kinetic energies in a gas sample. Each point on the curve tells what fraction of the molecules have the specified value of the kinetic energy. (Diag.A)

The temperature of a gas may be raised by the addition of heat and this raises the question: "What happens to the molecules as the temperature is raised?" The heat which is added is a form of energy and so can be used to increase the speed of the molecules and, therefore, the average kinetic energy. This is shown in the following diagram, where the dotted curve describes the situation at higher temperature. (Diag.B). At the higher temperature the molecules have a higher average kinetic energy than at the lower temperature. Thus, temperature serves to measure the average kinetic energy. The assumption that average kinetic energy is directly



proportional to the absolute temperature is supported by the fact that predictions based on this assumption agree with experiment.



The kinetic theory, presented above, accounts for the observed behaviour of gases as follows:

- (1) *Boyle's law*. If the temperature is kept constant so that the molecules move with the same average speed, the pressure exerted by a gas depends only on the number of molecular



impacts per unit wall area per second. When the volume is reduced, the molecules do not have as much volume in which to move. They must collide with the walls more frequently, such that the walls of the container receive more pushes per second, and the observed pressure is greater in the smaller volume.

- (2) *Charles' law.* The effect of raising the temperature of a gas is to raise the average kinetic energy of the molecules. As the molecules move more energetically, they collide with the walls of the container more frequently and more vigorously, thus producing a greater pressure. If the external pressure on a balloon is constant but the temperature is raised, the gas expands the balloon to a larger volume in which the more vigorous molecular motion is compensated.
- (3) *Dalton's law.* It has already been indicated that the kinetic theory assumes that there are no attractive forces between the molecules of an ideal gas. In a mixture of gas molecules, each molecule strikes the walls the same number of times per second and with the same force as if no other molecules were present. Therefore, the partial pressure of a gas is not changed by the presence of other gases in the container.
- (4) *Brownian motion.* When a particle is suspended in a gas, gas molecules collide with it. If the particle is very large, the number of bombarding molecules on one side is about equal to the number of bombarding molecules on the other side. However, if the particle is small, so that the number of bombarding molecules at any instant is small, collisions on one side of the particle may predominate, so that the particle experiences a net force which causes it to move. An analogue of Brownian motion is observed when a small chunk of bread is thrown on the surface of a pool

in which there are many small fish. The bread darts to and fro as if propelled by some unseen force, the invisible force being due to the bumping of the nibbling fish. The larger the piece of bread, the less its erratic motion.

- (5) *Graham's Law*. This law follows directly from the fourth postulate of the kinetic theory, namely that the average kinetic energy of molecules is constant for a given temperature. When gas<sub>1</sub> and gas<sub>2</sub> are compared at the same temperature, average kinetic energy of gas<sub>1</sub> molecules = average kinetic energy of gas<sub>2</sub> molecules, or

$$1/2m_1s_1^2 = 1/2m_2s_2^2$$

where  $m_1$  and  $m_2$  are the masses of the different molecules and  $s_1$  and  $s_2$  are their average speeds:

$$\frac{s_1^2}{s_2^2} = \frac{1/2m_2}{1/2m_1} = \frac{m_2}{m_1}$$

or

$$\frac{s_1}{s_2} = \sqrt{\frac{m_2}{m_1}}$$

Qualitatively, this last equation indicates that heavier molecules move more slowly than light ones. It is reasonable to assume that the relative rates of diffusion of molecules are measured by their relative average velocities, so that

$$\frac{\text{Rate of diffusion of gas 1}}{\text{Rate of diffusion of gas 2}} = \frac{R_1}{R_2} = \frac{m_2}{m_1}$$

This is Graham's law.

## APPENDIX G

## APPENDIX G

A final point concerning models and explanations is now to be considered, and this involves the idea of alternate models as alternate explanations of phenomena. An example from physics will be discussed.

