The Role of Inversion in the Genesis, Development and the Structure of Scientific Knowledge

A Thesis Submitted
In Partial Fulfilment of the Requirements
for the Degree of

Doctor of Philosophy

by

Gadiraju Nagarjuna

to the

DEPARTMENT OF HUMANITIES AND SOCIAL SCIENCES

INDIAN INSTITUTE OF TECHNOLOGY, KANPUR

September 1994

CERTIFICATE

It is certified that the work contained in the thesis titled *The Role of Inversion in the Genesis, Development and the Structure of Scientific Knowledge*, by **Gadiraju Nagarjuna** has been carried out under my supervision and that this work has not been submitted elsewhere for a degree.

Prof. Mohini Mullick
Professor
Department of Humanities and Social Sciences
Indian Institute of Technology, Kanpur.

September, 1994

SYNOPSIS

The main thrust of the argument of this thesis is to show the *possibility of articulating* a method of construction or of synthesis—as against the most common method of analysis or division—which has always been (so we shall argue) a necessary component of scientific theorization. This method will be shown to be based on a fundamental *synthetic* logical relation of thought, that we shall call *inversion*—to be understood as a species of logical opposition, and as one of the basic monadic logical operators. Thus the major objective of this thesis is to

This thesis can be viewed as a response to Larry Laudan's challenge, which is based on the claim that "the case has yet to be made that the rules governing the techniques whereby theories are invented (if any such rules there be) are the sorts of things that philosophers should claim any interest in or competence at." The challenge itself would be to show that the logic of discovery (if at all formulatable) performs the epistemological role of the justification of scientific theories. We propose to meet this challenge head on: a) by suggesting precisely how such a logic would be formulated; b) by demonstrating its epistemological relevance (in the context of justification) and c) by showing that a) and b) can be carried out without sacrificing the fallibilist view of scientific knowledge.

OBJECTIVES: We have set three successive objectives: one general, one specific, and one sub-specific, each one related to the other in that very order.

(A) The general objective is to indicate the clear possibility of *renovating* the traditional analytico-synthetic epistemology. By realizing this objective, we attempt to widen the scope of scientific reason or rationality, which for some time now has perniciously been dominated by pure analytic reason alone. In order to achieve this end we need to show specifically that there exists the possibility of articulating a synthetic (constructive) logic/reason, which has been considered by most mainstream thinkers either as not articulatable, or simply non-existent.

(B) The second (specific) task is to respond to the challenge of Larry Laudan by demonstrating the possibility of an epistemologically significant generativism. In this context we will argue that this generativism, which is our suggested alternative, and the simplified structuralist and semantic view of scientific theories, mutually reinforce each other to form a single coherent foundation for the renovated analytico-synthetic methodological framework.

(C) The third (sub-specific) objective, accordingly, is to show the possibility of articulating a synthetic logic that could guide us in understanding the process of theorization. This is realized by proposing the foundations for developing a *logic of inversion*, which represents the pattern of synthetic reason in the process of constructing scientific definitions.

STRUCTURE OF THE ARGUMENT: The dissertation is divided into three parts. In the Part-I we present a historical introduction to the problem. Parts II and III, are designed to meet the specific objectives (B) and (C). Finally we will attend to the general objective (A) towards the end of Part-III—moving from the specific to the general. In the first part we present a *quasi-historico-philosophical narrative*, which substantiates the observation that ever since its inception traditional epistemology, despite being analytico-synthetic, lacked certain necessary elements of analysis for purposes of characterizing some, especially the constructive, aspects of scientific knowledge—giving rise to pure consequentialism. A thematic study of the origin of epistemology and method in the hands of the ancient trio Socrates, Plato and Aristotle is presented. This is then contrasted with the development of the mathematico-experimental method, which is necessitated by the needs of new objects of scientific knowledge, by the modern trio Galileo, Descartes and Newton. The factors that eventually gave rise to the hypothetico-deductive methodology and its comrade-in-arms falsificationism, are discussed. It is observed that accounting for the highly theoretical and mathematical nature of the modern scientific knowledge eventually became one of the central problems of epistemology. It is argued that the lack of understanding of how we arrive at the highly theoretical and mathematical aspects of scientific knowledge has given rise to pure consequentialism.

In the second part we critically review the arguments against a discourse of discovery in epistemology, which culminated in Laudan's challenge (referred to above). The attempts made by Thomas Nickles and others to defend the discovery program are critically assessed. It is observed that Nickles' arguments to save the program, despite being the most comprehensive of those available, are deficient, because of a lack of an alternative generativist framework. We then work out a generativist framework based on a distinction between the epistemic values attributable to mathematical structures and models, such as closure, invariance, and symmetry in the *context of generation*, on the one hand, and those of truth and falsity attributable to scientific assertions in the *context of application* on the other. In this context we propose a simplification of the non-statement view of W. Stegmüller and the semantic approach defended by F. Suppe, and Bas van Fraassen.

In the rest of Part-II, we present the essentials of the idea of inversion, which is the heart of the matter of the thesis. The presentation starts by making certain conceptual distinctions such as between: negation and inversion; one-over-many and one-to-one relations; predicate and proportion based identities; definite and inverse definite descriptions; entities and dimensions; and between the taxonomic and inverse systematization. These distinctions are made in order to make the foundation of the framework explicit, enabling the reader to anticipate possible developments.

We then outline the development of number theory as a known example where inverse reasoning has played a necessary role. This is followed by suggesting a similar pattern of analysis for the structure and semantics of definitions of dimensional elements (measurable parameters)—extending the idea from the known case of number theory to the unknown cases of physical theories. These definitions, it is suggested, are to be interpreted as 'complex predicates' that describe 'physical systems'. They may be viewed as semantic structures or unsaturated propositions, which are not by themselves either true or false, but will have epistemically desirable values, such as closure, invariance, and symmetry. These definitions in their own right and irrespective of their applicability, are justifiable pieces of knowledge, not only because of the *values* they possess but also because they developed from *terra cognita*. It is then shown that the two views, the generativism of scientific definitions and the simplified semantic view of scientific theories, mutually reinforce each other to form a single coherent foundation for the proposed analytico-synthetic framework. It is further argued that the various meta-theoretical predicates, such as closure, invariance, relativity and symmetry, are based on inversion, in the sense that, inversion is a necessary (though not a sufficient) condition for their emergence. Finally, it is also argued that inversion makes measurement and mathematization possible, explaining the epistemic transformation from qualitative to quantitative science. In the course of the discussion the affinities of the proposed framework with the views of H. Weyl, E. Wigner, Bas van Fraassen, F. Suppe, W. Stegmüller, J. Piaget etc., among others, are presented.

In Part-III, we substantiate the major claims of the thesis in the form of detailed case studies. We start with an account of how proto-scientific knowledge emerged out of a 'soup' of opposites prevalent in ancient Greece. The metaphysical views from Thales to Plato suffer from being highly global (non-paradigmatic). It is observed that the shift from global world views to local (paradigmatic) problem oriented science took place with Aristotle. A study of Aristotle's *Physics* and *De Caelo*, shows that most questions later dealt with by Galileo in the 17th century were posed by Aristotle, though his solutions were all 'wrong'. [Aristotle]'s *Problems of Mechanics*, was the real turning-point towards mathematical physics addressing certain local problems in statics, such as that of the lever, balance, pulleys, and certain other geometrical problems. It was followed by Archimedes' contributions to statics and hydrostatics. We have sought to demonstrate that in these developments inversion played a necessary role, in the sense that *without this seminal notion the genesis of scientific knowledge would have been impossible.*

We then present a detailed reconstruction of Galileo's discoveries regarding the problem of motion, based on his works *De Motu* and *The Two New Sciences*, which show beyond doubt that the development of modern physics has been made possible by applying the known knowledge of the balance—a representative example of a theoretical structure based on inverse reason—to the unknown case of motion. Galileo's study shows how he successively reduced the motion of bodies in natural fall, projectile motion, on inclined planes, and the motion of the pendulum, to that of the balance. We trace the steps by which Galileo gradually 'bends' his attention from the vertical component to the horizontal component, employing the above mentioned analogies, ultimately leading to his greatest discovery, the relativity of motion and the composition of the two components, among other discoveries such as the law of free fall and the law of inertia. This case study also demonstrates how the use of inverse reasoning by Galileo can explain the conceptual change from Aristotelian to the modern physics. Most concepts of modern dynamics, like mass, force, momentum, energy, etc., show their presence in this context, demonstrating the *discovery potential of inversion.*

Another case study is presented to illustrate how reversibility, invertibility of chemical processes, have played a necessary role in the discovery of chemical elements. This study extends from the Stahlians to Lavoisier, explaining the role of inverse reason in the overthrow of phlogiston chemistry.

The ideas of equilibrium and homeostasis, which are also based on inversely related processes, have been shown to have played a significant role in different branches, such as population genetics, general physiological models in biology, further corroborating the role of inversion.

Having demonstrated the omnipresence of inverse thinking in various contexts, we conclude by proposing that inversion should undoubtedly be an additional and necessary parameter for a philosophical study of scientific knowledge in its genesis, development and its structure. We end the dissertation with an outline of the analytico-synthetic epistemological framework to replace pure consequentialist epistemology.

PREFACE

This thesis is based on the following historico-philosophical observation regarding the failure of philosophers of science in understanding the methods of generating scientific knowledge. Though traditionally philosophers have addressed the problem of how we arrive at scientific knowledge—which for them is infallible—they could not achieve anything that can be called success. Towards the end of the last century a significant *turn* took place in epistemology and philosophy of science leading to fallibilism. Emergence of fallibilism has led to the abandonment of one of the fundamental problems of a traditional epistemologist, which is the problem of how we arrive at (true) scientific knowledge. Though we agree that we are in possession of no method that can generate *true* scientific knowledge, we think that we can devise methods that can generate *meaningful scientific concepts*. Therefore it occurred to us that there is no need for a complete abandonment of traditional generativist epistemology. The initial problem of the thesis was then formulated to explore the possibilities of renovating generativism. Thus when the work began, the problem chosen was to argue out a case for a version of analytico-synthetic generativist methodology.

The history of philosophy has strong evidence against the role of induction in the generation of scientific concepts: for it was realized that scientific concepts are much more than what can be obtained by means of induction. Convinced that induction cannot generate scientific concepts, we went on to explore the possibility of other methods such as the method of taxonomy, which surely has played a decisive role in the discovery of scientific kinds (natural kinds). If the idea of *inversion* did not eventually occur to us, the thesis would have been on "The Role of Taxonomy in the Discovery of Natural Kinds: Towards an Analytico-Synthetic Approach".

Eventually we realized that there are two main aspects to the problem of generating scientific knowledge. One of them is the problem of devising the methods that lead to proliferation of scientific kinds, and the other is the problem of devising methods that lead to unification and abstraction of theoretical (scientific) concepts. The problems that we have encountered in articulating this distinction are responsible for the eventual emergence of the idea of inversion. The title of the thesis at that point of time would have been "The Two Faces of Science: Inversion and Taxonomy". Inversion is intended to be the method of dealing with *invariance*, and taxonomy with *variety*. But the vitality and the force of the new idea naturally took all our attention. The present thesis is therefore about only one of the aspects

of scientific knowledge; the other, where taxonomy plays the central role, stands postponed.

The development of this thesis involved many alterations in both form and content. In this process I never heard a word of discouragement from my thesis supervisor Prof. Mohini Mullick. I wish to express my gratitude and indebtedness for all the help and training I received from her. I wish I had attended more conscientiously to all the suggestions and comments given by her. I wish to express my gratitude and indebtedness to Prof. R.S. Mishra, who supervised my work when Prof. Mullick was away on sabbatical. I express my gratitude to Professors S.A. Shaida and Bijoy Boruah, who helped me in various ways apart from being my teachers. I have benefited enormously from the discussions with Dr. N. Mukherji during his presence in this department as a Visiting Professor. I wish to thank Professors. B.N. Patnaik, M.B. Meena, Kunal Ghosh, C.V.R. Murthy, Pravir Dutt, Deshdeep Sahdev, for their friendly and informal guidance in matters both academic and non-academic. I also wish to acknowledge Professors L. Krishnan, N.K. Sharma, M.A. Ansari, R.M. Rao, Suhita Chopra, who helped me in various ways. I wish to thank Professors Kulkarni and Haribabu who helped me in procuring reference material from the library of Central University, Hyderabad.

I wish to thank my senior colleagues, Kanoujia, Neezar, Rais, Kanika, Uma, and Achla who have shared with me their experience and helped me in various ways. I have to thank the help I had received from Anupam, Shubra, Sharad, Dheeraj, Sangeeta, Augustine, Sarabjit, Nachiketa, Gobind, and Anurag. I worked mostly in the company of my colleagues Patil, Sanil and Neeraj, and my very special thanks to them. I have also discussed my problems with Siva, who corrected my understanding of mathematical notions.

In the process of typing, editing and typesetting the thesis, I took the help of a number of friends. Special thanks to Deepak Murthy who solved almost all my problems in this connection. I must also thank Jitendra, Himanshu, and Venugopal, from whom I learned computer fixes specially during the final days of typing the thesis.

I can never forget the time I spent with the members of the 'Sunday Meetings' with P.R.K., A.P.S., Sule, Kalyan, Sanil, Venkatesh, Ravindra, Srikanth, and Patil. It was the prolonged discussions with them on matters related and unrelated to the thesis work, that have broadened my understanding.

It is because of the love, faith, encouragement and support of my parents, in-laws, all my brothers and sisters, my wife and daughter that I could live and work comfortably. Knowing fully well that this thesis is nothing in return to what they gave me, I dedicate this thesis to that relationship where the 'mania' of balancing ceases to have any meaning.

Nagarjuna

Contents

Part I

Historical Introduction

Chapter 1

Origin of Epistemology and Scientific Method

Two fundamental questions concerned philosophers ever since the inception of epistemology. (a) How do we arrive at knowledge? And (b) How do we know that the arrived knowledge is true? These questions can also be put in a different manner: (a') What factors make the genesis of knowledge possible? (b') What factors make knowledge true? Retrospectively we may say, following Reichenbach's famous distinction, that the two questions relate to the context of discovery and the context of justification respectively.[1] Any discussion regarding the context of genesis of scientific knowledge in the contemporary discourse is questioned on the ground that epistemology has nothing to do with the questions of genesis or origin of ideas. Or even if the questions are rated legitimate for epistemology, no formal pattern of genesis is believed to be possible.

However, as we have just stated, such has not been the case since the inception of epistemology. Philosophers have attempted to answer the question of origin of knowledge, ever since knowledge making or seeking has been realized as a component of human nature, as if it is *natural* for them to do so. Why did this become a natural question to start with? And why is this not so with us in this century, when it is no longer considered that a theory of discovery/invention would naturally form a part of epistemology? Today some philosophers, who find it interesting to address this question, have been addressing this question only by way of defense, with hesitation and with little confidence. So they have to attend in the first place to a meta-philosophical problem of legitimizing the problem, and then attend to the relatively first-order philosophical problem of searching for an order in the context of

[1] The dichotomy has been questioned by some philosophers on various grounds. These details will be discussed in Chapter 4.

discovery.

For traditional epistemology the question of finding the method/s of arriving at knowledge has been a necessary problem, because justification of knowledge was thought to be partly, or completely dependent on the basis of generation, i.e., the problem of genesis and justification were not thought to be independent. On the other hand, for the majority of the contemporary epistemologists the epistemological problem consists in finding methods, if any, of justifying knowledge by deducing specific truthful consequences from general abstract laws or theories. We will call, following the terminology of Larry Laudan (1980), the former position *generativism*, and the latter *consequentialism*.

How did this philosophical transformation from generativism to consequentialism take place? To give a detailed and critical explanation to this interesting transformation in epistemology would in itself require a separate work. We are not attempting to provide such an account in this work. However, some explanatory observations pertaining to the transformation will be presented which would form a background to this work.

This part is written with the view that the problem of this work can be best stated if we understand the situation or the historical circumstance of its origin, i.e. the context of the genesis of epistemology and scientific method, for it is our diagnosis that mainstream epistemology ever since its inception lacked certain necessary elements of analysis for characterizing scientific knowledge. We assume that the problems of epistemology can be better understood by knowing some of the necessary conditions that made a theory of knowledge possible. Especially, from the point of view of the present work, it is necessary to seek answers to these questions: Why, in the first place, did the early philosophers felt that there should be a method of arriving at knowledge? Why did epistemology, as a theory of knowledge, come into being? With these questions in mind if we look back at the history of Greek thought, we may possibly come to know the genealogy of the problem at hand, the pitfalls of the various answers given, and the direction in which to seek the answer today.

We will be presenting this as an account of *the genesis and dynamics of thematic-pairs*. The following material, let us make it clear, may not contain any new historical 'facts' of philosophy that most of us are unaware of. What we have done is to realign the 'substance' in a new form, which being a 'rational reconstruction', is intended to form an argument in itself. Though the thesis is an argument in favor of the analytico-synthetic epistemology, it is not however written in an argumentative style. Rather, we have adopted a different kind of style which may be described as that of a *quasi-historico-philosophical narrative*.

It is quasi-historical because the character of our research is not similar to that of a historian. And at the same time we cannot say that the work has nothing to do with the history of ideas. It is quasi-philosophical because it is not presented in the form of a rigorous argument, rather it is presented in the form of an *extended argument with internal coherence*, which emanates from the alternative framework that we have in mind. This framework will be identified by comparative characterization, and in this sense we frequently attend to various epistemologies by comparing them with ours. We have called it a narrative because the objective is to tell of or explicate, a possible alternate framework, and reformulate some of the problems of epistemology and philosophy of science from this new point of view. It is also a narrative because there is reconstruction involved in our attempt to retell the otherwise familiar material.

The method of organizing that we have adopted consists basically in the manner in which the presuppositions of a given thought are analyzed into antinomies or thematic-pairs. Some examples of such thematic-pairs are: reality and appearance, variable and invariable, Being and Becoming, simple and complex, universals and particulars, genus and species, analysis and synthesis etc. We will see that these thematic-pairs by their intrinsic anchorage in a tradition—being presuppository in nature—function as regulatory constraints, controlling the thinking process of the tradition. Thus they not only make some line of thinking possible, but at the same time they put a limit on that thinking, on that very basis. It is due to the possibility of knowing both the necessary conditions as well as limitations of epistemology, that we intend to narrate the story of early stages of epistemology in a form based on thematic-pairs and their dynamics. The semantics of theoretical order, we think, can best be understood by this method of thematic analysis based on opposites of various kinds.

1.1 The Genesis of Universals and Epistemology

If there was ever a period in the history of ideas that was fruitful in terms of variety and creativity, it certainly was the period from the 7th century BC to the 4th century BC. It was during this period that a variety of early conceptions of nature were proposed starting with Thales and ending with Plato. The conceptions of nature will be presented separately in Chapter-7, because the neglected thematic-pairs based on inversion have been playing a central role in their development. Since our objective is also to show the inherent limitation of mainstream epistemology, by way of looking at the context of its genesis, for the present we shall limit our presentation to the developments pertaining to early conceptions about knowledge.

Earlier to the Sophists, who went on to develop conceptions about human nature as well, there are certain conceptions 'about' knowledge, which are based on the thematic-pair, *appearance and reality*, an early precursor of another modern thematic-pair, *primary and secondary qualities*. We may recall that the early theoreticians of nature, often described as the physiologues, presumed that the apparent world is confusing, complex, everchanging (in constant flux), and so on, and that the real world is ordered, simple, and has a permanent form. The thematic-pairs Becoming and Being are thus connected to the apparent and the real. This is possibly the first attempt to understand the underlying reality in a manner which is different from the mythological and theological modes of knowing.

This distinction between the 'physiological' mode on the one hand and the mythological and theological modes on the other is not intended to show that the former is superior to the latter modes of knowing. With the assumption that mainstream epistemology has not much to offer on mythological and theological modes of knowing, and because mainstream epistemology at least claims to be about 'scientific knowledge', we concentrate only on the physiological mode to begin with. We intend to demonstrate that mainstream epistemology could not give a satisfactory and complete account of scientific knowledge because it has not been able to delineate one of the essential and basic components of scientific thinking. Another of our assumptions is that we consider the physiological mode as a precursor to the scientific mode of knowing.

That there is something beyond what is given to us in experience has been generally explained on the basis that a large number of events that we experience have no *visible* causes, given the assumption that we have a natural tendency to search for causes. In order to bring in security, closure and symmetry, the human mind has created many *invisible* 'theoretical' (it may be appropriate to say mythological or theological) entities, including ghosts, demons, gods, etc. The invisible has somehow taken the 'primary' level of reality, while the visible has become the 'secondary' level of appearance. These presuppository themes such as hidden and visible, apparent and real, primary and secondary, appear to have animated one of the basic modes of knowing, namely the explanatory mode, which is at some level of generality common to all the modes of knowing. Thus some of the first thematic-pairs that started moulding our thinking can be stated to be the visible and the invisible, and the apparent and the real.

In the first phase of the genesis of scientific knowledge, characterized by a manner of theorizing at a *global* level, various proposals have been made regarding what is that *underlying invisible reality*. We can easily see that the expression 'underlying invisible reality' is a composition of the thematic-pairs mentioned above. As mentioned already except towards the end of this phase, proper epistemological questions were not a central concern. Thematic analysis of 'theories' about nature are presented in Chapter 7, where the role of inversion in the genesis of scientific knowledge is elaborated. In what follows we shall begin narrating the crucial moments in the genesis of epistemology.

It is generally noted by historians that Greek philosophy begins with an inquiry into the objective world and then gradually turns its attention to man himself, leading to the study of the human mind, human conduct, logic, knowledge, ethics, psychology, politics, and poetics. Such a turn took place due to the Sophists, who shifted the attention of thinkers from the problems of nature to the problems of human knowledge and conduct. Before them, there had not been any attempt to question the possibility of knowledge. It is assumed that men can know and rightfully 'theorize' about the world.

An antithesis to this trend was provided by the Sophists, who thought that cosmological and metaphysical speculations are futile. Having seen the diversity of opinions found among the Greek naturalists, they concluded that this was due to the limitations of human thought and abilities. According to the Sophists our opinions about nature would necessarily be diverse, paradoxical, and without any interpersonal agreement. The picture given by them appears to be true because, different thinkers chose different 'things' as the underlying invisible reality. As is well known, while some considered water as the underlying principle, some considered air, and some others fire. A few others 'created' highly abstract things like *apeiron*, undifferentiated substance, and proposed a mechanism of creating the rest of the substances from them. Which among these 'theories' is the best? It is almost impossible to answer this question because each of them is internally coherent, and ultimately it is just a matter of one's choice. The theories are proposed at such a highly global level that it is difficult to judge or verify them. Karl Popper would describe these theories as unfalsifiable, therefore nonscientific, though meaningful.[2] The Sophist's criticism, however, was not based on this modern notion of falsifiability, but rather on the impossibility of solving the riddle of the universe. It is impossible because knowledge depends upon the knower. What appears to be true for one need not be true for the other. There is no objective truth, only subjective truth. They preached that "man, collectively, is the new corporate entity which replaces the cosmos; it provides its own measures."[3] Thus, man is the measure of all things—*Homo mensura*. They repudiated the earlier thinkers in favor of common sense judgments of the individual. This, to our understanding, is the initial problem of knowledge, challenging the

[2]Cf. Popper 1963, *Conjectures and Refutations: The Growth of Scientific Knowledge*, Ch-11, p. 253ff.
[3]Giorgio de Santillana 1961, *The Origins of Scientific Thought: From Anaximander to Proclus*, p. 172.

efforts of knowledge seekers in an uncritical manner.

We think that this challenge remains only partially resolved, from the philosophical point of view, by Socrates, Plato and Aristotle. However partial their solution may be, from the point of view of the positive contributions they have made towards the birth of scientific knowledge, their success consists in creating the initial *categories* into which a precursor of scientific knowledge in the form of systematic knowledge started pouring in. We will critically elaborate the attempts made by these great thinkers starting with Socrates.

The genesis of mainstream epistemology can be narrated by first looking at the pattern of the Socratic method used to generate knowledge of the universals. The Socratic method, which consists in asking questions in feigned ignorance and refuting all answers is in fact identical with the Sophistic method of argument intended to disclose contradictions in the opponent's statements or views. But in contrast with the Sophists who seek to prove that knowledge is impossible in principle, Socrates only comes out against false knowledge. His goal is to expose false claims to wisdom and lay bare human ignorance.

It is well known that Socrates' attention was not directed towards knowing the physical world, because he thought that our abilities to know it are limited. The subject of his inquiry is human nature. A host of questions raised by Socrates in the early Dialogues of Plato, which are about virtue, courage, temperance, etc., give us this indication. However these inquiries have an inherent pattern that was explicitly identified and defined by Plato in his later Dialogues, i.e., from *Theaetetus* onwards.

From a study of the early Dialogues of Plato, we now present an account of how the discovery of the thematic-pair universals and particulars took place.[4]

The discussion will be conducted, as mentioned above, as a part of the account of his method, usually called the Socratic method, for the distinction between universals and particulars become operative in the method. The central concerns of a philosopher can best be understood by examining the questions raised by them. Here we make use of a study by Santas on the type of questions raised in Plato's Dialogues, which in turn is based on the study by Belnap on the logic of questions. It has been demonstrated by Santas that in Plato's Dialogues mainly two kinds of questions are asked, namely, *which-questions* and *whether-questions*.[5]

How is the classification of questions relevant for our purpose? The answer is that

[4]It is assumed that the early Dialogues of Plato describes the Socratic position and the later ones his own. Thus when we use the expression 'Socratic method' it is the method enunciated in his early Dialogues apparently practiced by Socrates, to which we refer. The expression 'Plato's dialectic' refers to the method formulated in his later Dialogues.

[5]Santas 1979, *Socrates: Philosophy in Plato's Early Dialogues* pp. 72-73.

it enables us to understand the underlying expectations, and motivations of Socrates while asking questions. These expectations clearly reveal that he distinguished between universals and particulars. The following account on which-questions indicates how one can 'define' or fix a universal, and the subsequent account on whether-questions explicates what is peculiar to the Socratic method, which aims at an ultimately clearer understanding of universals through the imperfect knowledge of particulars.

Which-questions are non-dialectical and whether-questions are dialectical. This division is made on the basis of whether the alternatives provided by the question are infinite or finite and also on the basis of the manner of presenting the alternatives. It must immediately be made clear that this division is not merely made on the basis of which questioning expression occurs in the question, but, as we shall see, solely on the basis of the alternatives suggested by the question—whether the alternatives are infinite or finite. This will become clearer from the examples given below. Examples of whether-questions will be given later in an extended discussion on the Socratic method (§1.2 page 18), while examples of which-questions shall be discussed here.

Which-questions allow great latitude to the respondent, therefore these are also called 'infinite' questions.[6] According to Belnap which-questions and whether-questions are differentiated on the basis of the *manner* of presenting the alternatives. In a whether-question the alternatives are explicitly mentioned, while in a which-question the alternatives are described by reference to some condition and an appropriate set of names or terms. To arrive at any answer the conditions are to be provided, otherwise it would not be clear to the respondent from what kind or range of alternatives to choose.

In the Dialogues of Plato which-questions, in relation to whether-questions are not numerous. But it is with a which-question that each of his Dialogue is initiated. The most famous kind of questions raised by Socrates are which-questions. For example, What is knowledge? (*Theaetetus*) What is virtue? (*Meno*) What is courage? (*Laches*) What is temperance? (*Charmides*) What is justice? (*The Republic*) What is beauty? (*Hippias Major*).

These questions are of the form 'What is X?'. We could see that no further conditions are given in the question, unlike in a typical example of a which-question 'What is the smallest prime number greater than 45?', where 'greater than 45' provides a condition. They have only a main term. But as the Dialogue proceeds some conditions are introduced subsequently by Socrates, in order to delimit the scope of the question and also to clear the misunderstandings of the respondents.

[6] Which-questions may also be called 'what-questions', however we shall use the term 'which-question' since it is already in use. Cf. Belnap 1963, p. 13, and pp. 37–8.

An analysis of the which-question and its complete form will throw great light on the objective of the Dialogues, which is to arrive at a definition of an idea. A preliminary characterization of universals can be obtained from a longer version of a which-question, which looks like a typical which-question with generalized conditions. According to Santas the longer version of the question can be stated as follows:

> What is the kind (characteristic, property) which (a) is the same (common) in all F things, and (b) is that by reason of which all F things are F, and (c) is that by which F things do not differ, and (d) is that which in all F things one calls 'the F'?[7]

Here (a) and (c) characterize F on the basis of similarity and difference, while (b) and (d) give reasons for calling some thing/s F. The main part of each Dialogue starts with a which-question, which defines the scope of the question, and since each Dialogue generally addresses itself entirely to that very question it defines also the scope of the entire Dialogue. It is not the terms, like 'knowledge', 'beauty', or 'virtue', that appear in the questions which do this, but the conditions (a), (b), (c), and (d) of the longer version of the question form quoted above. These conditions from (a) to (d) clearly tell us what Socrates is looking for. These conditions are just those criteria which define the *universals*. In the language of Plato they define the *Forms*, whereas *particulars* are represented in those conditions as 'F things'. If the form of the question is any indication to the thematic-pair guiding the Socratic method in the Dialogues, it is clearly the thematic-pair universal and particular.

This pair further presupposes certain familiar ideas of similarity and difference, one and many. Things around us have certain similar qualities, and *one* quality can characterize *many* things. The *one* is an instance of a universal and the *many* are called particulars. This pair thus presupposes the ideas of similarity and difference. However, certain other significant aspects are involved, but are not clear from the question form explicated above. One of them is the involvement of a logical operation called *negation*. The question form explicated above however does not capture this important logical relation. Through the mention of difference, as it occurs in one of the conditions above, one might say, the relation is captured. However picking out examples of a type is most often not a trivial job. Therefore we think it appropriate to further explicate the conditions (a) to (d) as follows: *What is the kind (characteristic, property) which (a) is the same (common) in all F things and not the same in non-F things, and (b) is that by reason of which all F things are F, and all non-F things are non-F, and (c) is that by which F things do not differ, and is that by which F-things*

[7]Santas 1979, *op.cit.*, p. 83.

and non-F things differ, and (d) is that which in all F things, but not in any non-F thing, one calls 'the F'?

This brings out the essential logical relation that is involved in relating a set of tokens to a type, for similarity cannot be captured independently of difference. It is rather well known that Socrates displays a tendency to know the examples of some *kind*, and the class of objects is delimited by means of separating out those objects that do *not* belong to that kind. We conclude therefore that negation is the logical basis of the thematic-pair universal and particular.

This is how, we can best reconstruct the reasons why Socrates attempts to capture the essence of a property by means of both positive and negative examples. Of the various kinds of thematic-pairs this pair of universals and particulars is unique in many ways. While negation is certainly one of the unique 'properties' of the pair there are few other ways of capturing the uniqueness.

Another significant manner in which the uniqueness of the thematic-pair can be highlighted is by distinguishing the two levels to which the elements of the pair belong. Traditionally speaking universals belong to the level of Being, and particulars to the level of Becoming. Considering the type-token relation of the elements, we can say, in rather non-traditional terms, that universals belong to the conceptual realm, and particulars to the object-realm. It can also be said that the former belongs to the intensional level and the latter to the extensional level. The significance of this characterization gets enhanced specially in relation to another fundamental thematic-pair of epistemology, genus and species. After introducing the context in which the notions genus and species enter into epistemology we will be highlighting this characterization once again.

Let us now consider the significance of these developments in the context of the Sophists' challenge. Knowledge of particulars is impossible, since there can not be knowledge of things that change. Socrates and Plato agree with the Sophists on this point. But, then they would say the knowledge of universals, i.e. our understanding of the nature and essence of qualities of things, is unchanging, therefore we can *know* universals.

This development is interesting, because new objects of knowledge, namely universals or Forms are defined. To the best of our knowledge the contribution of Socrates and Plato to epistemology mainly consists precisely in the discovery of universals. To understand the nature of this move let us look at its character.

The Sophists demonstrated that there is change *in* the object as well as the subject of knowledge. How is then knowledge of the world possible? One possible way of finding a solution to the problem is to show that knowledge is possible despite the variation involved on the part of the subject as well as the object. One way out is to show that the variation in knowledge is due to variation in the objects (Becoming), while invariance in knowledge, if possible can be due to the invariant object (Being). This demands a further distinction within knowledge into its variable component and invariable component, and also a corresponding division into the resulting kinds of knowledge. Socrates' solution consists in making precisely this kind of move. The variable component of knowledge is named *opinion* or *doxa*corresponding to the knowledge of the variable particulars, and the invariable component, *episteme*, corresponds to the knowledge of the invariable universals. Thus two forms of knowledge have been distinguished corresponding to its two objects. Socrates and Plato concede the point made by the Sophists only with respect to the opinion of particulars, and not with respect to the *episteme* of universals. This is how we think Socrates tried to meet the challenge of the Sophists'.

The reconciliation could have been impossible had Socrates not 'discovered' the need for the creation of an idealized world of Forms, and we will see how this step of idealization is necessary even for scientific knowledge. We will also see below that without this move the transformation of Pythagorean mathematics into Euclidean mathematics would be impossible. Details of the precise role of idealization will be discussed later.

Not only that this distinction between objects of knowledge on the one hand and the distinction between common-sense (opinion) and *episteme* (systematized knowledge) on the other hand, was found essential to the development of science, but most of mainstream epistemology depends heavily upon this distinction.

Is this the only possible way of solving the riddle posed by the Sophists? Aren't there other alternatives? We think that there exists at least one more clear alternative.

The other alternative is to suppose that variance or change at the level of objects is *real*, not apparent. But this real variance has an order or a pattern in it, such that *that* order of variance can be called invariant. Even if the objects of knowledge are of the changing kind, knowledge is possible, because there is a possibility of finding invariance in the changing objects of knowledge. This latter possibility, it can be seen, is significantly different from the previous one, where the invariance is ascribed to an unchanging object of knowledge, universals. The objects of knowledge are not assumed to be invariant. Instead it is assumed that the variance has an invariant structure. There may be other alternatives. But for our purposes distinguishing these two possible answers is sufficient. The epistemological framework that we are going to elaborate below tries to address the epistemological question of the possibility of knowledge based on this second possibility. We claim that mainstream epistemology, since it is based on the distinction of universals and particulars, can not capture the essence of changing objects.

Though this alternative also depends on *abstracting* or *idealizing* a Form out of the *given*, this Form is different in nature from Forms based on the thematic-pair universals and particulars. We would be basing this Form upon another basic logical relation, that of *inversion*, and not *negation*. The attempt of the thesis is to work out a basis for this alternative.

1.2 The Method of Socrates

Once universals are taken to be the objects of knowledge new problems crop up. We have just seen that the thematic-pair universals and particulars was conceptualized in order to distinguish variable and invariable elements of knowledge. The notion of universals is not immediately given, for an understanding of this requires a meta-level abstraction. Since it is held by Socrates and Plato that true knowledge is the knowledge of universals, which is not easily ('naturally') accessible, the acquisition of the knowledge of universals requires deliberate and conscious effort. Since universals are not like the familiar objects which have spatial and temporal properties, they cannot be 'looked' at directly.

This problem is very acutely recognized by both Socrates and Plato. Their answer briefly is that the knowledge of universals can be gained only by conscious effort, and the effort consists of an ordered search towards reaching universals. Knowledge of universals like scientific knowledge cannot be obtained without some form of training. Indeed unless some sort of difficulty is involved in the acquisition of such knowledge the question of method does not arise. For it makes little sense to conceive of a method when the objects of knowledge are immediately and naturally grasped. With these comments about the need for a method, let us look at the essential aspects of the Socratic method.

Two stages can be identified in the Socratic method. In the first stage, the questioner elicits from the respondent what he thinks he knows by asking a question. His answers are then taken as suggestions or hypotheses, which are criticized by deducing consequences conflicting with other opinions the respondent holds by a series of questions and answers. The second stage proceeds by the same method by considering fresh suggestions, criticized and amended until it reaches an end, which is the correct definition of the form.[8]

[8] Cornford 1935, *Plato's Theory of Knowledge: The Theaetetus and the Sophist of Plato,* p. 184.

In this method, the role of a which-question is mainly to elicit from the respondent what he thinks he knows. The conditions that form part of the question, as elaborated above, play the actual role of regulating the thinking of the respondent toward understanding the meaning of an idea. To have an understanding of an idea is to be able to explicitly define it. A notable feature of these conditions is that they are 'known' to the respondent, in the sense that he understands the meaning of the conditions. This is the most significant and essential feature common to most of the methods proposed for arriving at knowledge, i.e., *moving from the known (familiar) to the unknown (unfamiliar).* Not surprisingly, it is also an essential element of any pedagogy.

Coming now to the other type of questions, i.e. the whether-questions or the dialectical questions, they are those for which generally either 'yes' or 'no' are the appropriate answers. Usually the alternatives presented in these questions are a proposition and its negation, or they state explicitly a finite number of alternatives and make some request to the respondent concerning these alternatives. In Plato's Dialogues these constitute the majority of Socrates' questions. Some examples of dialectical or whether-questions raised by Socrates are: "Don't you see that I am looking for that which is the same in all such things?"[9] "Do you suppose that anyone can know that something is an element (part) of virtue when he does not know virtue?"[10] "Do you consider that there is one health for a man, and another for a woman? Or, wherever we find health, is it the same nature (or kind) in all cases, whether in a man or anyone else? ... Is it not so with size and strength also?"[11]

The role of whether-questions is to help the respondent to see for himself (a) how some of his answers contradict his more secure beliefs and (b) to see the worth of certain alternatives by demonstrating how the response fits with common beliefs. It is in the course of raising whether-questions and answers, which constitute the major part of the Dialogue, that definitive answers are arrived at.

It is well known that the Socratic method is dialectical. It can also be characterized as *inductivo-deductive* for it has both the elements of induction and deduction.[12] The method is inductive in the sense that it lays emphasis on grasping the commonness of a given set of particular opinions. It is deductive in the sense that the proposed commonness of a Form is tested by drawing out its consequences, to see whether they 'cohere' with commonly accepted

[9] *Meno* 75.

[10] *Meno* 79.

[11] *Meno* 72. More examples of both kind of questions raised by Socrates are in Santas 1979, pp. 59–65.

[12] However Karl Popper would not accept this interpretation, for he characterizes the Socratic method as hypothetico-deductive or as a method that follows conjectures and refutations. Cf. Popper 1963, *Conjectures and Refutations*.

beliefs. However from this, one should not jump to the erroneous conclusion that Socrates characterized his method to be either inductive or deductive, for he never gave a meta-level analysis of the method he practiced and preached. It was Aristotle who explicitly identifies the two logical methods. We will come to this a little later.

The initial developments of epistemology thus consist in the discovery of universals, and a method of arriving at them. The most important contribution of Socrates in this context is the coordinate set of abstract thinking, universals and particulars, which continues to regulate and structure philosophical reflection since then. The kind of amplification in philosophical reflection that took place after Socrates is undoubtedly due to this coordinate set. It is an instance of a fertile philosophical idea that is responsible for the proliferation of other philosophical ideas. The most significant developments that result from this coordinate took place in the hands of Plato and Aristotle.

1.3 Plato's New Dialectic

Plato develops his edifice upon the foundation prepared by his teacher Socrates, and one of the most important 'brick' in that foundation, as already stated, is the idea of universal (and particular). Plato's views about the questions 'What can be or cannot be known?' and 'What are the criteria of knowability?' are to a large extent similar to the views of Socrates. Let us recall from the above discussion that to know, according to Socrates, is to be able to give an explicit definition of the universal (Form). An 'advancement' over Socrates is that Plato introduces a distinction between two kinds of Forms, *simple* and *complex*. There are enough indications to believe that Plato, in the course of time, clearly made up his mind about the need to further distinguish universals, for he thought that if the objective of *episteme* is not only to arrive at universals but also to characterize them by definition, it is necessary to show how one Form relates to another Form. And the notion of definition requires that the definiendum be analyzed into simpler elements. His dialectic differs from his teacher's in this significant respect. Thus after *Theaetetus*, Plato's attention turned from a group of individuals (particulars) with its common Form (universals) to the relations between Forms themselves, and specifically the relations between Forms that occur in the definition of a Form.[13] The method of arriving at the complex Form or *genus* and dividing it into its ultimate simple Forms or *species* has been formulated in the new dialectic as the methods of synthesis and analysis respectively. Thus to our understanding Plato's dialectic is one of the first comprehensive methods which has incorporated both the contexts of 'discovery' and

[13] Cornford 1935, *op.cit.* p. 185.

'justification', and it is here that we see the rudiments of the method of *analysis*, which has become part and parcel of scientific method ever since.

One may raise the question: 'Why was the need felt for introducing the genus-species distinction?'. The supposition, as mentioned above, is that knowledge is about Forms, and true knowledge consists of a description or a characterization of Forms. The nature of this characterization is such that to describe one Form we need other Forms, for only Forms can combine to form Forms. Particulars can combine to give rise to another particular, but can never *become* a Form. This is because, according to Plato universals and particulars cannot belong to the same world. While particulars are accessible to the senses, universals are accessible only to reason.

One may raise also the question 'Why is Plato after definitions of Forms or universals alone, why not define particulars or individuals?'. We cannot raise the question of defining particulars, Plato would answer, because they are, by nature, not definite or determinable. A definition would use the term 'is' or 'being', which can only be applied to 'Beings' and not 'Becomings'. Since 'Becoming' is associated with 'being produced', 'perishing', and 'changing', Plato refuses to use the term 'is' or 'being' to individuals which are 'Becoming'.[14] Thus we see that the epistemological thematic-pair universal and particular is related to the corresponding ontological thematic-pair Being and Becoming. Since 'Being' is immutable, definite etc., only universals which are Beings can be defined. For Plato, definability is a criterion of knowability. Hence sensible particulars are not the object of *episteme*.

Here we shall briefly see how the changes mentioned above have come about leading to the dialectic, a method of *conceptual analysis*.

Plato's earlier conception about Forms is that they are indivisible (atomon) and simple. But he realises at the end of *Theaetetus* that the objects of knowledge (Forms) are complex, for a definition is an analysis of a complex Form into simple Forms. Socrates, it seems, is not aware of the contradiction between the views that universals be simple and and that they be defined. But, Plato realising this, abandons the earlier view that Forms are absolutely simple and indivisible. It is clear that this is a natural consequence of any view which requires that the object of knowledge be defined. Since definition should not be by enumeration of particulars, the Form to be defined and also the Forms with which it should be defined are all to be found within the world of Forms, he has to make some of the Forms simple and others complex.[15] It follows from this that simple Forms cannot be defined,

[14] Gulley 1962, *Plato's Theory of Knowledge* p. 80.

[15] Though it was found impossible by Plato, definition by enumeration specially in the context of learning, is not uncommon. Ostensive definition, specially discussed in the context of a critique of empiricism by

and hence cannot be known by the method. With this added distinction all the sufficient conditions (sufficient concepts) are available for him to formulate the method of synthesis and analysis. What constitutes the method of analysis? The method of analysis as described by Socrates, (Plato's mouthpiece) in *Philebus* is as follows:

> [W]e ... ought in every enquiry to begin by laying down one idea of that which is the subject of enquiry; ... Having found it, we may next proceed to look for two, if there be two, or, if not, then for three or some other number, subdividing each of these units, until at last the unity with which we began is seen not only to be one and many and infinite, but also a definite number; the infinite must not be suffered to approach the many until the entire number of the species intermediate between unity and infinity has been discovered,then, and not till then, we may rest from division, and without further troubling ourselves about the endless individuals may allow them to drop into infinity.[16]

Thus, the proposed method of analysis starts with a single genus, which will be divided systematically spreading downwards on the basis of differences (differentia) until an indivisible species is obtained. Below the species are individual things (particulars) which partake of the indivisible specific Forms. These individuals however are indefinable and are not the objects of scientific knowledge (systematic ordering of Forms).[17]

But this does not mean that comprehension of particulars is not possible. Plato does allow the possibility of having opinions about particulars, and what he does not allow is the possibility of systematic knowledge of particulars. It may be pointed out that some authors have rendered *'episteme'* as scientific knowledge. But since there is a clear difference between 'scientific knowledge' as used in the modern sense of the term and the Platonic sense, we shall use the expression 'systematic knowledge' for Plato's *episteme*.