The phenomenon of light has interested and puzzled men from earliest times. The ancients were acquainted with the simpler phenomena such as rectilinear propagation, refraction, and, possibly, the principle of some optical instruments. They thought of light as a stream of particles or corpuscles. During the 17th century, the phenomena of interference and diffraction were noted. Huygens offered the explanation that light was a wave motion. Newton, who is most famous for his work in mechanics and the law of gravitation but who also made extensive researches in light ("corpuscular theory of light"), rejected Huygens' wave theory because this did not seem to be compatible with the rectilinear propagation of light, i.e., the fact that light travels in straight lines and casts sharp shadows.

The weight of Newton's authority was so great that for a century little attention was paid to the wave theory of light. It was then shown that the straight line motion of light is only approximately true, the departure in most cases being small because the wave length of light is extremely short, about two one-hundred-thousandths of an inch. The particle theory of light then lost favor, and was discarded completely when the speed of light in water was measured and was found to be less than the speed in air, a result predicted by the wave theory and contrary to the prediction of the particle theory.

Once it was established that light was a wave motion, the problem remained as to what constituted the medium for this wave motion. In order to account for the high speed of light and the fact that light travels in empty space, the ether was invented. The ether was conceived as an intangible elastic medium that pervaded all space. It explained

fairly well the way light behaved, as is only natural since the properties which were assigned to it were chosen for exactly that purpose. In the 19th century, Maxwell in the course of his development of the theory of electricity and magnetism, predicted the existence of electromagnetic waves. His theory included a prediction of the speed of these waves, and the predicted speed was the same as the known speed of light. Hertz then generated electromagnetic waves in the laboratory and showed that his waves possessed all the properties of light waves. Since then the electromagnetic basis of light has never been questioned.

After light was recognized as an electromagnetic wave, the ether was retained as the medium through which these waves travel. Attempts were made to measure the speed of the earth through the ether, and they all met with failure. Various attempts were made to explain the failure, and finally, in 1905, Einstein presented his famous Special Theory of Relativity in which he proposed that there is no such thing as absolute motion such as is implied by talking about the speed of the earth through the ether, but that all motion is completely relative. The Relativity theory has been so successful that the ether, no longer considered necessary, now is no longer postulated. Electromagnetic waves in empty space are no more difficult to conceive than the original ether waves.

At the beginning of the 20th century a phenomenon was observed which the wave theory by itself appears to be unable to explain. When light falls on certain substances such as caesium metal, electrons are ejected, a phenomenon known as the photoelectric effect, and the energy of these electrons is determined, not by the intensity of the light as anticipated from the wave theory, but by the frequency or wave length of the light. In order to explain the photoelectric effect Einstein extended an idea originally proposed by Planck in discussing the emission of light. Einstein suggested that light energy is absorbed always in discrete amounts, or that light energy is divided into bundles called photons or quanta. For light of a given frequency these photons all have the same energy but the photons for high frequencies, or short wave lengths, have more energy than those of lower frequency, or longer wave length. This

idea has been very fruitful, and as a result the present conception of light is that it is dualistic in nature, possessing wave properties in so far as its propagation is concerned, and particle properties in its generation and absorption.

The important point is that physicists do use such interpretations, and that fruitful ideas can come from a "mixture" of the two. The same holds for the analyses of power of David Riesman and C. Wright Mills regarding social structure. They need not be seen as conflicting interpretations, but, rather as alternate interpretations applicable to different social structures.

## APPENDIX H

## APPENDIX H

Apart from the direct problem of the ontological status *per se* of atoms, electrons, and protons, there is a further problem in that these entities are often constructed so that they can be pictured, a tendency encouraged by the use of models in scientific theory. The implication is easily drawn that these are actual physical objects in nature. Thus there is a tendency to ascribe a direct, ontological status to such entities (solely) on the grounds that this is how they are imagined to exist. The following problems must be considered:

Are all theoretical entities necessarily grounded in nature as directly perceived, such that they are really only gradually introduced transformations of discovered structures? Do they always bear some residue of physical meaning? It could be argued that it is not legitimate to deny ontological status to theoretical entities because if they were not grounded to experience, to some degree, then they would be purely formal and not proper concepts for physical science as such. However, these theoretical entities do provide theoretical explanation in physical science, and so they probably do retain some physical meaning (ontological status), however indirect. Perhaps the problem is not in finding an object existing in nature to which the theoretical entity corresponds, but rather the degree to which such an entity expresses the physical nature as found in immediate experience.