The method of analysis, however, should be preceded by the method of synthesis or *collection*. In the method of collection we take a synoptic view and bring widely scattered things under one idea, so that one may make clear by definition whatever it is that one wants to expound at the time, while the method of division allows us to be able to cut it up at its natural joints, not hack at any part like an incompetent butcher.[18] The method of collection is a process of generalization and *abstraction* culminating in the *re*cognition of a single common Form.[19] Thus it fixes the genus to be analyzed.

But no methodological or systematic procedure is possible in collection. The idea

Wittgenstein, belongs to this category. Cf. Wittgenstein 1953, *Philosophical Investigations*.

[16] *Philebus* 16.

[17] Cf. Cornford 1935, *op.cit.* pp. 186–87, and also Gulley 1962, *op.cit.* p. 110.

[18] *Pheadrus* 265.

[19] Gulley 1962, *op.cit.,* p. 108.

must be divined by an act of intuition for which no rules can be given.[20] Then, why call it method? We can still call it so, because this stage is said to be closely related to the method of hypothesis and the Socratic method on the one hand and to the theory of recollection on the other.[21]

Plato's latter account in *Republic* clearly shows an element of the *hypothetical* nature in the method of collection.

> [U]sing the hypotheses not as first principles, but only as hypotheses—that is to say, as steps and points of departure into a world which is above hypotheses, in order that she [reason] may soar beyond them to the first principle of the whole; and clinging to this and then to that which depends on this, by successive steps she descends again without the aid of any sensible object, from ideas through ideas, and in ideas she ends.[22]

Here we see not only the hypothetical nature of the method, we also see how it is linked with the complementary method of analysis, which operates above all kinds of indefiniteness.

This in a way looks like a hypothetico-deductive schema of Popper. However, at least two striking differences should be looked at. First of all Plato has a method of collection, which contains inductive elements of the Socratic method, while Popper goes to the extreme by rejecting *any* possibility of the synthetic method. Popper's arguments against induction, along the lines of Hume, are rather well known. Second, Plato's analysis is with respect to Forms, and as the above quotation reveals, it starts and ends in ideas, while in Popper's hypothetico-deductive schema we have general and particular statements, where particular statements are reports of sensory experience. Plato's dialectic, as we have already mentioned, has no such objective to describe the objects of perception given by sensory experience. Further Plato's *episteme* is not Popper's scientific knowledge. We think that Popper's epistemology is unique in the sense that it is an epistemology minus synthesis, though this development has a history, while Plato's is evidently analytico-synthetic despite lacunae.

It is worth noting that the method of analysis is clearly an original feature of Plato's dialectic and has no clear place in the Socratic method, for it was never discussed in the earlier Dialogues. The only possible rudiment of the method of analysis in the Socratic method is when, intermittently, while leaping towards the universals from particulars, there is an attempt to see if the 'leaps' are proper by enumerative examination, to see whether the consequences are contradictory to common belief. It is possible, therefore, to argue that

[20] Cornford 1935, *op.cit.,* p. 187.

[21] Here we find that Plato's position sounds like Karl Popper's, however these positions are markedly different. See the discussion below.

[22] *Republic,* 511.

the Socratic method is a method of arriving at ideas (synthesis or discovery), which fixes universals, and the method of analysis, which is 'deductive', is a method of *confirmation* or *justification*. It ensures that the result of the former method is coherent (or true). Analysis takes place purely within universals. This is possibly the beginning of conceptual analysis and systematic knowledge, and also a step essential for the development of logic. In the *Republic* Plato makes the point very clear that the method of analysis proceeds 'without the aid of any sensible object' that it starts from ideas, through ideas and in ideas it ends. On this point the two complementary methods on the one hand, and Plato's dialectic and the Socratic method on the other hand, differ.

What is the application of the method apart from the claim of gaining a clear understanding of the world? The knowledge of the new dialectic will guide the progress of actual discourse; it is the philosopher's science of dividing correctly. An expert in dialectic will not confuse one Form with another. In the *Sophist* the Stranger speaks about the utility of the method as follows.

> [H]e who can divide rightly is able to see clearly one form pervading a scattered multitude, and many different forms contained under one higher form; and again, one form knit together into a single whole and pervading many such wholes, and many forms, existing only in separation and isolation. This is the knowledge of classes which determines where they can have communion with one another and where not.[23]

Here we see a glimpse of what Plato has in mind regarding the objective and the outcome of *episteme.*

We shall see below that for understanding the nature and structure of scientific knowledge, the notion of *complex universal* ("single form knit together into a single whole and pervading many such wholes, and many forms"), is inevitable. We shall interpret a scientific definition to be a *complex predicate*, 'truly' attributable only to an idea or ideal system. According to the semantic view of theories, a version of which is being defended in the thesis, modern scientific definitions are taken to be complex predicates or models attributable to an idealized system. (Details are worked out in Chapter 5 and 6.)

From this point of view Plato's contribution with regard to the detailed structure of Forms, interrelating one with the other, can readily be seen as highly significant. However we will base our analysis of non-Platonic (modern) scientific definitions not on the relation between genus and species, but the inverse relation between universals. This is not to say that modern science has no definitions relating genus and species. The whole of taxonomic

[23] *Sophist,* 283.

systematization of various elements of natural science is in the form of Plato's world of Forms. In the present thesis, however, we will specifically concentrate on inversion based relations between special kinds of Forms which we call dimensions. It is also important to make another distinction for our purposes which is that the analysis that we find in this world of Forms, will be taken as conceptual analysis, as contrasted with the analysis of arguments where the elements are not Forms but statements or judgements. We will shortly see how Aristotle 'invented' an analysis of judgements, as contrasted with Platonic analysis of concepts. Before we turn our attention to another master's contributions, we first recapitulate, and then end with a statement of what is going to come.

If one looks at the general picture of philosophical speculation after Socrates and Plato, we find that a new abstract level is created 'above' the concrete. This is not to say that thinkers before them did nothing abstract. But the difference, according to our understanding, is that the abstract entities and relations 'invented' were given the same place along with other 'corporeal' things. When Pythagoreans, for instance, abstracted out the notion of number, they held that what they see in reality are numbers, for they did not postulate an independent world of numbers. Both apparent and real aspects are seen in a 'undivided' region. Plato's picture, on the other hand, consists of an independent world order of Forms, distinct from the unreal world of particulars to which we have access through sensations. Metaphorically we can describe the development as follows: Before Plato, thinkers thought that both Being and Becoming 'occupied' the same *plane*, while after Plato, we can say that there are *two planes*, one of Being and another of Becoming, one above the other with a gap in between, as shown in the figure 1.1. This development certainly helped in a

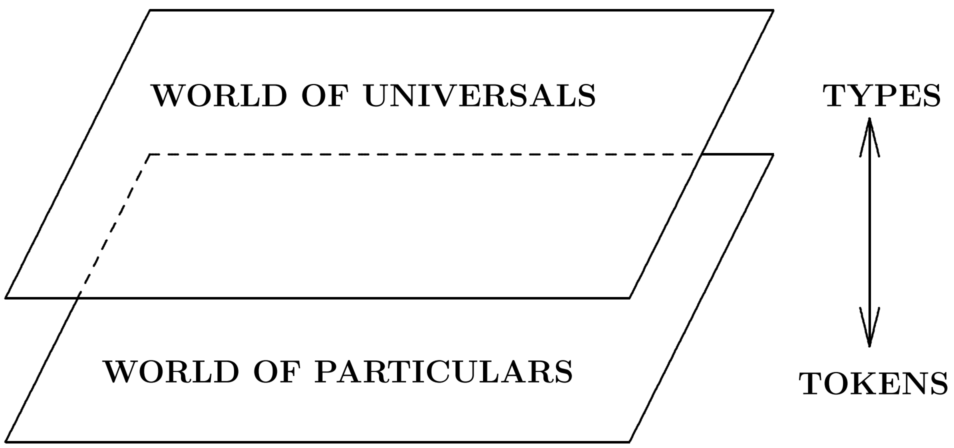


Figure 1.1: Picture of Epistemology After Plato

remarkable manner, catalyzing a new stream of philosophical development, specially those that are essentially based on thought or abstraction, such as dialectics, formal logic, grammar etc., but the gap between the planes eventually became a *gulf*, generating in due course wide ranging epistemological problems.

Philosophers disagreed about the ontological status of the planes. Which plane is virtual and which real? Plato, as is well known, argued for the reality of the upper plane. Aristotle made the upper plane a *nous* dependent world, and the lower plane real and independent of the *nous*.[24] This gave rise to the problem of realism. Philosophers also disagreed on the problems related to the content of universals. Plato argued that the objects of the upper plane are the objects of scientific knowledge, while Aristotle argued that the upper plane is an essential 'instrument' for having scientific knowledge of the objects belonging to the lower plane.

Plato denied any *logical* relation between particulars and universals, consequently propositions for Plato are only relations between universals. Aristotle invented a *heterogeneous* relation between them in the form of thematic-pair *subject and predicate*, necessary for forming a judgement or statement. (The Stoics also have a possible hand in this invention.) The formal logic of categorical statements is based on this heterogeneous relation.

Before we consider the other important thinker, Aristotle, a few observations are in order regarding the place and role of particulars in the methods of Plato and Socrates, the role and place of universals being secure in their method. This is found necessary because Plato's preoccupation with universals and his seemingly idealist or rationalist position has overshadowed the usual discussions to such an extent that his epistemology has been portrayed as one that does not in principle give any significance either to experience or to particulars. There are certain apparent pointers to show that he does not appreciate the role played by particulars. However we will see that these hints are misleading and have led to incoherent portrayals of Plato's thought. By carefully following the role played by particulars in the process of the method of recollection (the method of synthesis) leading to the discovery of knowledge of universals, we shall try to show that Plato did not deny the role of experience and of 'opinion' of particulars in the process. Without this, the method, which is characterized as dialectical remains bereft of one of its complements. M.F. Burnyeat has argued that Plato did not depreciate the role of particulars in his method.

The mistaken view of particulars must have emerged due to Socrates' disapproval of definition of forms by way of examples. Whenever examples are given as an answer to

[24] Aristotle's disagreement with Plato is also in terms of the metaphysical status of universals and particulars. He regards particulars as 'containing' both form and matter. Their views on ontology can be contrasted by the labels *universalia ante rem* (universals prior to the objects) and *universalia in re* (universals in the objects), where the former is Plato's and the latter Aristotle's conception.

Socrates' which-questions by the respondents, he ridicules them on the ground that they are not answers to his question. For example Theaetetus responds to Socrates' question 'What is knowledge?' by giving examples such as geometry, the art of the cobbler and other craftsmen.[25] To that Socrates replies that that is not the kind of answer he wants. He illustrates to Theaetetus the nature of the answer that he is looking for. To the question 'What is clay?', to reply that clay is potter's clay, oven-maker's clay, brick maker's clay etc., would be ridiculous. He says a desirable answer would be that *clay is moistened earth*.[26] This illustration is usually cited as that which demonstrates a depreciatory role for particulars in the method.

Socrates disregards examples even as a preliminary answer to the question 'What is knowledge?'. Why? Because he considers that to know or to understand is to be able to give a explicit definition of the Form or universal. A definition of a Form cannot be obtained by enumeration. He says so because we may have learned to *use* a name for a collection of things, without ever giving a thought to the question of what is *common* in that collection of things. The philosophical turn that takes place with Socrates and Plato, to our understanding, consists precisely in this. They have seen a possibility of talking about something other than the common name that stands for all the examples—a non-trivial characterization that describes the common quality applicable to a collection of things.

To arrive at this sort of understanding giving attention to examples (particulars) is necessary in Plato's dialectic. This is clear from the way Socrates examines various definitions suggested during the Dialogues. The definitions suggested are examined with reference to examples. He only insists that the commonness of all examples be explicitly stated. Sometimes he himself would add examples to help the respondent. He rejects examples only because examples alone do not constitute knowledge or an adequate definition. He regards them as data from which a definition is to be reached by a process of 'leaping' generalization. There are enough indications to believe that this is an inductive leap. (Aristotle characterizes the Socratic method as inductive.) Socrates explains that if the definition of an idea is known then we will be able to tell what is and what is not an example of the idea.[27]

Plato held that the knowledge of Forms is present in us in dormant state, and it can be brought back to our consciousness by the help of the method of dialectic.[28] There are certain 'facts' which upon initial consideration appear unfamiliar, even incredible, but after, attending to them by pure reason, they appear self-evident. This is usually experi-

[25] *Theaetetus* 147.
[26] *Ibid.*
[27] *Euthyphro* 6.
[28] *Meno* 81ff.

enced with regard to mathematical 'facts'. This is the nature of the truth that is achieved after recollection. In Plato's dialectic, as well as in the Socratic method, one property of method that is mentioned above is necessarily present, which is to help learning, either in oneself or to others. This process is never complete without the knowledge of particulars (examples), for how could one judge whether there is real understanding or consistent rule following behavior?[29] So it is one thing to say that Socrates and Plato argued going beyond opinions about particulars (examples) and consequently have lowered the rating of opinionated knowledge, but another thing to say that particulars have no role to play in the dialectic. The former statement applies to Plato's view but the latter does not. Without the method of recollection, where sensory experience of particulars has a definite role to play in the process of generating knowledge of Forms, Plato's dialectic is incomplete. It is, we think, correct to say that for Plato sensory experience and eventually the knowledge of particulars plays an *instrumental role* in gaining the real knowledge of Forms. We shall see below that Aristotle differs with his master on this issue in a subtle way.

1.4 Aristotle's Empirical Method and Logic

Before we go on to a statement of Aristotle's method of scientific demonstration, it is worthwhile to compare him with his master, for he disagreed on crucial matters and it is from these disagreements that his method, which is generally considered the real scientific method, emerged.

The most crucial difference is with regard to the status of the dialectical method. Aristotle differentiates two kinds of methods, viz. the *empirical* and *dialectical* methods. Empirical inquiry begins from perception, followed by induction and generalization, and tests theories by appeal to experience. Dialectical inquiry is initiated from common beliefs, followed by raising and solving puzzles, and tests theories amongst common beliefs.[30] Philosophers, according to him, argue according to the truth which is known by nature, and we can reach this by the empirical method. Dialecticians on the other hand argue according to common belief.[31]

Why did Aristotle demand two distinct methods? An answer to this question can

[29] The latter Wittgenstein argued that understanding does not consist in anything more than following a rule consistently. He criticized Socrates in *The Blue Book*, for being so obsessed with discovering the essence of knowledge that he refuses to look at Theaetetus' examples. For a detailed argument against Wittgenstein's view of Socrates, cf. Burnyeat 1977, *Examples in Epistemology: Socrates, Theaetetus and G.E. Moore,* pp. 381–383.

[30] Cf. Irwin 1988, *Aristotle's First Principles* p. 26.

[31] *Ibid.,* p. 534, n14.

be furnished if we understand the intent of some of the new divisions he introduced, his major differences with Plato, and some of his original, and positive contributions to philosophy and logic.

Aristotle's differences are based on the fundamental distinction between substance and quality. Aristotle thinks that, dialectic fails to yield scientific knowledge because it deals only with attributes, let loose from the beings to which they are attributed. In *Metaphysics*, for instance, he says, "dialectic and sophistic deal with the attributes of things that are, not of things qua being, and not with being itself in so far as it is being; ... "[32] Here Aristotle is pleading for a distinction between attributes on one hand and substance on the other reintroducing the thematic-pair, substance and quality, prevailing in the thinking of Thales, Anaximander and Anaximenes.[33]

More significantly Plato and Aristotle differed on the notion of definitions. Definitions, according to Aristotle, are statements of essence of a substance which inheres in it, while for Plato they represent the way in which a particular Form is related to other Forms. For Aristotle's predecessors no definition of substance is possible, since there was no 'being-what-it-is', and therefore they were not knowable. M. Grene justifiably maintains that Aristotle's predecessors including Plato were unable to unequivocally state the prerequisite for the establishment of scientific knowledge. The prerequisite is *real definition* in contrast to *conventional definition*. Plato's definition ultimately depends on conventions held by the community because, as mentioned above, Plato's dialectic is initiated by common beliefs. A real definition is a statement of the *essence of things*, belonging to the lower plane, and it speaks of "the peculiar substance of each thing, and what it is to be that thing". Aristotle contends that substances fall naturally into classes in such a way that we can specify, in carefully chosen formulae, their essential natures.[34]

With this added distinction Aristotle classifies universals into *accidental* and *essential*. This new distinction should not be viewed as an alternative to Plato's distinction of Forms into genus and species. Aristotle also holds this Platonic division of universals. The object of scientific knowledge is to know the essence of things by discovering real definitions, and the knowledge of the essence is obtained *through* universals. Besides the essence inheres in particular substances. Certain universals which describe a thing without referring to its essence, are accidental; these do not constitute the objects of scientific knowledge. This is a point of difference with his master who believes that all universals have a world of their own

[32] *Metaphysics* 1061 b 4–11.
[33] For details of the views of these thinkers see Chapter-7.
[34] Grene 1963, *A Portrait of Aristotle* p. 81.

and are the objects of scientific knowledge.

The character of this transformation in view, rather an inverted view of Aristotle, is that scientific knowledge is about the essence/s *present* in the lower plane, while Plato's is about the Forms present in the upper plane. (See figure above.)

There is another point of difference between Plato and Aristotle with regard to the dialectical method and the scientific method, which is very crucial for understanding the nature of scientific knowledge, in the modern sense of the term. This is in relation to fixing the subject-matter. The demonstrative arguments of the scientific knower, on the other hand, predicates essential attributes of a carefully restricted subject-genus. The dialectician does not restrict his subject. He maneuvers the argument to his advantage whatever the context of his argument. Therefore, dialectical arguments, though formally valid, are baseless and unscientific.[35] Aristotle says, in *Posterior Analytics*, that we should not try to know the whole of existence, the *summum genus*, through scientific method.

> We can never know anything about anything, as distinct from having opinions about it, unless we cut out one limited subject-matter out of a wider range and restrict ourselves to it.[36]

Plato on the other hand seeks definition, against a background of indefinite flux. Grene presents the difference between the two methods cogently as follows:

> The unambiguous predications of science are possible only because things sort themselves out naturally into kinds; knowledge results from the mind's response to such natural groupings. Transcend them and you transcend the limits of univocal speech, which are the bounds of science. You have strayed beyond the well-fenced limits of the being-what-it-is and are lost in the quicksands of dialectic once more.[37]

We will see below that one of the important differences that can be found between the pre-Platonic inquiries and post-Aristotelian inquiries consists precisely in this point of confining oneself to a subject matter. Problems of inquiry are defined within the limits of this *local*, *vis á vis.*, *global*, domain of inquiry. Without localization, essentially paradigmatic science cannot be said to have begun. In this sense, the earlier conceptions about nature before Plato, and of Plato, can not be called proper scientific knowledge.

It is therefore claimed in the thesis, that despite Aristotle's failure in arriving at correct scientific conceptions, his successful contribution in directing the attention of scholars

[35] Cf. Grene 1963, *op.cit.* pp. 190–191.
[36] *Posterior Analytics* 97 a.
[37] M. Grene 1963, *op.cit.* p. 87.

towards problem oriented research can be rated as a revolutionary suggestion. We will see in detail his specific suggestions in the Case Studies. Aristotle had very strong ground to differ from Plato on certain basic assumptions. There is another dramatic development by Aristotle, which is regarding the kind of relation that is admissible between universals and particulars.

It is one of the unique features of Aristotle's philosophy that while Plato associated universals and particulars with the thematic-pair Being and Becoming, Aristotle associates them with the thematic-pair *subject and predicate*. This is an indication of his attention towards statements and language. Unlike Plato, Aristotle concentrates on statements as elements of his study. Plato's interest was either on a single idea or on relationships between ideas. This is not to say that the Platonic association is not accepted by Aristotle, for he never rejects the distinction between Being and Becoming. Rather he continues to think with the same metaphysical orientation, though he prefers for a very important reason, which becomes clear as we proceed, to use the terms 'Form' and 'Substance'.

Nothing is available in Plato's works in favor of subject-predicate distinction. Besides he would not have agreed with this distinction to be associated with universals and particulars, because, according to him, a statement is an instance of a blending or 'weaving-together' of Forms.[38] That is, it is a combination or synthesis of two or more universals. This point is significant because we can distinguish only two significant ways of relating Forms in Plato's philosophy, granting his view on statements, viz., part-whole relation and identity relation. All non-definitional statements are statements relating genus and species. For example, 'Man is an animal' means the species Form 'Man' is a part of the genus Form 'Animal'. And according to Plato if the Forms are 'properly', i.e., coherently combined, they are true, otherwise false—a coherence theory of truth. All definitional statements on the other hand are statements where a Form is defined by *identifying* it with the combination of Forms that define it. For example, in the statement "Man is a rational, biped, animal" the Form 'Man' is identical with the synthesis of the Forms, 'Animal' + 'Biped' + 'Rational'. This can appropriately be termed a *chemistry of Forms*.

It is necessary to digress and make an observation here about a deficiency of Plato's conceptual analysis, which is dubbed as a chemistry of Forms. This is with regard to the lack of any scope for stating invariance of changes in the Platonic framework. Modern natural science captures the Form of Becoming (variable and changing phenomena) by discovering the invariant proportionality relations between variables. It it however clear that Plato has a

[38] Cornford 1935, *op.cit.* p. 266.

reason for not searching for this. As mentioned above Plato is working out one of the possible options, and certainly not the only possible option, of responding to the Sophists' challenge. For Plato Forms represent invariance, therefore the question of entertaining a possible science of variations is inconceivable in his theory of knowledge.

Even in Plato's metaphysics, as presented in *Timaeus*, where a mathematical Atomistic theory of reality is proposed, what we see is that Beings are allowed to combine and separate giving rise to a variety of species. Despite his mathematical maturity he could not foresee the other possibility of a Form 'within' variations. To our understanding he was obsessed with his discovery of Forms, with the 'one over many relation', and can not see the possibility of 'one to one relation', necessary for capturing the Form of functions based on proportionality.

Aristotle makes a genuine attempt, though ultimately he too fails, to study a science of motion in *De Caelo*, and *Physics*. An attempt to explain his failure is made in the Case Studies. Let us return to the Aristotle's views on the subject-predicate relation.

Aristotle's views on predication are more elaborate and different from Plato's. The difference is not merely that he allows a subject-predicate relation between universals and particulars, he furthermore insists that the subject of a statement can refer to either a Substance or a Form, but the predicate of a statement should necessarily be a Form. He says in *Metaphysics* (1017 b 10-14) that Substances are not said of a subject. One of his criteria for recognizing a Substance from Form is that it be a basic subject.[39]

Since anything that can be said of something else as its subject must have some kind of generality, i.e. it can be said of other objects also, and since only universals can have this character, only universals can act as adjectives. "An adjective which could be used only on one unique occasion would not function as an adjective; and the something unique it designated would not be something sayable of a subject."[40] Therefore all things that are predicable of subjects are non-individual.

On the basis of the condition 'present in a subject' (inherence) we can distinguish between two kinds of individuals, dependent and independent individuals, things that do not exist by themselves and things that do. Those things which are individuals and independent, e.g. this man, this horse etc., are first substances. These are the ultimate subjects in which dependent individuals (individual accidents) are present, and of which other predicates are said. Scientific Knowledge depends wholly on the right application of predicates, which are

[39]Similar statements suggesting the same are found in *Categoriae*, where the fourfold division of things is discussed. Grene discusses the relevance of fourfold division of things for science. (Grene 1963, *op.cit.* p. 73.)
[40]*Ibid.,* p. 73.

general, to kinds of substances, which are also general. Thus science can approach as far as independent individuals, while dependent individuals, being accidental, cannot be approached by science. Thus Grene writes:

> The propositions which constitute a science are univocal statements attributing certain characteristics to certain kinds of substances. ... In order to establish a science of some subject matter, we must take a natural class of first substances, and elicit from some other category or categories, also at appropriately generalized levels, the right predicates for the characterization of its essence.[41]

A correct relation between a class of first substances and an appropriately chosen predicate produces a real definition. Let us recall that, according to Aristotle, a real definition is the prerequisite for the establishment of scientific knowledge, and that it is a statement of the essence of the thing defined.

Another most remarkable achievement of Aristotle is that almost single handedly he developed the foundations of formal logic. Though Aristotle's logic is limited to Categorical propositions, it is nevertheless a landmark achievement in the history of ideas. Our concern here, however, is to highlight the too obvious point that unlike the conceptual (philosophical) logic of Plato, Aristotle's syllogistic logic is a logic of statements of the subject-predicate form. It is important to make this observation that this logic, like most of modern logic of statements, is based on *the principle of non-contradiction.*

> The same attribute cannot at the same time belong and not belong to the same subject and in the same respect.[42]

The crux of the proposal of the present work lies in presenting a visualization of a possible logic of construction based on the logical relation of *inversion*, which has at least three contrasting characters with the deductive logic. First, it is not based on the principle of non-contradiction, second, it is not a logic of statements, and third, the outcome of the inference is not a statement but a constructed structure. Detailed characterization, and argument will be presented in Part-II, and Part-III.

Having noted the main thematic features of Aristotle we shall highlight certain important features of his scientific or empirical method, which can be regarded as one of the first scientific methods.

[41] *Ibid.*, pp. 77–78.
[42] *Metaphysics*, 1005 b 19–20.

1.5 Aristotle and the Joint Method

Aristotle talks of 'the right method of investigation' in the *Posterior Analytics* (Bk. II, ch 13), which "starts by observing a *set* of individuals, and considers what they have in common",[43] and then examines another set of individuals, generically identical, and so on till we arrive at a 'principle'.[44]

This is the description that he offers for the starting point of the method of investigation, which is clearly induction – from particulars to universals. This is Aristotle's second level of induction which makes use of the 'products' of the first level of induction. The first level fixes the universals and the latter the first principles or real definitions. According to the traditional method of investigation we arrive at the knowledge of the unknown (first principles) from known (the knowledge of the universals). The knowledge of the universals comes from intuitive faculties of human being or *nous*, which includes the operation of perception, experience and memory. Knowledge of the first principles depends on *nous*. The first step does not require the expertise of the investigator, in the sense that he need not consciously use his faculty of thinking. In the sense explicated above about the nature of method, this first step cannot be properly regarded as a methodological step. Aristotle says the following regarding this first level of induction.

> [All human beings] have an innate faculty of discrimination, which we call sense-perception ... after the act of perception is over the percipients can still retain the perception in the soul.[45]

If this happens repeatedly a coherent impression is produced, thus giving rise to memory. And repeated memories of the same thing constitutes experience, i.e., memories of a thing may be many but they constitute a single experience.

> And experience, that is, the universal when established as a whole in the soul - the One that corresponds to the Many, the unity that is identically present in them all - provides the starting-point of art and science.[46]

These faculties arise from sense-perception, just as, when a retreat has occurred in battle, if one man halts so does another, and then another, until the original position is restored. The soul is so constituted that it is capable of the same sort of process. Up to this point Aristotle is talking of the first level of induction, which is a prerequisite for the second level

[43] Our italics.

[44] Here individuals can be safely interpreted as particulars, though they are not interchangeable in all instances.

[45] *Posterior Analytics* 99 b25-100 a 14.

[46] *Ibid.*

of induction, which alone is a part of the *joint method* of scientific investigation. Regarding this Aristotle (100 a 15-b 5) says:

> As soon as one individual percept 'has come to a halt' in the soul, this is the first beginning of the presence there of a universal ... Then other 'halts' occur among these (proximate) universals, until the indivisible genera or (ultimate) universals are established. E.g. a particular species of animal leads to genus 'animal', and so on. Clearly then it must be by induction that we acquire knowledge of the primary premises, because this is also the way in which sense perception provides us with universals.

Thus the path to the first principles is inductive. Clearly the processes that the term 'induction' designates in modern and Aristotle's sense are so different. This is more akin to the method of synthesis in Plato's dialectic, except that it is rooted in sensory experience, while in Plato this is aided by hypothetical 'leaps'.

After fixing the genus by induction, he describes how a definition can be established through the method of division in Ch.13. The investigator begins with the subject-genus and divides it carefully to get the order of differentia correct, checking that the divisions are exhaustive and that members of the species being divided all lie under one branch of the genus.[47]

This latter method of division, as we clearly see, is akin to that of the method of analysis in Plato's dialectic. But, one thing we must bear in mind, which is that Aristotle provided only hints and no explicit statements in this regard, and is therefore subject to the whims of the interpreter. Nevertheless a few points are clear: The induction should precede the method of division. The inductive method arrives at the definition, while division establishes it. Induction moves from the particular to the general, and division from the general to the specific.

Interpretations offered by the Italian Aristotelians of the school of Padua suggest that Aristotle is the champion of the joint method of analysis and synthesis. However it should be kept in mind that their writings are commentaries mainly of *Physics* and *Posterior Analytics*, where the search is to discover *causes* of specific physical events. In this sense, the terms, "analysis" and "synthesis" in the following discussion describe different methodological procedures. This difference is the difference between the methods used in conceptual understanding (relations between genus and species) on the one hand, and indirect understanding of natural phenomena by demonstrative syllogism on the other.

Aristotle never explicitly used the terms "analysis" and "synthesis", but these terms are used by the later Aristotelians appropriately following the description he gives of the two

[47] Cf. Noretta Koertge, in Thomas Nickles (ed.) 1980, *Scientific Discovery, Logic, and Rationality* p. 143.

kinds of demonstrations, which are two complementary modes of knowing the fact. All syllogisms, Aristotle says, will not yield scientific knowledge which is by demonstration, i.e. by demonstrative syllogism. The premises of the demonstrative syllogism must be true, primary, immediate and better known than the conclusion.[48] The relationship between the premises and conclusion is like that of cause and effect.

Of the two modes of knowing the fact, the first one is called *demonstration qua* which follows the natural way of discovering the cause or the fact, which is possibly by inductive method, and the second one is called *demonstration propter quid*, which follows the causal order starting with the discovered cause and deducing the effect.[49] The Greek terms for the two modes are *oti* and *dioti*.

In the beginning of *Physics* (Bk.I 184a) Aristotle says that the starting point of science is a confused mass, usually interpreted as that of effects, which require analysis.[50] That is science (not the *episteme* of Plato, but *Physics* of Aristotle begins with the known or proximate (effect), by the help of which the unknown or the ultimate (cause) can be reached by the method. The former movement from effect to cause is called the *resolution*, while the latter movement from cause to the effect is called the *composition*. After the discovery of the cause, the effect would be explained in terms of the cause, i.e., the effect is approached *again* in the method *indirectly*, via the knowledge of the cause. There is thus a *regress* or return to the effect with which the inquiry started. However there is no circularity in the process. Paul of Venice (one of the Italian Aristotelians) defends Aristotle's joint method from the charge of circularity as follows:

> For in scientific method (*in processu naturali*) there are three knowledges: the first is of the effect without any reasoning, called *quia*, that it is. The second is of the cause through knowledge of that effect; it is likewise called *quia*. The third is of the effect through the cause; it is called *propter quid*. But the knowledge of why (*propter quid*) the effect is, is not the knowledge that (*quia*) it is an effect.[51]

In other words, first, the knowledge of the effect thus obtained is arrived at *indirectly* via the cause, and second, the modality of the knowledge involved is explanatory. Using contemporary expressions, the knowledge of the effect via the cause is theory impregnated. Since the causes are the sorts that are usually not given to our direct sensory experience, they need to

[48] *Posterior Analytics* Ch-2, 71b.

[49] *Posterior Analytics* Ch-13, 78a.

[50] All the Italian Aristotelians (of Padua) interpreted the starting point of the method to be the knowledge of effects. Good details of Italian's reading and development of Aristotle is presented by Randall Jr. (1962). It is interesting to know that Galileo, according to Randall, inculcated Aristotelian method from them during his visit to the University of Padua at the time.

[51] *Summa naturalis*, Book I, cap. 9. Quoted in Randall, *op.cit.,* p. 288.

be ‘discovered’ by theoretical imagination. While the place where reason should play its role is properly identified, the nature of the reason, except that it has a name of resolution, is not clearly specified.

We think that this deficiency remained uncorrected not only in the entire Aristotelian thinking, but also in other methodologies proposed later. We shall see in detail in the case study below, how Archimedes, and later Galileo, who belong to a *mixed tradition*, could devise a model method based on *inverse reasoning*, for the discovery of ‘causes’, followed by the explanatory regress.[52] The exact role and nature of reason, complementing the role of experience, is specified, giving rise to the proper scientific knowledge in the modern sense of the term.

The worthiness of the Aristotelian model, however, lies in properly identifying the place where the role of reason is involved in the context of discovery.[53] Niphus (another Italian scholar) interprets that the resolution of effect is captured in a conjectural syllogism, while the composition of effect with the help of cause is captured in a demonstrative syllogism.[54] A long period of critical reconstruction of Aristotelian teachings culminated in the works of Zabarella, who, it is claimed by Randall, influenced Galileo.

Typical to the tradition Zabarella characterizes method as an intellectual instrument producing knowledge of the *unknown from the known*. Method is a kind of syllogism, according to him, because it connects one with the other through inference. There are only two possible methods, composition or demonstrative method and resolution.

> Demonstrative method is a syllogism generating science from propositions that are necessary, immediate, better known, and the causes of the conclusion ... Resolutive method is a syllogism consisting of necessary propositions, which leads from posterior things and effects better known to the discovery of prior things and causes.[55]

Zabarella and the whole new generation of scientists that followed him, of which Galileo is also a crucial figure, entertained the belief that scientific experience springs from mere ordinary observation. They insisted that experience must be first carefully analyzed to discover the principle or cause of the observed effects. Thus, science proceeds from rigorous resolution of a few *selected instances* to a general principle, and then from that principle to the systematized

[52] The expressions “mixed tradition”, and “inverse reasoning” will be explained below.

[53] It should be pointed out that by “reason” is not meant deductive reason alone. Such a narrow view of reason is the character of scientific methodology of the current century, which more or less has eventually denied the complementary creative component.

[54] Cf. Randall, *op.cit.*, p. 290.

[55] *De methodis*, Lib. I, cap. I. Quoted in Randall, *op.cit.* p. 293.

science, and then composition as a proof.[56]

He finds four stages in the process of this regress: observation of the effect; resolving the complex fact into its components and conditions; mental examination of the hypothetical cause to find its essential elements; and demonstration of the effect from that cause. The third stage is called "mental examination", which Niphus called *negotiatio* of the intellect. He elaborates, then, the two things that are considered in the middle stage of mental examination, which helps us toward knowing the cause distinctly.

It is interesting to see that the first three stages are part of the method of resolution, while the fourth demonstrative stage, which is deductive syllogism, is all that there is to the method of composition. This is an indication that the problem of discovery is the more dominating concern than the problem of justification. Today's situation is just the reverse, as we will see below. Another point to note here is that the Aristotelians have seen that without the involvement of "mental examination", i.e., involving a source other than sense experience, the discovery of causes is impossible. It is therefore well recognized by Aristotelians that scientific investigation depends on both creative and sensory faculties.

One of the shortcomings of Zabarella's method, as well as of any other Aristotelian, is that in the discovery of scientific principles no role is assigned to mathematics. However he makes interesting observations worthy of consideration. Like his predecessors, such as the Averroists, he makes the distinction between the method of resolution suitable for natural science and the method of "analysis" of mathematics. In the latter we can start from either the principles or the consequences. In the former, however, we must start with effects observed by the senses, i.e. with the method of resolution. In the mathematical method whether we start from resolution or composition is merely a technical matter, and each of the methods here are independent.

This is a general sketch of the joint analytico-synthetic methods in Aristotelian thinking. We distinctly see that the method, over a period of time, has been enriched without a corresponding development of scientific knowledge. Either something is wrong with the methodology, or it is preached but never practiced, or it could also be that methodology has nothing to do with the actual development of scientific knowledge. All these doubts and speculations are natural, for this whole stream of philosophical reflection from Plato's Academy to the University of Padua could not produce scientific knowledge, in the modern sense of the term. But this is just one stream that emanated from the Academy. There are

[56] We will see in the case study that Archimedes made a big break-through by choosing balance as the selected instance, from which he *lifts* (abstracts) general principles of the lever, which in turn became fundamental for not only statics, as usually considered, but also for the development of modern science in the hands of Galileo, which will be disussed in detail below.

other rather fertile streams, from the point of view of the development of scientific knowledge.

One such stream is based on Euclid's mathematical edifice, while another stream is based on Archimedes' experimental edifice. Needless to say, both these have eventually become very hard bricks in the bedrock of scientific knowledge. Interestingly both the streams developed in the School of Alexandria. What is peculiar to this School? We will have to wait till it gets answered eventually in the course of the essay. Presently our concern is not to narrate the success story, but the unsuccessful story of philosophical reflections on scientific knowledge and method.

Before we end this section on Aristotle we shall make, what we regard, an observation of some interest. It is to note that the method of resolution can be said to be a part of, what we today call, the context of discovery, while the method of composition can be regarded as a part of the context of justification. We are aware that in the current usage the context of justification is deductive, and hence called analytical, while the context of discovery is ampliative or inductive, and hence synthetic.[57] It therefore appears that, in the same context, one has seen resolution, while the other has seen synthesis. This terminological inversion should not cause much confusion, when we realize that the Aristotelians are talking in terms of what is happening to the objects of inquiry, causes and effects, while methodologists of the current century use a linguocentric vocabulary, which cares more about what is happening to the 'instruments' of inquiry in the process of inquiry, such as statements. This inversion in the philosophical orientation, as it appears to us, is due to the shift in points of view from the extensional view to the intensional view. Despite this transformation in orientation, the central concern, which is to attempt answering the two fundamental questions of epistemology, which continued till the middle of this century. It would be a very interesting problem for a historian of ideas to study what factors led to this change. To understand this change demands a separate work. Since we are not presently engaged in understanding the intricacies of this historical problem, it suffices to make the following observation.

In the conceptual methods of synthesis and analysis, which are discussed above, i.e., those of Plato and of Aristotle, there exists a process called synthesis, which refers to the process of cognitive movement from the level of particulars to the level of universals. And there exists another process called analysis which refers to the division of genus to species, both of which belonging to the level of universals. Thus in different contexts the terms meant different things. However it is interesting to note that in this case too, the two processes correspond to the contexts of discovery and justification. Thus the joint methods should

[57]We will see below that in due course this synthetic component was abandoned by many leading to epistemology-minus-synthesis.

be properly contextualized to know the proper reference and thus to avoid confusion. The general methodological theme of analysis and synthesis remains only a theoretical model functioning as a meta-level guide to organize epistemological thinking, which can be seen in several contexts in the history of methodology.

One significant theme of the joint method is that science does not start from scratch, for it starts from something which is already known. This theme remains a part of the other successful stream mentioned above, which (so we claim) generated science proper. To this we now turn.

1.6 Method in Greek Mathematics

Among the ancient mathematicians too the method of discovering solutions to mathematical problems followed the pattern of reaching the unknown from the known, and then returning to the known from the newly discovered knowledge, in order to validate newly arrived knowledge. The method employed by ancient geometers to calculate the area of any regular shaped surface can be a best example to illustrate the method of reducing the unknown to the known. It was taken for granted that the area of a rectangle is the product of its base and height, (Area= $b \times h$), which is the known constituent of knowledge. From this they found out methods of calculating the areas of all polyhedrons. The method can be generally characterized thematically according to the joint method as follows. As shown in the figure 1.2, any other polydedron other than a regular rectangle is first *analyzed* (dissected)

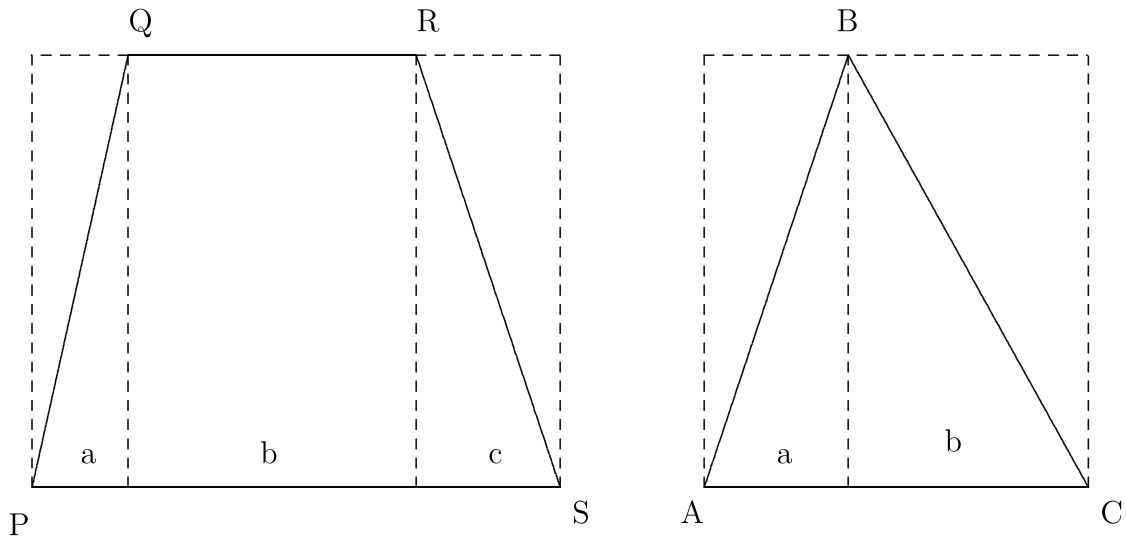


Figure 1.2: Reducing the Unknown to the Known

in such a way that the area can now be seen as a sum of several rectangles, or several half rectangles. Since they know the method of measuring the area of any rectangle, all that they need to do is to add the rectangles and half rectangles to get the total area of the unknown polyhedron. The polyhedron PQRS, for example, is first analyzed as if it is made of two half rectangles (shown in dotted lines) and one full. If the areas of the rectangles are a, b, and c, in the order they are drawn, the area of PQRS will be $a/2 + b + c/2$. Similarly for the polyhedron ABC, which is a triangle, $a/2 + b/2$ or $1/2(a + b)$ will give its area.

The same method was applied to calculate the area of irregular shapes, though the value obtained would be true only approximately. It is well known that Archimedes extended the same method to know the measurements of other complicated shapes like the circle, the oval etc. His method has later come to be known as the *method of approximation*.

This instance of the joint method in mathematics is clear and simple, because the operations involved are 'extensional'. However Euclid's version is pretty involved due to its highly theoretical character. The characterization of the method can be found in Euclid's *Elements*, Book XIII. The account of the method as understood by Euclid and other Greek mathematicians is given by Pappus (c.300 AD). The method, Pappus says, is for those who "are desirous of acquiring the power of solving problems ... and it is useful for this alone." The method, according to Pappus, was worked out by Euclid, Apollonius and Aristaeus, which proceeds by way of analysis and synthesis.