Insofar as model construction involves idealization (the conversion of discovered structures into forms which are not, and often cannot be actual physical facts, e.g., "point locations," "instants of time," "point masses," "instantaneous velocities" and "accelerations," "point charge occupying zero volume," etc.), one is in danger of committing the fallacy of misplaced concreteness, i.e., of taking these idealized,



theoretical entities as the primary concrete realities under discussion. Is the danger then of carrying out scientific analysis directed toward the symbolization of discovered structures rather than to the discovered structures (i.e., physical facts, objects) themselves? For various reasons, one may end up primarily interested in discussing the equation rather than the physical facts which it represents (e.g., changing a proportionality into a mathematical equation involves the introduction of a constant, which may equal 1, or not, and may also possess certain physical dimensions required by the equation, thus raising the interesting problem of whether the constant refers to something in the physical facts, or merely represents a formal property of the equation). In any case, the point is that there has been a shift in the scientific analysis from discovered structures or physical facts towards the symbolization of these facts.

A good example of this problem is found in Coulomb's Law, which states that the force asserted by one point charge on another at rest is directly proportional to the product of the charges and inversely proportional to the square of the distance between them:

$$F \propto \frac{q_1 q_2}{d^2} \quad \text{or} \quad F = k \frac{q_1 q_2}{d^2}$$

where  $F$  = force,  $q_1, q_2$  = magnitude of the charge, and  $d$  = distance. Now, in the mks (meter-kilogram-second) system of units,

$$k = \frac{9 \times 10^9 \text{ newton} \cdot \text{m}^2}{\text{coulomb}^2}$$

and, for practical purposes, the allowed error, or  $\epsilon_0$  (permittivity of free space) is:

$$\epsilon_0 = \frac{1}{4 \pi k} = 8.85 \times 10^{-12} \frac{\text{coulombs}^2}{\text{nt} \cdot \text{m}^2}$$

However, in the more often used the cgs (centimeter-gram-second) system of units,

$$\epsilon_0 = 1.$$

The problem becomes one of deciding whether  $\epsilon_0$  bears a physical or only a purely formal meaning. Nevertheless, in a manipulation such as this, the attention of the investigator has shifted from the physical facts symbolized by the equation to the properties of the symbol or equation itself.

## APPENDIX I

## APPENDIX I

	GOOD	POSSIBLE	BAD
From Ia	to IIa, IIb, IVa, IVb, Va, Vb, VIa, VIIb.	IIIa, IIb, VIb, (1).	IIIa.
From Ib	to IIa, IIb, IVa, IVb, Va, Vb, VIb, VIIb.	IIb, VIa.	Ia, IIIa.
From Ila	to Ib(2), IVa(3), Va, Vb, VIa, VIb, VIIb(4)	IIb, IVb.	Ia(6), IIIa.
From Iib	to Ib, IIb, IVb, Va, VIa, VIIb.	IVa(1)(3), Vb, VIb.	Ia, Ib, Ila, IVa, IVb, Vb.
From IIIa	to VIa, VIb, VIIb.	IIb, Va(5)	IIa, VIIb.
From Iiib	to IIb, IVb, VIa.	Ia, Ib, IVa(2)Vb(5), VIb.	IIIa.
From IVa	to Ia, Ib, Ila, Va, Vb, VIIb.	IIb, (1), IIb(2), VIa, VIb.	IIIa.
From IVb	to Ia, IIb, Iiib, Va, Vb, VIb.	Ib, Ila, VIa(1), VIIb.	IIIa.
From Va	to Ia, Ib, Ila, IIb, IIIa, IVb, VIa, VIIb	IIb(1), IVa, VIb.	
From Vb	to Ia, Ib, Iiib, IVb, VIa, VIb, VIIb.	IIa, IIb, IIIa, IVa.	
From VIa	to IIa, IIb, Iiib, IVa, Va, Vb.	Ia, Ib, IIIa, IVb(1), VIIb.	
From VIIb	to IIa, IVb, Vb, VIIb.	Ia(1), Ib, IIb, IVa.	IIIa, Iiib, Va(6).
From VIIb	to Ia, Ib, IIb, VIa(8), VIb.	IIa(7), IVa(7), IVb, Va, Vb	IIIa, Iiib.

## NOTES

- (1) In these progressions the bass should leap an octave.
- (2) Not good when the fifths of both roots are in the highest part.
- (3) Only good when II is on an accented beat.
- (4) Only good in cadence of 2nd Species.
- (5) Only practicable if the root of III be in the highest part, and on an accented beat.
- (6) Bad in major key: possible in minor.
- (7), (8), see next page.

- (7) Can rarely be used effectively.
- (8) Not good in a minor key.

---

IIIb and VIb generally produce the best effect when the sixth is in the upper voice;

IIa, IIIa, IIIb cannot be used in a minor key.

## APPENDIX J

## APPENDIX J

The fugue may be divided into three rather arbitrary divisions: the exposition (the most consistently uniform section in any fugue), the middle section, and the close. The subject, in the tonic, always enters first, unaccompanied, in any voice, and is usually stated concisely. Consider the subject from the Fugue No. 2 (Well-tempered Clavier Volume I):



The subject is then stated in the dominant in another voice, this restatement being termed the answer. The subject could then be stated in still a third voice, again in the tonic, and so on down until all voices have entered, and always with the alternation of tonic in subject and dominant in answer. If the answer is an exact transposition, a "real" fugue obtains; if not, a "tonal" fugue, such as the one referred to above (C-minor) results. One additional important term is necessary. The counterpoint that appears consistently with the subject or answer is called the countersubject.

In the middle section, the theme is stated recurrently, separated by portions in which the theme is not stated intact, these sections being termed episodes. (Appearances of the subject separated by material of an episodic nature, but shorter, is termed a codetta.) The episodes are generally made up of material from the subject, countersubject, and free voice of the exposition, but may be treated in different ways by, for example, inversion, key-change and voice-change, or combinations of these.

There is no restriction on the number of episodes, and they may also be used to effect modulations, as in the case of the first middle entry of the same fugue.

The close is often marked by the last episode modulating into the tonic. The fugue generally finishes off with a special section called the coda.

Many additional devices such as stretto, pedal, *tierce de picardie*, double subject, counter-exposition, and redundant entry, though by no means common to all fugues, have been omitted. However, together with the rules of counterpoint already discussed one can see the notion of "conceptual framework" as it applies to the composer, as well as the notions of explanation and prediction for both the composer and listener in respect to the reasons why baroque music possesses its distinctive sound.



## APPENDIX K

## APPENDIX K

"Sonata first-movement form" unfortunately, does not designate what the name implies, i.e., the form of the sonata, but the form used frequently for single movements of the sonata (symphony, quartet, etc.). Since this form is practically always used for the first movement of a sonata, it is also designated as first-movement form. This term, however, is also misleading since the same form is frequently employed also for the slow and for the final movements of sonatas. Both terms designate a form which is of fundamental importance in music from Haydn and Mozart to the contemporary composers of sonatas or symphonies although, after 1900, it was so freely treated that sometimes only traces of it are discernible. It is probably correct to say that eighty per cent of all the movements found in the present day are written in sonata-form, strictly or freely applied.

A movement written in sonata-form falls into three sections, called exposition, development, and recapitulation . . . the last usually followed by a shorter or longer coda.

Sonata-form may be diagrammed thus:

	<u>Exposition</u>	<u>Development</u>	<u>Recapitulation</u>	<u>Coda</u>
Theme:	I II (III)		I II (III)	
Key scheme:	Tonic to Dominant	Modulatory	Tonic (remaining)	

The exposition contains a number of themes and connecting passages (bridge passages) which fall into two groups, first and second group or, as they are also called, first and second themes. There is usually a noticeable difference in character between the first and the second themes, the former being, for example, dramatic, the latter lyrical. Furthermore, the second theme is in another key, normally in the key of the dominant if the tonic is major, and in the relative key if the tonic is minor. Towards the end of the second group one frequently finds a "closing theme" which stands out for its individual character.