> Analysis . . . takes that which is sought as if it were admitted and passes from it through its successive consequences to something which is admitted as a result of synthesis: for in analysis we assume that which is sought as if it were (already) done, and we inquire what it is from which this results, and again what is the antecedent cause of the latter, and so on, until by so retracing our steps we come upon something already known or belonging to the class of first principles, and such a method we call analysis as being solution backwards.
>
> But in synthesis, reversing the process we take as already done that which was last arrived at in the analysis and, by arranging in their natural order as consequences what were before antecedents, and successively connecting them one with another, we arrive finally at the construction of what was sought; and this we call synthesis.[58]

This method is fundamental to Plato's program in mathematics, which is to find trivially true axioms and to deduce all of arithmetic and geometry from them. Euclid was considered by Proclus as one who completed Plato's program.[59] However, from the point of view of our search for a logic of discovery, this method does not have much to offer, because so much is

[58] Quoted in Imre Lakatos 1978, *Mathematics, Science and Epistemology: Philosophical Papers*, Vol.II p. 64.
[59] Cf. P. Marchii 1980, 'The Method of Analysis and in Mathematics, in Nickles 1980, p. 164ff.

assumed in the process of the method, such as first principles which are already considered to be a part of accepted knowledge. What would be interesting is to know how we arrive at the first principles. The above method does not appear to have any scope for that. But from the remarks of Pappus it is clear that it is intended to be a problem solving method. Lakatos and Szabo have suggested that the method can be viewed as a discovery method provided the starting point of the principles is regarded as *hypothetical*. If the hypotheses are rigidly fixed, their use becomes less interesting.

Assuming that problem solving belongs to the context of application of already arrived principles, we find it reasonable to think that Euclid's method does not properly belong to the context of discovery. Further it should be noted that in this version of the joint method, the order of analysis and synthesis appear to be irrelevant. If it is a method of discovery, however, the starting point and the nature of the starting point would matter significantly, because the initial step should be that which leads to the principles or hypotheses. But the method is clearly a perfect "circulatory system", as Lakatos would put it, without beginning and end. Therefore we think that whatever be the significance of the method for problem solving heuristics, it does not throw enough light on the problem of discovery, with which we are presently concerned.

Chapter 2

The Marriage of Mathematics and Natural Science

We have seen above basically three kinds of joint methods of analysis and synthesis. The first kind is about discovering and establishing the relationship between genus and species, e.g. Plato's method of composition and division, and Aristotle's first method of finding 'essences'. The second is the later Aristotelian method of demonstration *qua* and *propter quid* (resolution and composition) extended and enriched by the Paduan school. The third is the method announced by the Greek mathematicians in the form of analysis and synthesis. Another new form of methodology, also put in the traditional analytico-synthetic theme, came into being in the hands of Galileo, though, the method has clear beginnings in Archimedes and later in Kepler. We shall discuss thematically the birth of a new methodology, that has really survived with success till date. It is this form of methodology that will be renovated in the thesis, keeping in mind contemporary epistemological problems. As already mentioned more than once, one common theme of all the analytico-synthetic methodologies is the reduction of an unknown to the known. We will attempt to show that this indeed is the enduring theme of the generativists.

2.1 Galileo's Role in Transforming the Objects of Knowledge

Galileo's contribution, we think, lies in synthesizing the Aristotelian empirical method and the mathematical or geometrical method of the Greeks. Galileo's program is to translate scientific experience into experience that can be expressed in mathematical terms. Anyone who attempts to accomplish this must be able to face a problem long known in history. The problem has been posed by the historians of science as one between the Platonists and the Aristotelians. The former thought that nature is mathematical in character and the latter thought that mathematical descriptions are neither true nor false, while physics tells us the truth about the world by following the empirical method.[1]

Unlike the Platonists, Galileo was not trying to apply mathematics or geometry to describe the Platonic world of ideas, but was attempting to apply geometry to the real physical world, which is believed to be hidden behind the phenomenal world.[2] For traditional Platonists the problem of application does not arise because the perfection of Beings can not be applied to or matched with the imperfection of Becomings. The Book of Nature which Galileo was intending to read, which he believed is written in the language of mathematics, is not a book of the Platonic kind. If that had been the case, truly speaking, Galileo would have had no problem to solve. Whatever Galileo had contributed is acknowledged as a remarkable achievement mainly because he tried to apply mathematical order also in a domain which had been traditionally conceived as non-mathematizable.

In what follows we shall observe that Galileo took some significant steps, first to enable the application of mathematics to physical phenomena; second to suggest that the material hindrances be eliminated in order to find the objects that are independent of sensory experience and convention; and third to suggest a mathematico-experimental method, Galileo's version of the joint method.

In the *Dialogues Concerning the Two Chief World Systems* (1632) Simplicio, like a true Aristotelian, expresses doubts about Galileo's project.

> ... [T]hese mathematical subtleties do very well in the abstract, but they do not work out when applied to sensible and physical matters. For instance, mathematicians may prove well enough in theory that *spheara tangit planum in puncto* ... ; but when it comes to matter, things happen otherwise. What I mean about these angles of contact and ratios is that they all go by the board for material and sensible things. [3]

Now Galileo should either show how a physical (real) plane touches a physical sphere at a *point* or show how an ideal plane can touch an ideal sphere over many points over a surface. In fact Galileo's answer consists in realising that both are geometrically possible. Salviati, who speaks for Galileo, responds to Simplicio's objection after long deliberations.

> Salviati: Are you not saying that because of the imperfection of matter, a body

[1] Cf. Butts 1978, *New Perspectives on Galileo* p. 70.

[2] For more appropriate interpretation of Galileo's position we shall have to wait till the distinction between primary and secondary qualities is also presented.

[3] Galileo Galilei 1632, *Dialogues Concerning Two New Sciences* (Translated by Stillman Drake) p. 203.

> which ought to be perfectly spherical and a plane which ought to be perfectly flat do not achieve concretely what one imagines of them in the abstract?
>
> Simplicio: That is what I say.
>
> Salviati: Then whenever you apply a material sphere to a material plane in the concrete, you apply a sphere which is not perfect to a plane which is not perfect, and you say that these do not touch each other in one point. But I tell you that *even in the abstract, an immaterial sphere which is not a perfect sphere can touch an immaterial plane which is not perfectly flat in not one point, but over a part of its surface, so that what happens in the concrete up to this point happens the same way in the abstract* ... [4]

This argument contains one of the most central thesis of Galileo, in his attempt to show that mathematics can be the language of the book of nature. Butts reformulates the central point as follows:

> For any x, y and t, if x is a perfect material sphere and y is a perfect material plane, and t is a definite interval of time, and x and y remain perfect through t, then x and y touch one another in a single point when y is struck as a tangent of x.[5]

It is indeed a token statement of applied geometry, which describes a particular condition or situation of the world. Given that the antecedent can never be satisfied by actual solid objects, (he agrees with his predecessors on the point) the statement will always be a true counterfactual, because the 'fact' to which it is applied is not directly given in experience.[6]

What happens in the world,therefore, is what happens in geometry. This is Galileo's first move in the direction of achieving his target. Given this, what should one do in order to see mathematical order in the world? His answer is that one must deduct the material hindrances, or defalking the impediments of matter. This, according to Butts, is the second central theses of Galileo.[7] This consists in choosing only those characters that can be mathematically expressed and eliminating those characters that fall outside mathematical description.

> Just as the computer who wants his calculations to deal with sugar, silk, and wool must discount the boxes, bales, and other packings, so the mathematical scientist (*filosofo geometra*), when he wants to recognize in the concrete the effects which he has proved in the abstract, must deduct the material hindrances, and if he is able to do so, I assure you that things are in no less agreement than arithmetical computations.[8]

---

[4] *Dialogue* p. 207; our italics.
[5] Butts, *op.cit.* p. 73.
[6] *Ibid.*
[7] *Op.cit.* p. 74.
[8] *Two Systems*, p.207.

This is the condition of mathematization. In other words deducting material hindrances would mean creating a set of ideal conditions such that abstract effects can be actualized in the concrete world. This must be the real reason for experiment in science. We shall return to this a little later.

This point can be appreciated in relation to the other very important distinction Galileo introduced, which eventually became a very popular theme of philosophical speculation, namely *primary* and *secondary qualities*. One might think that this distinction is necessary for accomplishing Galileo's program. But we will show below that this distinction does not play the said role of finding out mathematizable properties, and therefore has no methodological significance. The two famous passages from the *The Assayer* clearly indicating the distinction are as follows:

> Now I say that whenever I conceive any *material or corporeal substance*, I immediately feel the need to think of it as bounded, and as having this or that shape; as being large or small in relation to other things, and in some specific place at any given time; as being in motion or at rest; as touching or not touching some other body; and as being one in number, or few, or many. From these conditions I cannot separate such a substance by any stretch of my imagination. But that it must be white or red, bitter or sweet, noisy or silent, and of sweet or foul odour, my mind does not feel compelled to bring in as necessary accompaniments. Without the senses as our guides, reason or imagination unaided would probably never arrive at qualities like these. Hence I think that tastes, odours, colors, and so on are no more than mere names as far as the object in which we place them is concerned, and that they reside only in the consciousness. Hence if the living creature were removed, all these qualities would be wiped away and annihilated. But since we have imposed upon them special names, distinct from those of the other and real qualities mentioned previously, we wish to believe that they really exist as actually different from those.[9]
>
> To excite in us tastes, odours, and sounds I believe that nothing is required in external bodies except shapes, numbers, and slow or rapid movements. I think that if ears, tongues, and noses were removed, shapes and numbers and motions would remain, but not odours or tastes or sounds. The latter, I believe, are nothing more than names when separated from living beings, just as tickling and titillation are nothing but names in the absence of such things as noses and armpits.[10]

Of the two lists Galileo gives the former is the list of primary qualities, while the latter is of the secondary qualities. The qualities included under the head of primary qualities is very revealing of the non-Platonic position of Galileo. To be in space and time, and being in

[9] Galileo Galilei, *The Assayer*, 1623, Quoted in Stillman Drake 1957, (Translated and Edited) *Discoveries and Opinions of Galileo*, p. 274.

[10] *Ibid.*, pp. 276-77.

motion are considered primary qualities and the objects of scientific knowledge. Let us recall that for Plato the objects of *episteme* are Forms, which are not located in any space, are eternal, and since they are Beings, they do not become, so no change and no motion can be attributed to them. It should also be noted that the primary qualities are about corporeal and not incorporeal 'substance', and therefore there is no doubt that Galileo is quite unlike Plato.

Secondary qualities (like tastes, odours etc.) are not in bodies which do have certain other qualities called primary qualities (like shapes, number, motion etc.) to excite in us the experience of the former. Primary qualities are considered to be some sort of causes impinging in us the sensations. What is given to us in our consciousness is therefore considered as effects due to the senses and what is not immediately (directly) given to us are the independent things of the world, because Galileo says shapes, numbers and motions would remain even if our senses were removed.

As far as Galileo's program of mathematizing the real world is concerned, primary qualities are indispensable, and secondary qualities dispensable. However, this relationship between primary qualities and mathematizable qualities is unwarranted. When Galileo talks of deducting the material hindrances, one may say, he certainly has in mind the secondary qualities. Let us look at Galileo's analogy. (See the quotation above.) For the purpose of determining the amount of sugar in a warehouse a clerk neglects (deducts) the contingent facts, such as the sugar is in bags, or in boxes, or in open containers and so on. If he wants to measure the weight, size, shape etc. of the container, though primary, are to be deducted. Is this 'deduction' based on any water tight compartmentalization of primary and secondary qualities or does it depend on any other factor? If one wants to measure some thing by volume, some other factors should be eliminated than those mentioned above, and if one wants to consider the geometrical forms, both volume, weight, along with others become eliminable. Therefore in the process of applying mathematics to the world, the principle of deducting material hindrances can be employed only as a way of approaching the measurable, and *what gets deducted depends on what quality one desires to measure*. Galileo cannot be right if he says that only primary qualities are measurable. Butts also criticizes him for grouping all sensory qualities as secondary, and therefore not measurable and also for holding that secondary qualities are not *in* the object. He argues that Galileo can be right that motions are the *cause* of heat, and still be wrong that the heat in no sense exists *in* the object, e.g., the boiling water. Certainly the thermometer measures *something*, and it is not a something that exists merely as a potentiality to produce a sensation of heat in a perceiver.[11]

We therefore think that the distinction between primary and secondary qualities is not necessary for finding measurable qualities, and therefore for mathematization. It is incorrect, therefore, to confuse material hindrances with secondary or sensory qualities. The above example shows that even relational qualities like mass, volume etc., which are clearly primary qualities according to Galilean criteria, can also become hindrances if what we want to measure is say shape or number or something else. Therefore we conclude that this distinction has no methodological significance. What is significant for the program of idealization is deducting hindrances, not necessarily *material* hindrances. What counts as a hindrance cannot be stated in certain terms.

In this connection it is important to consider another distinction that Galileo makes between extensive and intensive modes of knowing.

> [H]uman understanding can be taken in two modes, the *intensive* or the *extensive*. *Extensively*, that is with regard to the multitude of intelligibles, which are infinite, the human understanding is as nothing even if it understands a thousand propositions; for a thousand in relation to infinity is zero. But taking man's understanding *intensively*, in so far as this term denotes understanding some propositions perfectly, I say that the human intellect does understand some of them perfectly, and thus in these it has as much absolute certainty as Nature itself has. Of such are the mathematical sciences alone; that is, geometry and arithmetic, in which the Divine intellect indeed knows infinitely more propositions, since it knows all. But with regard to those few which the human intellect does understand, I believe that its knowledge equals the Divine in objective certainty, for here it succeeds in understanding necessity, beyond which there can be no greater sureness.[12]

Undoubtedly such pronouncements must have played a very fundamental role in Galilean days, when humanism was on the rise. The message is clear: Human beings can know Nature as perfectly as God. This would make a clearly different kind of response to the Sophists' challenge that we have discussed above, and is quite non-Platonic. Extensively we may never be able to exhaust all the variety of nature. Since extensive knowledge is based on non-mathematical qualities, and if each such quality refers to some essence of a thing then there are as many essences as there are qualities. Since there is no limit to kinds of things, complete knowledge of them is impossible. To this extent the Sophists should have agreed with him. But the intensive knowledge of mathematical objects is possible. Plato's response looks similar to Galileo in the sense that both of them thought that true knowledge is about mathematical objects. However, as already mentioned above, Galileo's mathematical objects

[11]Butts *op.cit.,* p. 67.
[12]Galileo, 1632, p. 103, Italics original.

are different from Plato's. But it should be remembered that Galileo is not here responding directly to the Sophists; he is arguing against the Aristotelians who believed that *episteme* is about these innumerable essences, and that the knowledge of them is possible. This is a clear departure from Aristotelianism. If these observations are correct they should indicate sufficiently that Galileo is neither Platonic nor Aristotelian, but is original in many ways.

Galileo's opposition to Aristotelian essences becomes more clear in his letter to the Jesuit mathematician, who denied that the sun-spots could be on the sun itself, for as the most luminous of bodies the sun could not generate its opposite, darkness. Galileo bursts out at him - as though things and essences existed for the sake of the name, not the names for the sake of the things. He writes that he does not find any advantage in understanding the essences of substances.

> If I ask about the substance of the clouds, I am answered, they consist of a damp mist; if I wish to know further what this mist is, so I am taught perchange that it is water rarefied through the force of warmth. If I remain in my doubt and wish to know what water really is, in all my investigations I will only learn in the end that it is that fluid which runs in streams and which we continually touch and taste: a knowledge which to be sure enriches our sense perception, but *leads us no further into the interior of things* than the notion I had of clouds to begin with.[13]

Our knowledge of nearby objects is not more than that of distant objects like the moon and the sun. But with respect to intensive knowledge, our knowledge of the celestial objects is better than that of nearby objects.

> For do we not know the periods of the planets' revolutions better than the different tides of the sea? Have we not grasped the spherical form of the moon much sooner and more easily than that of the earth?[14]

Having denied importance to the *extensive* mode of knowing Galileo chose the *intensive* mode of knowing.

The objects of knowledge of this intensive mode are *relational forms* of things, "their position, their motion, their form and size" etc., and are therefore mathematizable or measurable. Characterization based on certain relational qualities has "absolute certainty as Nature itself has". We are reminded of Aristotle's desire to know things as clearly as they are known *by nature*. However, as we just observed, they differ on the issue of what are the objects that are known *by nature*. Aristotle thought we can know the essence of

[13] *Lettere intorno alle macchie solari*, in *Opere*, Ed, Alberi, III, 462ff. Quoted in Randall Jr. 1962, *Career of Philosophy: From the Middle Ages to the Enlightenment*.

[14] *Ibid.*.

things, which is his object of knowledge, i.e., by extensive knowledge, while Galileo thought that we can understand Nature's language better by intensive knowledge. The objects of scientific knowledge have clearly undergone a transformation. Succinctly we may say that the nature of transformation with respect to Aristotle is from *qualitative episteme* to *quantitative episteme*, and with regard to Plato it is from *absolute Forms* to *relational Forms*, which includes *dynamic and static relational forms*.

According to Galileo's version of the joint method of analysis and synthesis, the scientist begins with a hypothetical assumption. The hypothesis does not come *immediately* from observation and the measurement of facts, but rather from an *analysis of the mathematical relations* involved in a given problem. Only after the mathematical relations involved in the initial hypothesis have been demonstrated by the method of composition, does it possess a quantitative meaning and implication that it can be compared and measured with observations and experiments.[15] What is involved in mathematical analysis? Galileo illustrates the method of mathematical analysis, thus:

> When . . . I observe a stone initially at rest falling from an elevated position and continually acquiring new increments of velocity, why should I not believe that such increase takes place in a manner which is exceedingly simple and rather obvious to every one? If now we examine the matter carefully we find no addition or increment more simple than that which repeats itself always in the same manner. This we readily understand when we consider the intimate relationship between time and motion; for just as uniformity of motion is defined and conceived through equal times and equal spaces (thus we call motion uniform when equal distances are traversed during equal time-intervals), so also we may, in a similar manner, through equal time-intervals, conceive additions of velocity as taking place without complication, thus we may picture to our mind a motion as uniformly and continuously accelerated when during any equal intervals of time whatever, equal increments of velocity are given to it.[16]

The mathematical analysis of the problem first consists in understanding "the intimate relationship between time and motion". Then, motion is "defined and conceived through *equal times* and *equal spaces*" arriving at the definition of uniform velocity: a motion is uniform when equal distances are traversed during equal time intervals. One might ask 'Why define uniform motion?'. It could not have been because Galileo thought motion is always uniform. But because uniform motion is a *simple* kind of motion, which can be defined and experimentally realized for empirical study. Similarly, i.e., in the same simple manner, he defines uniform acceleration. Thus acceleration and velocity have a specific definition, and as a result

[15] Cf. Randall Jr. 1962, p. 348.
[16] *Two New Sciences*, Crew and de Salvio, p. 161.

a specific meaning. Then having obtained the definitions, he postulates hypothetically the law of free fall, which is a *statement* that *asserts* that the distance increases proportionally to the square of time. For this he gives a plausibility argument that "we find no addition or increment more simple than that which repeats itself always in the same manner". Why square of time, why not simple proportionality? Galileo did not arrive at this without false starts. In the initial stages he never analyzed the matter in terms of acceleration. Since the details are presented in the case study, we shall postpone further discussion till the substantial details are also available. It is sufficient to observe here that there were many false starts before he could finally arrive at the law.

Thus the most important phase in the context of discovery is first to have clear and precise definitions of measurable (mathematical) parameters of a phenomenon, such as velocity and acceleration, in terms of certain other measurable parameters, such as space and time. This phase is the initial mathematical analysis, followed by an hypothetical assumption. Based on definitions and assumptions certain theorems (consequences) are proved to demonstrate the internal coherence. Galileo spends a lot of time in his later works proving a number of theorems, explicating the semantic content of the assumptions and definitions. This is the complementary mathematical synthesis. Then Galileo proposes that the hypothesis be verified by experimental observations.

After completing the method of mathematical resolution and composition, which is based on definitions, the mathematically demonstrated hypothesis can now be compared and measured with observations and experiments.

> If experience shows that such properties as we have deduced find confirmation in the free fall of natural bodies, we can without danger of error assert that the concrete motion of falling is identical with that which we have defined and assumed; if this is not the case, our proofs still lose nothing of their power and conclusiveness, as they were intended to hold only for our assumptions – just as little as the propositions of Archimedes about spirals are affected by the fact that no body is to be found in nature that possesses a spiral motion.[17]

Here Galileo is more than clear that the *theoretical* analysis based on definitions and assumptions (hypotheses) has its own value, whether we actually find them in reality. Here lies the significance of mathematical physics *per se*. If we can find the mathematical objects, properties of which are well known in the concrete world, then and only then we "can without danger of error assert" that the world is as we have defined and assumed in the definitions and assumptions. Even if we cannot find the counterparts of such theoretical objects in the

[17] Letter to Carcaville, 1637; *Opere*, Ed. Alberi, VII, 156; Quoted in Randall Jr. *op.cit.* p. 348.

actual concrete world, the knowledge of the defined object would not be entirely worthless. The demonstration, therefore, will be *valid* whether or not an application is found. However, Galileo is not for complete theoretical research without caring to verify it empirically.

Why is Galileo introducing a new experimental method, apart from the mathematical method? Why is it that only experimental observation and not mere observation can demonstrate mathematical hypotheses? This is because it is only in an experimental situation, which tries to mimic ideal conditions, that idealized mathematical propositions can hold, however approximately. This is to create an 'environment' where the material hindrances are deducted. The affine manner in which the mathematical objects are constructed and demonstrated cannot be obtained in the world of 'open' experience. Therefore we need to construct a 'closed' experimental world free from material hindrances. Thus Galileo felt the simultaneous need of both mathematical and experimental methods of scientific investigations.

This picture of Galileo's methodology appears to have affinities with the hypothetico-deductive methodology proposed in the beginning of the twentieth century by Popper, Hempel etc. However in Galileo's method definitions are given more fundamental status than hypotheses, for the latter are formed on the basis of pre-constructed definitions. Thus the origin of hypotheses has a clear basis, unlike in Popper's view where any basis is denied. The problem however still persists, because it is not clear how one would construct definitions. We discuss the role of inversion in constructing definitions in Chapter 6. Though Galileo is not entirely explicit, he gives clear clues about how he constructs them, after which he postulates hypotheses. Here he would make use of the idea of inversion, and therefore we will postpone the details to a latter part of the thesis. We are content here to state that Galileo too believed in the methodological theme of analysis and synthesis.