The development is the central section of the movement, on account of its position as well as its character. The style and treatment here differ radically from that in the exposition. A great number of devices and procedures are used to produce that special character of "development," "dynamic tension," "increased temperature," "fighting forces," etc., which is proper to this section.

Two most important means of development are:

- (a) Melodic segmentation
- (b) Rapid harmonic modulation (change of key)

Other devices used are:

- (c) Contrapuntal imitation of melodic motives  
(i.e. "fugal style")
- (d) Contrapuntal combination of different motives
- (e) Use of themes or motives in inversion or  
diminution

For example, the relationship of the opening bars of the development can be traced to the first theme of the exposition. There are no set rules as to any of the detail of procedure. In the development section more than anywhere else the composer is free to use his ingenuity in forming a dynamic body from the building material at his disposal.

The recapitulation normally contains all the material of the exposition, although usually with certain modifications, particularly in the bridge passages. One modification is obligatory, namely, that which makes the second theme appear in the tonic (not, as formerly, in the dominant) so that the whole movement comes to a close in the tonic.

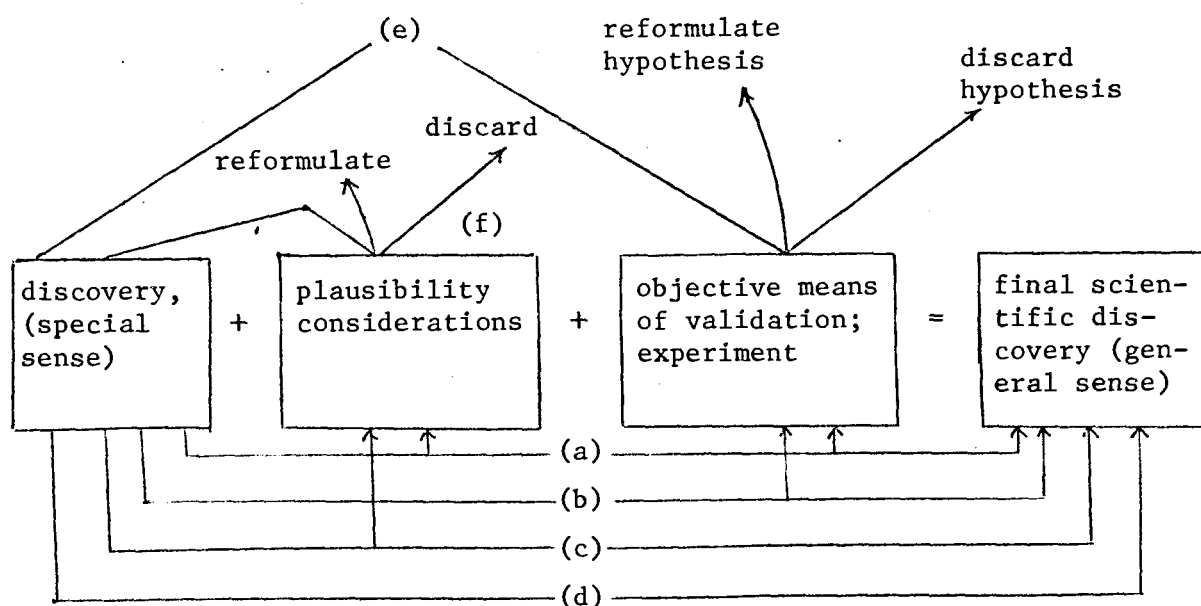
The coda which, in many compositions, is only a closing sentence of moderate length; in others it assumes considerable proportions and sometimes spreads out into another development section.

There are other important forms, such as theme and variations, rondo, sonata-rondo, etc. but they will not be taken up in this thesis. The above account of the most important classical form will suffice.

## APPENDIX L

## APPENDIX L

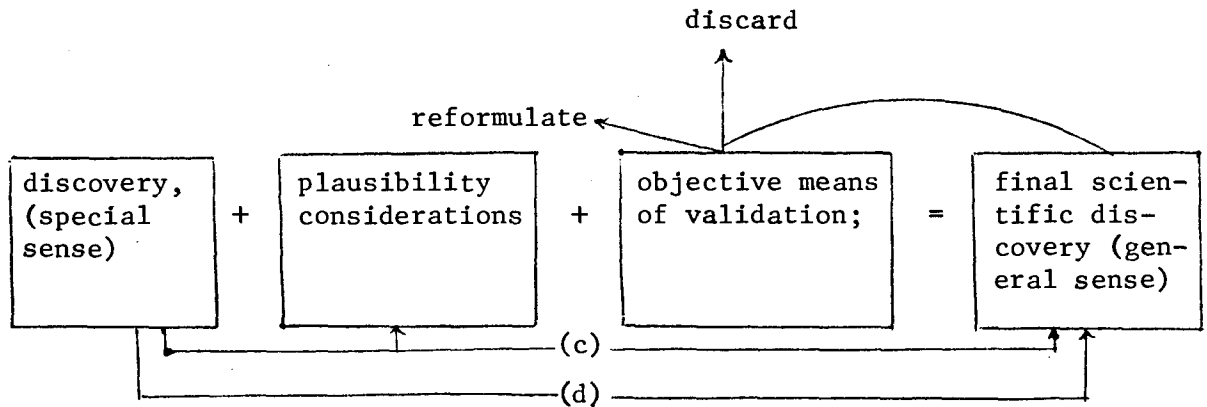
It is also important to recognize that the model presented in Chapter Five is simplified. The following models, showing alternate routes, are probably more accurate.



SD Model B

Route (a), in which each stage is taken in turn, may, in fact, not obtain. The researcher may very well formulate the final law or theory in one leap ("intuitive leap") as shown in route (d). It is also possible that testing may occur without any detailed considerations of plausibility, as in route (b). This may be especially true if one is analyzing a revolutionary discovery, for, if considerations of plausibility are made within a theoretical framework (as at (c)), then it is difficult to

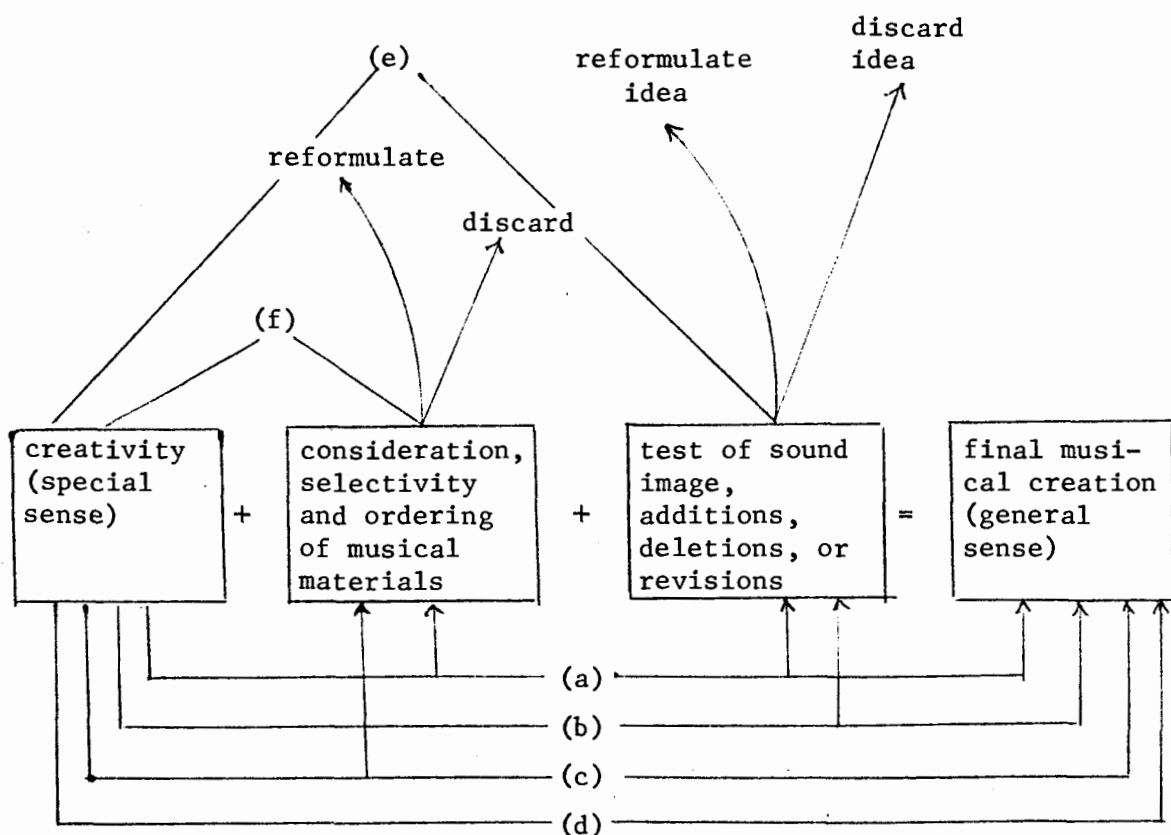
accommodate the notion of overthrowing that theoretical framework. It is also possible that, in the theoretical formulation of a law, the testing may have been by-passed. However, as this thesis has so strongly argued, the testing is the crucial stage. Routes (c) and (d), then, must be revised as follows:



*SD Model C*

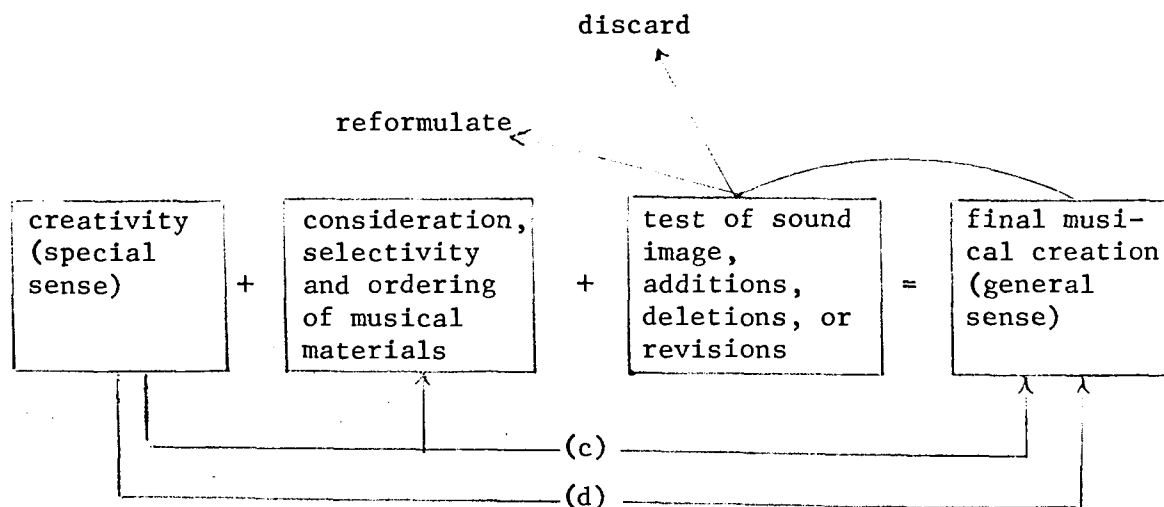
In each of these models (*SD Models B and C*) the arrows below represent a successful outcome while the arrows above represent, after testing unsuccessful outcomes which result in the discarding or the reformulation of the original hypothesis.

Similar revisions are required for musical creativity. The following model may more accurately represent the development of a musical idea.



MC Model B

Route (a) is again the most systematic, and probably more representative of student composers than of professionals. In this process, testing is not quite as crucial as in discovery, such that routes (c) and (d) probably do not require revision as in *SD Model C*; however, the following model, analogous to *SD Model C*, may very well occur, for it seems that very few composers do not make at least some minor revisions before presenting the composition for performance.



MC Model C

Again, the routes below the boxes represent successful outcomes; those above represent the progress of musical ideas that are not fruitful, or require modification.