We have observed in the beginning of this section that Galileo is linking two methods together: the Euclidian method of analysis and synthesis and the Aristotelian method of resolution and composition. Randall Jr. (1940) argued that Galileo is influenced by the Aristotelians of the school of Padua. Gilbert (1963) argued in response to Randall's thesis that he is influenced by the Greek mathematicians. We think that both these claims are true. We have observed above that he differs with both Plato and Aristotle in a significant manner. The Greek mathematicians on the other hand clearly influenced Galileo, but they were not concerned with the philosophical problems for supporting either mathematical or experimental physics. The Aristotelian influence is also clear in his desire to solve specific problems. We should therefore understand him as a great blending character. Peter Machamer (1978) correctly observes that Galileo belongs to a tradition of *mixed sciences*.

> The tradition is that of the mixed sciences, which is itself a tradition blending mathematics and physics (or natural philosophy), blending Platonic (or neo-Platonic) and Aristotelian elements, blending reason and observation.[18]

Galileo's method has another feature that requires special mention, and this is also a major point of difference between Galileo and Descartes. Galileo is not only interested in pure mathematical mechanics *per se*. He is interested in those principles that are exemplified in nature. For that he fixes his subject matter by defining the natural phenomena to be studied.

> And first of all it seems desirable *to find and explain a definition best fitting natural phenomena*. For anyone may invent an arbitrary type of motion, and discuss its properties; thus for instance some have imagined helices and conchoids, as described by certain motions which are not met with in nature, and have very commendably established the properties which those curves possess in virtue of their definitions; but we have decided to consider the phenomena of bodies falling with an acceleration such as *actually occurs in nature* and to make this definition of accelerated motion exhibit the *essential features* of observed accelerated motions.[19]

The first remark clearly suggest that he is not inclined to do pure mathematics like the Greek mathematicians. Here Galileo is clearly referring to Archimedes, whose work on helices and conchoids in geometry is well known. This should not be taken to mean that Galileo is against pure mathematics, but he is appealing to complement definitional knowledge by applying it to the actually occurring phenomena. Galileo's ultimate interest is to define natural phenomena in analogous terms with mathematical objects. His attempt is to apply mathematics in the world of natural phenomena. These remarks suggest that though Galileo is following the Greek mathematicians, he followed them with a difference. And this difference, we think, consists in Galileo's interest in *local* problems. Let us recollect that Aristotle also thought that fixing the subject matter is a crucial feature of natural science, contrary to the Platonic idea of Universal science. Descartes is a Platonist on this issue, while Galileo is not. Both Descartes and Plato, it well known, are great system builders. They believed and attempted to systematize science in a architectonic manner. Descartes thought that Galileo's approach was *piecemeal*; he wanted to construct science not from merely plausible hypotheses but from indubitable clear and distinct first principles as foundations. He accuses Galileo of having built mechanics without foundation.

> I find that in general he philosophizes much better than the usual lot for he leaves as much as possible the errors of the School and strives to examine physical

[18]Peter Machamer in Butts and Pitts *op.cit.* p. 161.
[19]*Two New Sciences*, Crew and de Salvio, p. 160.

> matters with mathematical reasons. In this I am completely in agreement with him and I hold that there is no other way of finding the truth. But I see a serious deficiency in his constant digressions and his failure to stop and explain a question fully. This shows that he has not examined them in order and that, without considering the first causes of nature, he has merely looked for the causes of some particular effects, and so has built without any foundation.[20]

Descartes appreciates Galileo's inclination to mathematics, but demands greater rigor, for Galileo did not, as Descartes thought, "stop and explain a question fully." While it is true that Galileo did not *stay forever* in the mathematical world, he cannot be accused for not having answered or explained a question fully. Insofar as specific contributions towards the science of motion are concerned, Galileo succeeded better as compared with Descartes. Descartes' contributions to mathematical analysis are undoubtedly more sophisticated than Galileo's, but it is not legitimate to accuse Galileo for his inclination to solve specific problems. One of the characteristic features of modern science, that comes out clearly in the studies of T. Kuhn also, is that it develops by attempting to solve local problems. Descartes, we think, has failed to see the significance of solving 'petty' problems.

Towards the end of the above passage, Descartes criticizes Galileo because the latter looked always "for the causes of some particular effects" without any foundation. Here also Descartes' understanding of Galileo has to be questioned. Because for Galileo, the cause-effect relation is not central, as it is in Aristotle's physics. He is interested in the mathematical relationship between the relevant measurable parameters of the phenomena under study. Traditionally there has been too much emphasis on the cause and effect relation in the philosophical accounts of science, as is evident from the writings of Aristotelians. We will see in Part-III that the distinction between cause and effect is not central to the Galilean approach. It is rather well known that Galileo did not so much look for causes of motion, but emphasized mathematical (functional) relationships between different measurable parameters. Though, later Newton returns to the question of causes of motion, his notion is functionally defined, unlike Aristotle's notion of cause and effect, which none in the 17th century accepted. In a functionally defined causal relation, the cause and effect can be interchanged, or reversed. We will see below that this reversibility is due to the invertible relation or symmetry of most mathematical relations. It is well known that the theoretical knowledge of science has a meta-theoretical property called symmetry. If theoretical knowledge had been grounded on Aristotle's notion of cause and effect which is necessarily asymmetrical, mathematical physics would not have been possible.

[20] Letter to Mersenne, October 1638, A.T., II, 380. Quoted in Shea, W.R. 1978.

Before we look at Descartes joint method, we shall summarize the above discussion. Galileo defined new objects of scientific knowledge as relational properties of measurable dimensions—another possible response to the Sophists' challenge—and accordingly devised a new joint method, which we have characterized as *mixed*, for it contained both mathematical and experimental components. He differed significantly from both Platonic and Aristotelian thought, and also in a subtle manner from the Greek mathematicians—his thought is unique and original. His main problem was to find applications of mathematical knowledge to natural phenomena.

2.2 Descartes

Galileo's contribution, as observed above, was in convincing people that physical nature can be quantified, and in making the mathematization of science possible. In that process he argued for the need of idealization and experimentation for understanding and validating scientific knowledge. The counterfactual nature of scientific conceptions and the need of not only physical experiments, but also thought experiments has been brought to light in his deliberations. Descartes too was not only convinced that physical nature can be quantified, but actually *identified* mathematical (geometrical) dimensions with the physical.

> [I]t is not merely the case that length, breadth, and depth are dimensions, but weight also is a dimension in terms of which the heaviness of objects is estimated. So, too, velocity is a dimension of motion, and there are an infinite number of similar instances."[21]

However, Descartes allowed some distinctions in relating them to actuality and possibility—physics is to actuality and mathematics is to possibility.

> The difference consists just in this, that physics considers its object not only as a true and real being, but as actually existing as such, while mathematics considers it merely as possible, and as something which does not actually exist in space, but could do so.[22]

Physics, then, becomes applied (actualized) mathematics. This development has far reaching implications for the advancement of modern science. In ancient times multiplication of dimensions other than geometric or arithmetic are thought to be impossible.[23] Unless the dimension of, say, mass is multiplied with the dimension of motion (velocity) no quantification

[21] Quoted in Mason 1956, *Main Currents of Scientific Thought: A History of the Sciences* p. 132.

[22] *Conversation with Burman* (V 160, C p.23), quoted in Bernard Williams 1978, *Descartes: The Project of Pure Inquiry* p. 259.

[23] Bochner 1966, *The Role of Mathematics in the Rise of Science*,.

of motion could be achieved in terms other than merely saying that something moves faster than some other thing. Development of physics without allowing the *functional correlation* or *covariation* (read multiplication) of geometrical dimensions and physical dimensions can be stated to be impossible. Thus the subject matter of physics and mathematics have found a common ground, such that they could develop, henceforth, dialectically, if not hand in hand. Anyone familiar with the development of both mathematics and modern physics after the 17th century, would not deny that neither mathematics nor physics could have developed independent of each other. The foundational contribution of Descartes is extremely relevant for enforcing such a development of both the fields. Since the study of such a development is a subject in itself, we shall not divert our attention to that here. It is sufficient to observe here that Descartes' contributions in working out a common framework for mathematical physics have been more fundamental than that of Galileo. However, when one looks at the comparative abilities of finding applications of mathematical knowledge in solving concrete problems Galileo's success is more commendable than Descartes. Modern science could not afford to miss either of them.

Descartes also proposes a joint method of Analysis and Synthesis, which is clearly conceived as a method of discovering and ordering knowledge. In *Regulae* he proposes rules for the direction of the mind. His rules IV, V and VI are as follows: Rule IV: There is a need of method for finding out the truth.

> Rule V: Method consists entirely in the order and disposition of the objects towards which our mental vision must be directed if we would find out any truth. We shall comply with it exactly if *we reduce involved and obscure propositions step by step to those that are simpler, and then starting with the intuitive apprehension of all those that are absolutely simple, attempt to ascend to the knowledge of all others by precisely similar steps*.[24]
>
> Rule VI: In order to separate out what is quite simple from what is complex, and to arrange these matters methodically, we ought, in the case of every series in which we have deduced certain facts the one from the other, to notice which fact is simple, and to mark the interval, greater, less, or equal, which separates all the others from this.

Rule V is a clear statement of the joint method of analysis and synthesis. However, we see that relational knowledge of things is what is sought, and not Aristotelian essences. The ultimate goal or aim of the analytic regression, as is clear from Rule VI, is not the simple *qua* simple but the simple 'relatively' to the other terms of the series. Also notice that the 'series' does not imply that we are to consider that things or facts can be arranged in a conceptual

[24] Our italics.

classification similar to that adopted by the Aristotelians. The series is not a static ontological classification based on genus and specific difference but an implicatory sequence of antecedent and consequent in which the important and decisive factor is the logical relation of one to the other.[25] Also to be noted is the use of the term 'propositions', and not classes.

Rule VI says that in order to know what is simple and complex, we should arrange terms in relative and absolute order. Descartes defines an absolute term as one which contains within itself the pure and simple of which we are in quest. Examples of such terms are independence, cause, simple, universal, one, equal, straight and so on. Relative terms on the other hand are those which are 'related' to the absolute and deducing them involves something other than the absolute concepts. Examples of such terms are what ever is considered as dependent, effect, composite, particular, and so on. Note that the terms in the independent category includes basically primary mathematical terms, and in the dependent category includes the secondary non-mathematical terms. Thus the method, couched in terms of analysis and synthesis, tends toward mathematical objects of knowledge, which is about divisions, shapes and motions.

The method of analysis ultimately reduces the problem by a regressive and gradual division until we reach a term which is *maxime absolutum*. From the discovery of the *maxime absolutum* the method of synthesis can begin, which is the arrangement of the facts discovered by analysis, in such an order that they will be successively relative and more concrete terms of the implicatory series will issue as the solution of the problem.[26].

Thus Descartes' program is to interpret nature in the form of an axiomatic structure of the whole system, by establishing indubitable foundations and the deducing from them the rest of the phenomena. Following such a maxim he tried to construct a system, which is purely mechanical in character, i.e. it employs no principle other than the concepts employed in mechanics, such as shape, size, quantity, motion etc.

Gradually Descartes realized how difficult was the program he visualized. Later he not only diluted the rigid architectonic approach of deducing everything from first principles, he allowed room for hypothetical premisses that are compatible with the first principles in his system. This point comes out vividly in the study of Larry Laudan (1981), who writes that:

> After trying to deduce the particular characteristic of chemical change from his first principles (i.e., matter and motion), he concedes failure. His program for the derivation of the phenomena of chemistry and physics from *a priori* truths remains

[25] Beck, L.J. 1952, p. 161.
[26] *Ibid.,* pp. 167–78

> uncompleted. His first principles are, he admits, simply too general to permit him to deduce statements from them about the specific way particular chunks of matter behave. ... Not content to leave anything unexplained, Descartes departed from his usual devotion to clear and distinct ideas and advocated the use of intermediate theories (less general than the first principles, but more general than the phenomena), which were sufficiently explicit to permit the explanation of individual events and which were, at the same time, *compatible with, but not deducible from, the first principles.* Descartes recognized that all such intermediary theories were inevitably hypothetical. Because their constituent elements were not clearly and distinctly perceived, it was conceivable that they were false. After all, nature is describable in a wide variety of ways and the fact the an explanation worked was no proof that it was true. Like any good logician, Descartes realized that "one may deduce some very true and certain conclusions from suppositions that are false or uncertain".[27]

This development in Descartes turns out to be highly significant for understanding the role of the method of hypothesis in the later developments of science. This moderately modified stand also brings Galileo and Descartes closer than before. In the earlier section we have noted why Marsenne in his letter to Descartes was critical of Galileo. Whatever be the significance of this later realization in the context of the development of the hypothetico-deductive methodology, as Laudan tries to stress, the significance of this in the development of problem oriented (paradigmatic) science, as opposed to architectonic science, should also be noted.

2.3 Newton

Galileo's second important successor Newton was closer to him in the sense that he is also a member of the *mixed tradition*. He tried to keep a proper balance between an unlimited confidence in mathematics unchecked by experience, and mere experimenting unaccompanied by mathematical analysis and demonstration.[28] His statements on method, therefore, sounded much like Galileo. He gave his method more experimental coloring than Galileo had done, for the latter did not feel the need to check by observation mathematically deduced consequences. For Newton the logical inclusion of a proposition within a deductive system was not a sufficient proof of its 'truth'. As rightly pointed out by Randall, the experimental analysis of instances in nature forms a part not only of the method of discovery but also of the verification.[29]

[27] Laudan 1981, p. 29. The quotation in the last sentence is from: R. Descartes, *Oeuvres* (ed. Adam and Tannery), Paris, 1897-1957, vol.2, p. 199. Italics are original.

[28] Randall Jr.1962 *op.cit.* p. 576.

[29] *Ibid.*

In the *Opticks* appears Newton's classic statement of the joint method of analysis and synthesis, with its experimental fervor.

> As in mathematics, so in natural philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing several conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments, or other certain truths, for hypotheses are not to be regarded in experimental philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much stronger, by how much the induction is more general. And if no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception shall occur from experiments, it may then begin to be pronounced from compounds to ingredients, and from motions to the forces producing them; and in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general. This is the method of analysis: and the synthesis consists in assuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations.[30]

It may be noted that the term 'analysis' is used to refer to the experimental and empirical context, unlike the modern usage of the term to the logical and deductive context. Accordingly the term 'synthesis' refers to deductive proof. The terms are used to refer to the same contexts as in the Aristotelians of the School of Padua at Italy, as elaborated above. This terminological inversion, as indicated above, must be due to the later linguistic orientation of philosophers, specially after Kant. It is typical, for Aristotelians, to consider the effects or phenomena as complex, therefore to be analyzed until they reach the causes, which are regarded as simple. The later modern philosophers use the term 'analysis' mostly to denote the logical movement from the more general statements to the more specific statements, while inductive movement from specific to general statements is regarded as synthetic. This inversion of terms demands historico-philosophical explanation. Again, we are afraid, we cannot meet the demand here, but must remain content with the observation.

The events mentioned in the method of synthesis, though include induction, are not mere simple unidirectional inductive movements. But it is characterized as dialectical, i.e., checking errors and collecting instances, ultimately arriving at the general. It is the well known view of Newton that in this context hypotheses should not be brought in. So much has been written, which is ridden with confusion regarding Newton's cryptic views on the role of

[30] *Opticks*, p. 380.

hypotheses, we shall not add anymore to it. However, it should be noted, that it is typical of the scholars of that period to believe in only those postulates that are 'deducible' from given experience. If Descartes allowed in the last resort some room for hypotheses, it is not because it is desirable to have them, but because we have nothing better than them. However the difference between Newton and Descartes should be noted. Newton wanted that the principles be 'induced' experimentally, while Descartes' earlier program was to deduce them from the clear and distinct principles. Thus the nature of the kind of reason they have envisaged is qualitatively different. Now for Galileo, as elaborated above, the first step was to construct the definitions, and then the hypotheses. Considering the deficiencies of both inductive and hypothetico-deductive methodologies that developed, it is Galileo's position that needs to be reconsidered. In the view that we are going to defend, constructing definitions will be considered the first step in the context of discovery.

Chapter 3

The Rise of Consequentialism

3.1 New Objects of Scientific Knowledge

It is usual to contrast Bacon with Descartes, the former being seen as an empiricist, and the latter as a rationalist. Bacon's name has become synonymous with inductivism, and has met with much criticism from various quarters. A quotation from Jevons would tell how Bacon has come to be regarded:

> The value of this method [Bacon's] may be estimated historically by the fact that it has not been followed by any of the great masters of science. Whether we look to Galileo, who preceded Bacon, to Gilbert, his contemporary, or to Newton, Descartes, Leibniz and Huyghens, his successors, we find that discovery was achieved by the opposite method to that advocated by Bacon.[1]

Bacon was very popular with the English scholars, even among those who took mathematics very seriously, such as Newton. Though Bacon opposed decadent scholasticism and barren belief in the authority of science, he continued to believe in the Aristotelian objects of knowledge, which consists in qualitative understanding of the nature or essence of things. Ideas about the nature of science that followed after Bacon, however, did not entertain Aristotelian objects of science, but undertook to probe for invariant antecedents.

In fact much before Galileo, the new objects of knowledge were developed in the school of Alexandria by Archimedes, but it took many centuries to apply similar methods to other physical problems such as motion. Some explanation as to why Archimedean methods did not take off immediately has been attempted in the case studies. Here we find it necessary to clarify the nature of knowledge that developed after Bacon, which was already available in the works of Archimedes. This partially explains why Bacon's inductive method failed.

[1] Jevons, p. 507.

Take Archimedes' law of the lever: When a two armed-lever is in equilibrium, the attached weights are inversely proportional to their respective distances from the 'fulcrum'. Thus, if one side of the lever is ten times as long as the other, a weight attached to that side will balance another weight ten times as heavy when placed on the other side of the lever. Here no reference is being made to "inherent qualities" or Aristotelian essences to describe the *system*. Neither is there any talk of any metaphysical 'force'.

> It [The law] expresses a mutual dependency of quantities and nothing more. Even the distinction between independent and dependent variables is obliterated, and the relationship which the law defines is completely reversible. In other words, the law expresses a type of dependency for which the mathematical notion of "function" has furnished the pattern.[2]

It should be noted that the pattern of relationship is symmetrical, because of which it is reversible. In fact we cannot say, except arbitrarily, which is the cause and which is the effect. What emerges here is an *invariant, symmetric, relational, and functional form*. This, we claim, is the character of the objects of scientific knowledge of not only Archimedean science, but also the science whose development we continue to watch even today. More examples of this pattern will be presented in the case studies.

Another example from Newton again suggested by Werkmeister in the same context, is equally telling. The form that emerges from Newton's law of gravitation expresses mutual dependency of two masses each one attracting the other. For example, in the case of a falling stone, the earth attracts the stone, and also the stone attracts the earth.

> Gravitation cannot even be defined without reference to at least two bodies. The attractive "force" is in every case proportional to the masses of the bodies and inversely proportional to the square of their distances. If this means anything at all, it must mean that the "force" of gravitation is not "inherent" in any one thing, but is essentially a relation between things. The "immanent" forces of metaphysics disappear, and there is left only mathematical proportionality.[3]

Therefore the new objects of scientific knowledge are based on relational invariance, and is undoubtedly non-Aristotelian. This knowledge is necessarily not obtained by Baconian inductive methods, for it involves creative abstraction. The role of abstraction is mostly in creating an affine space in which mathematical knowledge can find application and where induction has no place. It is in this context, we claim that inversion plays its crucial role

[2] Werkmeister, W. H. 1940, p. 40. Werkmeister makes these observations in the context of explicating the functional notion of force that Kepler and latter scientific tradition adapted. We are using his observations for the general objects of knowledge that science has adapted ever since.

[3] *Ibid.*

and induction fails miserably. If there is one singular achievement of philosophical reflection on scientific method so far, it is, we think, the limitation of the inductive method in understanding scientific knowledge.

The Baconian method would have worked if the objects of knowledge were Aristotelian, but since the new objects of science contained functional relations inductive method failed. Thus if Bacon's methods did not find application in science it is due to the new face that science took after Bacon.

The nature of the change, as we understand it, consists in realizing newer objects of knowledge that are not solely based on the thematic division of universals and particulars. We have noted above that for both Plato and Aristotle *episteme* constitutes the knowledge of the universals. We have also observed that the 'discovery' of universals can be understood as a requirement to meet the Sophists' challenge. Now, the new object of scientific knowledge is not merely a relation between universals, but between two measurable parameters of a physical phenomenon. Before Plato, and even after him, only unchanging objects were thought to be measurable, and mathematics was conceived as a science of such objects alone. Even the Archimedean science (statics) had this limitation of not being able to mathematize a physical phenomenon that has the character of necessary change, such as motion. However, after Galileo, it is realized that even changing phenomena can be mathematically understood. Galileo demonstrated this possibility with epistemological and methodological support. The discovery of Galilean relativity is indeed the first outcome of the new forms of knowledge. The character of this new knowledge is to capture the *invariance of variable phenomena*. It is no longer statics. Dynamics is the hallmark of the new science.

Earlier, after Plato, the changing objects of knowledge were regarded as a threat to understand the world around. If it is possible to show that *change itself can have a pattern*, change becomes a knowable object. It is in this sense that this new development can be regarded as an answer to the problem of knowledge that the Sophists raised. This is how we interpret the nature of the transformation that took place in the 17th century revolution in science. We will argue below that this change is impossible without inverse reason. The claim, that this newer form of invariance is not solely based on the relation between universals and particulars will become clear in Part-II.

After this transformation in science the discussion on whether scientific methodology should be Baconian or Cartesian (empiricist or rationalist) continued for a long period. We cannot go into the details of the events that followed after the 17th century. However we find it necessary to discuss the general nature of the interesting and highly significant changes that took place after the 17th century. Since we are not in agreement with the historico-philosophical observations of Karl Popper, Imre Lakatos, and Larry Laudan on the history of methodology after 17th century, we shall present below a critical discussion, which will also contextualize the problem of the thesis. The general theme of all the three philosophers is the rise of consequentialism, and the fall of infallibilism.

3.2 The Fall of Infallibilism

We shall start this section by considering certain historical remarks by noted philosophers of science, Popper, Lakatos, Laudan and others, specially concerning the period just outlined.

Karl Popper provides an interesting interpretation of the history of epistemology in his essay 'On the Sources of Knowledge and of Ignorance' (1962). According to him, an important and great movement of liberation started in the Renaissance which was inspired by unparalleled epistemological optimism. At the heart of this optimism lay the doctrine that *truth is manifest*. This movement was characterized by the rejection of authority.

> The birth of modern science and modern technology was inspired by this optimistic epistemology whose main spokesmen were Bacon and Descartes. They taught that there was no need for any man to appeal to authority in matters of truth because each man carried the sources of knowledge in himself; either in his power of sense perception which he may use for the careful observation of nature, or in his power of intellectual intuition which he may use to distinguish truth from falsehood by refusing to accept any idea which is not clearly and distinctly perceived by the intellect.[4]

Descartes' basis was the theory of the *veracitas dei*, the truthfulness of God. "What we clearly and distinctly see to be true must indeed be true; for otherwise God would be deceiving us. Thus the truthfulness of God must make truth manifest."

On the other hand Bacon's basis was the doctrine of the *veracitas naturae*, the truthfulness of Nature. "Nature is an open book. He who reads it with a pure mind cannot misread it. Only if his mind is poisoned by prejudice can he fall into error."[5] Thus Descartes and Bacon did not remove authority altogether, instead they replaced one authority, that of Aristotle and the Bible, by another. Bacon appealed to the authority of the senses, and Descartes to the authority of the intellect.[6]

[4]Popper 1962, p. 5.
[5]*Ibid.*, p. 7.
[6]*Ibid*, pp. 15-16.

While being highly critical of this optimistic epistemology, Popper acclaims it for having rejected textual authority.

> It encouraged men to think for themselves. It gave them hope that through knowledge they might free themselves and others from servitude and misery. It made modern science possible ... It is a case of a bad idea inspiring many good ones.[7]

Popper rejects both theses: neither observation nor reason can be authoritative or dependable *sources* of knowledge. However, this rejection has not led to the formulation of a third alternative source of knowledge. His point is that there exists no dependable source of knowledge. Briefly put, truth is *not* manifest.

Imre Lakatos makes similar observations about the history of epistemology. He thinks that there is a common feature of all the accounts of knowledge given until the period of Newton. This common feature is *infallibilism*. What Lakatos is trying to convey with 'infallibilism', has been conveyed by Popper using the expression 'the belief that truth is manifest'. Lakatos restates what Popper has said in terms of language oriented expressions. While empiricists believed in the truth of *factual statements*, intellectualists (rationalists) believed in the truth of *general statements* (first principles). He observes that Descartes, Newton and Leibniz all agreed that one can indubitably intuit truth and/or falsehood at *both points*; on the level of facts and on the level of first principles. He further makes the point that highlights the common theme shared by traditional epistemology, which is that neither factual statements nor first principles taken in isolation can be said to be true.

> They [first principles] are only respectable and suitable candidates for truth or falsehood if they are already embedded in the circulatory system of analysis-synthesis. Basic statement is meaningless outside analysis-synthesis.[8]

The truth of a proposition in an analytico-synthetic framework depends on how it is linked or related to the set of accepted or known beliefs. If it cannot be logically connected (whatever 'logical connection' may mean), the truth of that proposition is not secured. Commenting on this classical theme of analysis-synthesis, Lakatos says that the two methods link known and unknown together by a chain of deduction. When truth or falsehood is injected at some point of the analysis-synthesis circuit, it gets transmitted to every part of the circuit.[9]

[7] *Ibid*, p. 8.
[8] Lakatos 1978, *op.cit.* p. 77.
[9] *Ibid*, p. 76.

The general criticism of these traditional forms of analytico-synthetic methodology is that the latter component of the method becomes redundant. Because if there exists a definite source, or method, to arrive at true propositions, there remains no reason to reestablish their truth by another validating or proving method. Since at that time the propositions of science were considered infallible, nothing more can be achieved by returning to the starting point. That is, the process of proof does not give any extra epistemological warranty.

A few other interesting reasons for the untenability of the traditional epistemological framework have been 'excavated' by Larry Laudan. Laudan first makes the observation that soon after the 17th century the nature of science began to change necessitating a corresponding change in the scientific methodology. The change lies in the rise of the hypothetico-deductive methodology. He then gives an explanation as to why hypothetico-deductive methodology became "the ruling orthodoxy in the philosophy of science and the quasi-official methodology of the scientific community."[10] First, we shall summarize his position, followed by a discussion.

The method of hypothesis consists in validating an hypothesis by ascertaining the truth of all of its examined consequences. It was espoused in the middle of the 17th century by Descartes, Boyle, Hooke, Huygens, and the Port-Royal logicians. It fell in disfavor by the 1720s and the 1730s because of the fallacy of affirming the consequent.

> [M]ost scientists and epistemologists accepted the Baconian-Newtonian view that the only legitimate method for science was the gradual accumulation of general laws by slow and cautious inductive methods. Virtually every preface to major scientific works in this period included a condemnation of hypotheses and a panegyric for induction. Boerhaave, Musschenbroek, 'Gravesande, Keill, Pemberton, Voltaire, Maclaurin, Priestley, d'Alembert, Euler, and Maupertius were only a few of the natural philosophers who argued that science could proceed without hypotheses, and without need of that sort of experimental verification of consequences, which had been the hallmark of the hypothetical method since antiquity.[11]

The methods of inductive inference and analogical inference alone were considered capable of generating reliable knowledge. Later,

> the self-same method of hypothesis which was so widely condemned by 18th-century epistemologists and philosophers of science was, three generations later, to be resurrected and to displace the very method of induction which the philosophers and scientists of the Enlightenment had set such store by.[12]

[10]Laudan 1981, *Science and Hypothesis*, p. 1
[11]*Ibid*, p. 10.
[12]*Ibid.*

A number of methodologists of the 1830s and the 1840s such as Comte, Bernard, Herschel, Apelt, Whewell and Dugald Stewart acknowledged that the method of hypothesis was more central to scientific inquiry than induction, while Mill conceded that the method had a vital role without erasing the role of induction in scientific inquiry.

> This about-turn, which effectively constitutes the emergence of philosophy of science as we know it today, is clearly of great historical importance.[13]

Laudan's explanation runs as follows. While most scientists and methodologists were content with inductive generalizations from experimental data for the construction of Galilean or Newtonian mechanics, many other areas of inquiry, such as electricity, heat, organic and phlogiston chemistry, etc., did not readily lend themselves to such an approach.[14] It was soon realized that "the types of theories they were promulgating could not possibly be justified within the framework of an inductivist philosophy of science. Since there was no way to reconcile an inductivist methodology with such highly speculative theories about micro-structure, scientist-methodologists such as, George LeSage, David Hartley and Roger Boscovich, working in their respective areas, chose to develop an alternative epistemology and methodology of science, rather than abandon micro-theorizing. These views developed in the course of, and as a result of, epistemic criticisms directed against the speculative theories proposed by them. Further strength to this initiative was given by Jean Senebier, Pierre Prevost, Dugald Stewart, Herschel and Whewell.[15]

One commendable achievement of Laudan in tracing the historical development of the hypothetico-deductive method is that the method emerged out of the problems faced by the working scientists. *The source of modern methodology has not been pure the philosophical context.* Perhaps it has never been. Previous to this specialist era the distinction between working scientists and philosophers was difficult to make. Therefore, not only the modern methodology, but also the traditional methodology, with the exception of possibly Bacon, emerged out of the intellectual struggle of philosopher-scientists, rather than pure philosophers.

It was observed above that the change brought about in the objects of knowledge necessitated the development of appropriate methodologies. Never in the history of philosophy, was the question of methodology raised without a prior statement about what the objects of knowledge were. All the views elaborated above have had a specific perspective on what constitutes scientific knowledge. Therefore, if this observation is correct, the definite

[13] *Ibid,* p. 11.
[14] *Ibid,* p. 12.
[15] *Ibid.,* pp. 12–15.

role methodology could play would depend mostly on how well defined are the objects of scientific knowledge. If we have a clear taxonomy of the objects of knowledge, possibly we can also have a corresponding taxonomy of methodology. In the contemporary situation, however, nothing can be asserted with certainty about the possibility or impossibility of an exhaustive account of the nature/taxonomy/structure of scientific knowledge, and therefore no corresponding assertions are possible regarding the nature/taxonomy/structure of scientific methodology.

In the explanation given by Laudan, one reason for bringing in the method of hypothesis comes out very clearly, which is the nature of the *micro-sciences* as against the *macro-sciences*. Macro-sciences, according to Laudan, "deal with properties and processes which can be more or less *directly* observed and measured."[16] And micro-sciences deal with unobservable phenomena. Galilean mechanics, and Copernican astronomy are given as examples of macro-science, while optics, chemistry, physiology, meteorology, and pneumatics etc. are given as examples of micro-science. According to Laudan, the sciences that come under the macro-sciences are not the source of philosophical problem and are not responsible for the transformation that gave rise to hypothetico-deductive methodology.

Laudan argues that Alexandre Koyre's view, that Galilean mechanics posed a profound challenge to the Aristotelian empiricist epistemology, is incorrect.

> If we take Aristotelian epistemology to be summed up in the dictum "nothing is in the mind which was not first in the senses", there is little in Galileo's science of motion which *need* to be taken as challenging that epistemology. This is not to suggest, of course, that Galileo's own methodology was derivative from Aristotle's. Serious scholars continue to fight that one out. What is being claimed is that Galilean mechanics could be (and sometimes was) regarded as posing no acute threat to the theory of scientific methodology advocated (say) in Aristotle's *Posterior Analytics*. If the whole of 17th-century science had exhibited the largely phenomenological character of Galileo's mechanics, there need have been no revolution in methodology.[17]

The real threat, according to Laudan, is due to the micro-sciences, as mentioned above. He argues that although micro-sciences "address themselves to the observable phenomena, *the theories themselves postulated micro-entities which were regarded as unobservable in principle.*"[18] This feature of micro-sciences was philosophically disturbing due to "the *radical observational inaccessibility of the entities postulated by their theories*".[19]

[16] *Ibid,* p. 21.
[17] *Ibid,* p. 21.
[18] *Ibid,* p. 22.
[19] *Ibid.*

Laudan's linking of empirical epistemology with Galileo becomes very clear in the following passage.

> Earlier epistemologists of science Aristotle to Bacon had maintained that scientific theories could be elicited *from* nature by a careful and conscientious search for the "universals inherent in the particulars of sense". **Precisely because Galilean mechanics could be (and often was) regarded as a natural extrapolation from sensory particulars, it posed few problems for the traditional epistemology of science.**[20]

Thus he sees no threat to traditional methodology from Galilean science. This we shall argue is an incorrect view, while we agree with him that micro-sciences indeed threaten the traditional empiricist epistemology. First, let us recall from the above account that for Galileo the objects of scientific knowledge, as well as methodology are clearly different from both Aristotle's and Plato's. We have also remarked that Bacon's methodology can do good for Aristotelian objects of knowledge, but not for the Galilean. Therefore it is necessary to understand how a change in the objects of knowledge have not produced a corresponding change in methodology. In fact, it is one of the significant claims of Laudan that methodological choices are determined by the nature of science. A number of questions would naturally arise. If there existed no change in the nature of science then why have Galileo's contributions given rise to a revolution? And if there existed a change in the nature of science, then why couldn't that pose a threat to Aristotelian/Baconian methodology? Can the traditional Aristotelian methodology attend to the essentially mathematical objects of Galilean science? If there exist no problems in 'constructing' or 'reconstructing' Galilean science from experience, then why does the conceptual transformation from Aristotle to Galileo still constitute a major philosophical and methodological problem? Scholars still dispute over questions of the following kind: Who influenced Galileo, Plato or Aristotle? Is it the Italian Aristotelians or neo-Platonists that made the revolution possible? These questions cannot be satisfactorily tackled here, we can confidently claim that Galilean science is qualitatively distinct from Aristotelian and also Platonic science, and Aristotelian methodology cannot account for Galilean science.

We have seen above that Galilean science 'starts' with a suppositional (hypothetical) definition of a state of motion, namely uniform acceleration. It is not possible to arrive at such a theoretical definition from experience, as we never do find an object with uniform acceleration. It is an ideal and not a real state. One might say that the Baconian starting point is experimental experience and not ordinary sense experience, where uniform accelera-

[20] Italics are original, while boldface is ours. p. 23.

tion could be actually seen. How would one get the motivation to construct an experimental setup where uniform acceleration can be realized, without a prior definitional knowledge of what that 'state' (setup) would be? No experiment is ever conducted without some theoretical background and motivation. If Galileo really conducted experiments on inclined planes, pendulums, etc., the objective was not to arrive at the definitions, but to find confirmation for the mathematically deduced theorems from theoretically constructed definitions. Some one like Galileo, who gave a secondary role to experimental verification, can not be equated or seen as posing no threat to Baconian methodology. Recall what he says regarding experimental verification: experimental verification is to satisfy those who do not understand the mathematical subtleties. For an expert in mathematical reasoning the theorems are already proved mathematically.

It is also not true that Galilean science posits no 'objects' which are inprinciple unobservable. Without a notion of vacuum or void, which is undoubtedly unobservable inprinciple, the Galilean law of the fall of bodies could not be possible. Galileo gave detailed arguments and proofs to demolish Aristotelian opposition to the notion of vacuum. Detailed arguments of Galileo are presented in the Chapter 8.

There are also certain notions which have first been constructed by reason and then 'observed'. For example, when Galileo supposes that a floating body is like one of the weights of a balance, while the other weight is that theoretically delimited portion of liquid which is displaced by the body, he is clearly creating or constructing an entity. This construction is not exemplifiable in Aristotelian/Baconian methodology. The notion of the other weight that scientists have supposed has been constructed theoretically. Since we have elaborated them in the case studies we will not dwell on the example in detail here.

Therefore Laudan is incorrect in saying that Galilean science poses no threat to Aristotelian/Baconian methodology. He could have merely stated that later 17th-century micro-sciences posed a relatively greater threat than Galilean science could. This modified position is what we will defend in this thesis.

Another point needs to be stated. While Laudan's explanation to the rise of hypothetico-deductive methodology, with the modification just suggested, is justifiable, his later conclusion that the generativism has been abandoned to the point of no return will be contested. As indicated in the introduction our thesis can be viewed as a response to the challenge Laudan poses. This brings us to one of the specific problems of the thesis, which is dealt with in Part-II.

3.3 The Rise and the Fall of Logical Positivism

We have looked at certain factors that gave rise to consequentialism and the method of hypothesis. It is observed that simple inductive methods could not account for the discovery of highly theoretical, mathematical and unobservable aspects of scientific knowledge. Most scientific theories, being very far from sensory experience, could not be justified by any direct method. The only available method of justification is to test the relatively direct observable consequences deduced from the hypotheses which are often counterintuitive. However, this is only part of the story. The belief in inductivism did not vanish from philosophy of science altogether.

In the beginning of this century, a considerably influential group of philosophers, mostly Germans, began a movement called Positivism. They rejected metaphysics and were concerned with the reduction of all scientific statements to statements about sensation, seeking complete empirical verification. Early versions of Positivism are found in Herman Cohen's and Ernest Mach's neo-Kantian philosophy of science. Cohen characterized scientific knowledge as an underlying structure (form) of sensations that are exemplified in sensory experience. E. Mach viewed science as an abbreviated description of sensations. However, these attempts were unsuccessful, because of the abundance of mathematical relations occurring in scientific principles which could not be reduced to sensations.

An intellectual crisis in philosophy of science developed after the turn of the century as a result of the development of Einstein's theory of relativity, and quantum theory. Einstein's theory involved notions that required a high degree of mathematical sophistication, while quantum theory began postulating entities that are *in principle* unobservable. These new theories were found to be incompatible not only with the then prevailing philosophies of science, but also with classical physics. Initially most German philosophers opposed the replacement of classical physics by relativity and quantum theory.[21] The only school that was sympathetic to the new physics was a modified position of Machian Positivism. Ernst Cassirer also attempted to accommodate the new physics in a modified neo-Kantian philosophy.[22]

The Berlin school under the influence of Reichenbach and the Vienna school under

[21] We think that most philosophers and scientists misinterpreted the place of the new theories such as theory of relativity, and quantum mechanics. The idea that they would replace the prevailing classical theory was based on the view that the new developments are alternative world views. Here lies the major cause, according to to our diagnosis, of generating the problem of growth and development of scientific knowledge. We tend to believe that there was never a need to *replace* one theory with the other. The reasons for replacement were based on a lack of clear characterization of what scientific theories are. We have argued in Chapter 5, that the 'dislodgement' of a theory might have taken place only in the 'minds' of some of the scientists, and not from the scientific community at large.

[22] Cassirer 1923, *Substance and Function and Einstein's Theory of Relativity.*

the influence of Moritz Schlick agreed with Mach on verifiability as a criterion of meaningfulness for theoretical concepts. However, they did not agree with Mach on the place given to mathematics. One aspect of this development is Conventionalism of the Poincaré kind. Both theoretical as well as mathematical terms occurring in scientific statements are interpreted as conventional abbreviations that can be eliminated by expansion' into equivalent statements in phenomenal language. What came handy to these new developments, largely as a catalyst, were the logico-mathematical contributions of Frege, Cantor, Russell, etc. The program announced in Russell and Whitehead's *Principia Mathematica* was to provide foundations for mathematics *in* logic. This development suggested a promising possibility of accounting for the mathematical and theoretical terms of scientific knowledge in terms of logical and observational (phenomenal) vocabulary (language). Thus came into being the philosophy of Logical Positivism. It may also be said that this is also the birth of modern philosophy of science, which remained the official philosophy till the late fifties.

It is worthwhile to compare the Kantian problematic with that of the Positivist's problematic, to understand the nature of the changing views about science. Kant's major problem can be stated to be the problem of showing the possibility of *synthetic a priori* knowledge, which includes natural science, arithmetic and geometry, as well as metaphysics.

It was Kant's original idea of *synthetic a priori* judgements that shaped many philosophical schools after him. Kant's philosophical system has been a rich source of ideas for both philosophers and scientists alike. According to Alberto Coffa "the early stages of logical positivism may be viewed as a development to the point of exhaustion of this aspect of Kant's original idea."[23] Coffa's attempt is to understand the development of Positivist views on meaning from Kant onwards. One of the traditions that developed from Kant, according to Coffa, is the *semantic tradition*. The problem of the semantic tradition was

> the a priori; its enemy, Kant's pure intuition; its purpose, to develop a conception of the a priori in which pure intuition played no role; its strategy, to base that theory on a development of semantics.[24]

This recent work of Coffa demands serious attention, for it involves deeper issues for which, we have no space here. Therefore we shall be satisfied with a cursory comparison, which provides sufficient indications regarding the nature of the change that took place. We shall point out the essential connections between Kantianism and Logical Positivism on the other, followed by a summary of criticisms leveled against the latter leading to the modern version of consequentialism.

[23] Coffa 1991, *The Semantic Tradition from Kant to Carnap* p. 7.
[24] *Ibid,* p. 22.

Kant's problem was to to show: "How are synthetic cognitions *a priori* possible?"[25] Scientific knowledge is a synthesis of both form and content, or in other words it is a collection of informative forms. Undoubtedly Kant discovered a very original way of characterizing a category of knowledge that includes physics, metaphysics, and mathematics. Logic, according to him, is part of knowledge that is based entirely on the principle of non-contradiction, and therefore is on certain ground posing no further problem.

Kant's solution to his own problem consists in showing that all synthetic judgements are mediated by a concept, which is a higher representation instead of an immediate representation, that can hold many possible representations. Conceptual means of knowledge, which Kant calls discursive, yields the objective ground of the possibility of experience.[26] When the subject encounters an object by intuition, an illustration of the predicate, that the subject has *a priori*, is realized. Categories of understanding, such as space and time, are pure intuitions which contain *a priori* the conditions of the possibility of objects as appearances.

Thus Kant attempts to show that independently of all experience, a priori, the categories of understanding make possible synthetic knowledge of objects.[27]

Kant's epistemology gains significance because he posed a problem regarding the possibility of pure science, which was just beginning to emerge in a major way after mathematical methods of doing such a science were developed by Newton and Leibniz. Whether Kant was successful or not continues to be a debate till this day, but he posed an original problem for philosophers of science, one of utmost importance. Later in the 19th and 20th centuries, developments in science according to our understanding, vindicate in a major way the involvement of synthetic knowledge *a priori*, in what is known by the name of "theoretical physics". Theoretical physicists define a number of 'predicates' attributable to *possible* experience by constructive methods.

A striking difference between the Kantian and Positivists' characterization of scientific knowledge is that the latter eliminated metaphysics as meaningless. One must also note that the position of mathematics in the Positivist's framework, lies in the analytic form of knowledge, and not in the synthetic as in Kant.

The anatomy of scientific knowledge, according to the Logical Positivists, can be constructed out of three components.[28] (1) Logical and mathematical components (2) theoretical components and (3) observational component. (1) is considered analytical and 'true'

[25] *Prolegomena to Any Future Metaphysics*, Section 5.
[26] *Ibid*, B 93 - 94, B 127.
[27] *Ibid*, B 117 - B 124.
[28] Positivists considered scientific theories as axiomatic formulations in a mathematical logic L. This formulation appears below.

or 'false' by virtue of its form, (3) is considered synthetic, consisting of phenomenal observations, which is true or false by virtue of its 'correspondence' with the phenomena, obtained purely by experience. The problematic component here is theoretical statements which was initially regarded as abbreviations for phenomenal descriptions conventionally decided on. Since theoretical terms, if they are abbreviations, can be expanded to a set of sentences that can be reduced either to logico-mathematical terms (1), or to observational terms (3), the actual components of scientific knowledge are either analytic or synthetic, for the third component can be eliminated. Thus positivists' characterization of the body of scientific knowledge in terms of Kantian distinctions is clear.

As mentioned above the mathematical component becomes analytical with Positivism. The suspected ground for doing so, as mentioned above, is the Russelian logicism that mathematics can be founded on logical grounds. We know today that this project, so far, is unsuccessful. Arithmetic, considering it as one of the essential components of mathematics, could not successfully be explained purely in terms of logic. The problem is more acute with other branches of mathematics such as geometry. That various kinds of geometries 'refer' to different kinds of spaces, and that different kinds of numbers can gain significance only under geometrical interpretation, shows that mathematical objects are not totally devoid of informative content, therefore not altogether analytic. The failure of demonstrating the pure analytical character of mathematics also demonstrates that mathematics, unlike logic, cannot be grounded on the principle of contradiction alone. In spite of this failure people still consider mathematics a sure instance of the analytical component of knowledge. Undoubtedly mathematics shows structures (forms) that can be validated without recourse to experience. However, the aspect of *construction*, and *abstraction* that is involved in it cannot be construed as anything that can be based on the axioms of any pure logical system based on the principle of non-contradiction, and the principle of excluded middle alone. Which principle of logic can account for these typically mathematical characters? We think, none.

The place of mathematics is not the only source of the problem with Logical Positivism. There are a number of problems of which we shall examine some relevant ones in relation to the problem of structure of scientific knowledge and methods of validating scientific knowledge. Before we look at other problems it is necessary to say that after the formalist school developed the analytic nature of mathematics is taken more or less for granted. Various criticisms directed against Positivism did not make this failure a major point against Positivism. Another belief that is connected with the formalist view is that anything axiomatized or axiomatizable is analytic. Geometry was considered a paradigm case of analytic knowledge. Many attempts at axiomatizing different branches of sciences by the Positivists have been carried out to demonstrate that they can be reconstructed as logical calculi, to further support their views on the structure of scientific theories. The idea is that there is nothing more to the different natural sciences except the different specific/local interpretations the terms occurring in the calculi would have. If this could be achieved then their thesis about the basal structure of scientific knowledge stands more or less vindicated. Most such projects have remained mere dreams with little or no success.

We shall first look at the usual objections raised against the Positivist model and see how the Positivists themselves have made room for consequentialism in spite of an initial robust inductivism.

A summary presentation of the Positivist's view of scientific theories as presented by F. Suppe displays almost all the essential features of the view. The following conditions are proposed for any scientific theory formulated in a mathematical logic L:

(i) The theory is formulated in a first-order mathematical logic with equality, L.

(ii) The nonlogical terms or constants of L are divided into three disjoint classes called *vocabularies*:

  (a) The *logical vocabulary* consisting of logical constants (including mathematical terms).

  (b) The *observation vocabulary* , V_O, containing observation terms.

  (c) The *theoretical vocabulary*, V_T, containing theoretical terms.

(iii) The terms in V_O are interpreted as referring to directly observable physical objects or directly observable attributes of physical objects.

(iv) There is a set of theoretical postulates T whose only nonlogical terms are from V_T.

(v) The terms in V_T are given an *explicit definition* in terms of V_O by *corresponding rules C*—that is, for every term 'F' in V_T, there must be given a definition for it of the following form:

$$(x)(Fx = Ox)$$

where 'Ox' is an expression of L containing symbols only from V_O and possibly the logical vocabulary.[29]

As already mentioned the problematic elements of the view are theoretical terms. The theoretical terms present in a theory, it was believed, can be shown to be *cognitively significant* on the basis of the criterion of verifiability, which is also their criterion of meaningfulness.

Metaphysics was denied cognitive significance because the criterion of verifiability excludes it. For a term to have cognitive significance it must be either analytic or synthetic.

[29]Suppe 1977, *op.cit.* pp. 16–17.

Since metaphysics can be neither proven true or false by means of experience, nor can be shown to be true by mere form of the propositions or by meaning alone, it is denied any cognitive significance. Though Kant and the Positivists held a similar distinction between analytic and synthetic elements of knowledge the former did not deny the possibility of meaningful metaphysics, while the latter denies it. Thus both meaning and truth are decided by the same criterion of verifiability. Though semantic theories based on Tarski's suggestion have been developed that would avoid such a collapse of meaning and truth, not so much use of these developments has been employed, until recently, by philosophers of science. The semantic approach in the philosophy of science is fast developing into a coherent framework that accounts for the nature of scientific knowledge. We will defend below a version of the semantic approach.

Since it is the correspondence rules that are meant to provide cognitive significance to theoretical terms, the search for proper correspondence rules ultimately determines the success or failure of the Positivist model.

In the initial proposal the correspondence rules were thought to be explicit definitions. It was soon realized by Carnap that dispositional terms cannot be given explicit definition using observational vocabulary and first-order predicate calculus. No techniques of modal logic were developed at the time to interpret sentences containing dispositional terms as subjunctive conditionals. P.F. Bridgman made the proposal that every theoretical concept is synonymous with the corresponding set of operations. He called such definitions *operational definitions*. Operational definitions, too encountered difficulties with dispositional terms. They also faced another problem called proliferation of concepts, which is due to the possibility of many different sets of operations corresponding to an otherwise single concept. Unless a more general notion is available that can cover the various sets of operations for a given concept, the operational definition cannot be considered a definition at all, for a definition must be both necessary and sufficient.

Later the conditions imposed on correspondence rules were relaxed by the Positivists, having realized that they cannot be regarded as definitions, for it is very difficult to obtain necessary and sufficient conditions. In place of any definitions Carnap (1936-37) introduced *reduction sentences*. Reduction sentences also provide definitions but only *partially*. The theoretical (dispositional term) term 'fragile', for example, would have the following form of reduction sentence: "Any body be called fragile if it is struck at a given time, then it will break at that time, iff it is fragile". What this sentence stipulates is a condition under which such an effect is possible, without completely defining the term. We will not come to know what it is for something to be fragile. Thus it is no longer required that correspondence rules provide complete definitions, but only *partial definitions* for theoretical terms.[30]

A very important change in the view, of course by further weakening the idea of cognitive significance, was suggested by Carl Hempel (1952). He argued that since theoretical terms are never, and cannot be introduced by reduction sentences based on observables, but are introduced jointly by setting up a theoretical system formulated in terms of theoretical terms, what can be and what needs to be provided are only unique observational consequences of theories involving such terms. It it is not necessary that each such term be individually defined. Thus it is sufficient to grasp that the empirical manifestations of a theoretical construct/entities follow the pattern suggested by the theory explaining and predicting the observable phenomena. Hempel also proposed the well known covering-law model of explanation/prediction. This is the beginning of a new era of Positivism based on a consequentialist notion of cognitive significance. Ultimately all claims of acquirability of scientific knowledge by only direct/inductive means from observational source was abandoned. At around the same time Karl Popper proposed a proposed a stronger consequentialist view of science than Hempel, by totally abandoning all traces of inductive method. It may be noted here that those who held a instrumentalist view of theories attempted to eliminate or dispense with theoretical terms, while those who held a realist view of theories attempted to provide an ontological basis by accounting for theoretical terms/entities.[31]

The account so far shows how the development of consequentialist manner of supporting theories has taken place by successively weakening the load on cognitive significance.

According to to Positivists only either analytical or synthetic truths are cognitively significant. According to Carnap analytic statements are those that are true in virtue of their logical forms, *and* the meanings of the logical and descriptive terms occurring in them.[32] Carnap gives a positive characterization of synthetic, unlike Kant, who characterizes synthetic as that which is not analytic. It is clear that the Positivist view of cognitive significance is based on the distinction between the analytic and the synthetic. Therefore, if the distinction is untenable the Positivist views would also be in serious trouble.

One famous objection to the distinction is provided by Quine (1953). Of the two aspects of analyticity, one is by form and the other is by meaning, of which the latter notion is problematic. Firstly, its problem lies in the fact that analyticity is based on another more unclarified notion called synonymy, because all statements that are analytic in virtue of

[30]Cf. F. Suppe 1977, *op.cit.* pp. 18–22.
[31]Cf. Suppe, *ibid*, p. 35.
[32]Carnap 1966, *Philosophical Foundations of Physics* p. 259.

meaning can be transformed into analytic statements in virtue of form by proper substitutions of synonyms. Secondly, Positivism also holds the verification theory of meaning, according to which the meaning of a statement is equivalent to the method of confirming it. On this account, a statement that is analytic in virtue of meaning can only be that which is true "come what may". Thirdly, Quine challenges the view that each statement, even if it is about the world, can stand before the tribunal of experience individually.[33]

Though H. Putnam agrees that there is a distinction, he says that there is a large class of statements in natural science which are neither analytic nor synthetic.[34] Putnam, however, allows the possibility that analytic statements can become false if there occurs a change in the meaning of the constituent terms in the statement. The middle category according to Putnam consists of *law cluster concepts*, such as kinetic energy, principles of geometry etc. Since these concepts can be denied without change in their extensional meaning, they cannot be analytic. These principles cannot be thrown out by any isolated experiment or verified by inductive means. Since these principles are always applied in conjunction with certain other principles, the trouble could be with the combination, rather than with the principles as such. Therefore they are not synthetic either. Since a large number of definitions/principles/laws of highly developed science are of this kind, which are neither analytic nor synthetic, the traditional watertight distinction cannot be put to use to understand the nature of modern science.

Putnam's arguments have serious consequences for the future of the Positivist view of science. Putnam does not deny the distinction, but he has, we think, shown successfully that being able to distinguish the two categories has little consequence, if any, for understanding the nature of scientific knowledge, because a large part of scientific knowledge belongs to the class of definitions. That a large part class of scientific knowledge cannot be validated by empirical (synthetic) means introduces further problems to Positivism.

If it is true that the objects of scientific knowledge consist in this new class of definitions, then there should exist a corresponding methodology, which is neither deductive nor inductive. In the thesis being developed we attempt to show that the method of validating scientific knowledge has to be neither inductive (synthetic) nor deductive (analytic). For more precise characterization of the proposed structure of scientific knowledge we will have to wait.[35]

---

[33]Quine 1953, *From A Logical Point Of View* p. 41.

[34]Grice and Strawson (1956) also argued for a similar position, but on grounds and motivations different from Putnam. Since Putnam's 'middle' category is relevant for latter discussions we considered Putnam's rather than Grice and Strawson.

[35]A few other influential views were developed by Ernst Nagel, Braithwaite and Mary Hesse. One common

Independent of the above source of problems for the Positivist views, Karl Popper claimed that we can deny all modes of validating scientific knowledge by means of generatability. He rejected the method of verification as a criterion of cognitive significance based on the Humean line of invalidating inductive justification. He argued that scientific theories/laws cannot be verified by means of accumulating observational evidence. However, scientific theories/laws can be *falsified* by observational means. He, thus, introduced a new criterion of scientificity.

Popper argued against the observation/theory distinction, which is one of the basic presuppositions of the Positivist view. Against this distinction he argued that all observations are theory laden. His arguments in this regard also form an attack on the generationists in general. It is based on the thesis of the theory-ladenness of observations. This is targeted against the generationists', specially the inductivists' belief that induction enables them to infer mechanically true scientific theories from an exhaustive collection of facts gathered without any theoretical preconceptions. Criticizing inductivism, Popper says:

> I believe that *theories are prior to observations* as well as to experiments, in the sense that the latter are significant only in relation to theoretical problems ... I do not believe, therefore, in the 'method of generalisation', that is to say, in the view that science begins with observations from which it derives its theories by some process of generalization or induction.[36]

Popper uses a metaphor to describe the two traditional positions in *Objective Knowledge* (1972). He says our mind is not an empty 'bucket' as traditional empiricists had thought, which can be filled by making a number of observations. Rather it is like a 'search light' projecting theories on the world around selecting observations.[37]

Carl Hempel also expresses the view that without a prior tentative answer (hypothesis) to the problem under study, one would not know which facts are relevant to the inquiry, and the set of all the facts is not exhaustive.[38]

We think that theory-ladenness of observations should not be considered a valid objection against a logic of discovery or generationism in general, though it is a valid objection against the inductive view of arriving at theories. Reasons are elaborated below in Chapter 4.

theme of these philosophers of science was to understand the structure of scientific theories based on the notion of a model. A model provides an interpretation to an uninterpreted formal calculus. A model for a theory is that in which the theory is true. These views are developed to provide an independent support for the non-observational component of a scientific theory, over and above the partial interpretation views mentioned above.

[36]Popper 1957, p. 98., our italics.

[37]Peter Medawar makes the same point in a different way: "We cannot browse over the field of nature like cows at pasture . . ." Cf. Medawar 1969, p. 51.

[38]Hempel 1966, *Philosophy of Natural Science* pp. 12–13.

It should be noted that though Popper's falsificationism was not free of problems, it could command a large following.[39] This would be partly due to Popper's ability to successfully divert the attention of philosophers of science from the problem of canonical formulation of scientific theories based on formal and linguistic methods to the problems pertaining to the issue of the growth and development of scientific knowledge. Popper's success, thus, mainly consists in attending to a new set of problems.

The philosophy of science developed by Popper has two arms. One of them is fallibilism, and the other, consequential justification. These are put together in a coherent manner to give rise to the hypothetico-deductive methodology. According to this methodology the scientist proposes an hypothesis and deduces testable consequences from it. How a scientist conceives it is immaterial to a philosopher, for, it is held, that the manner of conceiving has no epistemological relevance. It is Popper's version of consequentialism that replaced inductivist/positivist philosophies of science.

The account given here on the rise of consequentialism is far from being complete. There are many other reasons that could have been stated. The role of American Pragmatism advocated by Peirce, for example, could have played a very important role, especially in the American continent.[40] However, the essential problems that have led to consequentialism, to the best of our awareness, have been covered in the account.

3.4 Kuhn's Irrationalism

The cumulative (linear accumulation) view of scientific progress has been attacked by Kuhn in *The Structure of Scientific Revolutions* where he proposed an alternative pattern of how scientific knowledge "advances" by alternation of a normal science phase and a revolutionary science phase. According to the cumulative view originally held by positivists (held even today by most working scientists) and uncritical believers and supporters of science, the development of scientific knowledge consists in gradual addition of true theories/laws one after another, accompanied by the rejection of the false theories/laws. That is, scientists reject a theory because it is false, and accept it if it is true, therefore there is a rational pattern to the development of scientific knowledge. If a theory is dislodged by another, the older theory is either false or less close to the truth than the new. Truth or falsity of scientific

[39] Quine/Duhem's thesis, for example, is one major objection against falsificationism, though in principle it applies to all methods of validation. Its main point, which is based on holism, is that an observation cannot conclusively falsify a theory which is a coherent net of propositions. By making appropriate changes elsewhere in the system of the theory, it can be made immune to falsification.

[40] Cf. Nickles 1980, "Introductory Essay" *Scientific Discovery, Logic, and Rationality* p. 4.

theories, it was believed, can be determined by the employment of methodological/systematic procedures. Kuhn's influential thesis attacks such a view.

Kuhn's powerful and insightful historical illustrations brought about a revolutionary change in the views held by contemporary philosophers of science. Briefly Kuhn's position is as follows:

There is a phase of scientific development called *normal science*, during which scientists work according to rules, solve puzzles (problems) on the basis of a more or less fixed (predefined) set of conceptual apparatus. During this period the behavior of scientists is uncritical (normal/rational/dogmatic). However, normal science occasionally faces certain crisis situations, called *anomalies*, which cannot be solved by the present set of rules and conceptual apparatus. When anomalies accumulate, science is said to be in a crisis. During this time some scientists become critical of the suppositions of the normal science in the light of an alternative set of assumptions and conceptual framework. Some scientists, specially those who are young, start looking at the problematic cases 'under' the light of new hypotheses. As many more scientists start looking at the problematic cases in the new manner, a rapid progress of of the idea takes place by pushing the older one aside. This phase of scientific development is *revolutionary.*

It is believed that every theory is born refuted, and hence the new set of ideas also face the problem of anomalies. On the basis of new set of rules and a new conceptual apparatus another normal science phase comes into being. Since normal science is usually set by solving a problem in a novel manner, that problem becomes a role-model or a paradigm case. Since the group of scientists share a set of common problems, goals, methods, standards and basic assumptions, called as disciplinary matrix, a paradigm of shared values is formed. In terms of paradigms scientific development consists in replacing one paradigm with another.

However, the controversial claim of Kuhn is that the conversion from one paradigm to another is comparable to a gestalt-switch, a religious conversion, a duck-rabbit situation, involving *no reasoning*. A paradigm becomes a view by *consensus,* it is the number of believers that determine the success of one theory against another. Thus Kuhn's view posed a challenge to those philosophers of science who held that scientific development can be explained by rational or methodological means. What emerged as a result of this is not favorable to the line epistemology has been following traditionally from ages.

Consequentialists believed, though they are against inductive verification, that epistemology has the role of validating scientific knowledge. After Kuhn's popular standpoint epistemology appears to have lost even that narrow footing based on consequential testing.

The scheme presented by Stegmüller clearly shows what precisely happened to philosophy of science in the course of its development.

(1) Hume says that science develops *inductively* and *nonrationally*;

(2) Carnap's idea is that it develops *inductively* and *rationally*;

(3) Popper's answer is the dual counterpart of Hume's, namely that it follows a *noninductive, rational* course;

(4) Kuhn's view deviates from all of these. A comparison of his conception with the other three seems to indicate that he thinks the course of science is *noninductive* and *nonrational*.[41]

What, therefore follows from this development is that all meta-theories of science are futile. This threatens the very basic tenet of the general philosophy of science. In the course of the thesis we will have more than one occasion to critically appraise the views of Kuhn.[42]

To summarize, we have seen that certain developments *within* science have prompted the development of consequentialism, for no methodological account could be provided for the genesis of theories/hypotheses/ideas that are highly abstract. We have tried to show that Laudan's observations regarding the character of Galileo's science is incorrect, for Galileo's science is sufficiently theoretical and abstract in order to show the failure of the Baconian/Aristotelian inductive methods. Though there are sufficient reasons for the development of consequentialism in the 19th century itself, there was a temporary rise of inductivism developed and defended by the Logical Positivists at the beginning of the century. Later we have shown how the rise of Positivism has seen another spurt of inductivism, which too has to be abandoned, for scientific theories could not be shown to be constructed in the manner suggested by them. Ultimately Popper's philosophy of science became an alternative to the problem ridden Positivism.[43] Kuhn's thesis on the structure of scientific revolutions, and inter-theory relations has led to the irrationalist views of scientific knowledge. Consequentialism has not vanished from the scene at all, despite the fact that the emphasis on method in philosophy of science has become rather out of fashion. It is certainly true to say that consequentialist

---

[41] Stegmüller 1976, p.136.

[42] Kuhn's *The Structure Of Scientific Revolutions* caused lot of critical reactions. The above account is more or less a digest of the immediately relevant issues taken from the following sources, apart from the original: Dudley Shapere 1964, 'The Structure of Scientific Revolutions' *Philosophical Review* **73**, Lakatos, Imre and Musgrave, Alan, eds, 1970, *Criticism and the Growth of Knowledge.*, Hacking, Ian 1983, *Representing and Intervening*.

[43] Popper's philosophy of science too did not continue in the manner in which it was formulated. It was modified by his supporters, mainly at the London School of Economics, led by Imre Lakatos, who defended a moderate version of falsificationism that was designed to meet the objections/criticisms raised by Feyerabend, Quine, Kuhn, among others.

testing/validation of scientific theories still continues to be the official philosophy of science. On the question of the possibility of a logic of discovery, the consequentialist views remain the received view.

Part II

The Central Argument

Chapter 4

Epistemology of Discovery

The essential argument of the thesis begins in this chapter. We will first critically review the arguments against a discourse of discovery in epistemology, which culminated in Laudan's challenge. It is observed that the dichotomy of contexts into those of discovery and justification, as proposed by Reichenbach, need not be challenged for promoting the epistemology of discovery. However, it is suggested that the epistemologically significant context of justification be properly distinguished into the context of generation and the context of application. With regard to the problem of theory ladenness of observations, it is proposed that in the context of the genesis of scientific knowledge observations are not theory laden, while in the context of development, all scientific observations are theory determined. Our response to Laudan's challenge consists in working out the possibility of generativism and fallibilism on one hand, and distinguishing meaning and truth as two distinct epistemological values on the other hand. We then explore the peculiar nature of ampliative logics as against explicative logics. We then give a positive characterization of induction as a species of the ampliative logic of abstraction. We propose that induction is based on the principle of excluded extremes, just as deduction is based on the principle of excluded middle. The question of validity of induction should therefore be considered independently from the notion of deductive validity. It is also observed that the world where induction is possible is the world where mathematics is possible, setting the context available for the highly ampliative logic of inversion, which is stated to be based on the principle of included extremes. In the last section we posed the tension between the content-neutrality of logic on one hand and the possibility of amplifiability of content by logics of discovery on the other.

4.1 The Received View

In the light of significant developments in the history of epistemology and scientific method, many scholars have found it necessary to raise the question of the legitimacy of the fundamental problems in epistemology. One of the major issues of traditional epistemological discussions that has met with greater opposition is regarding the method/s of scientific discovery. According to the "received view" in the philosophy of science the genesis or formation of concepts and theories (knowledge) should be distinguished from the epistemological analysis of concepts and justification of knowledge. It is claimed that the study of the genesis of concepts and theories is ultimately irrelevant to the objective study of their epistemological status. It is further claimed, that such a study would be appropriate for a psychological study of the processes of thought but not for a philosophical analysis of knowledge. The process of how we arrive at possible knowledge escapes logical analysis, thus the question has no relevance in philosophy. Our main task in this chapter, therefore, is to critically assess the position/s, expressed in the present century, against the epistemological relevance of the issues pertaining to the genesis of knowledge.

A discussion on this issue can be initiated by considering one of the most important distinctions introduced in the present century between the context of discovery and the context of justification. The distinction has gained widespread currency among both camps, i.e., those who are in favor of a logic of discovery and also those against it. This distinction however particularly boosted the received view. One reason for this is that though it was originally introduced to separate the psychological component from the philosophical or epistemological the distinction itself was based on another—between the logical and the illogical. Since this distinction has gained a great deal of popularity, and since the roots of this distinction have bearing on the content of this chapter, we shall repeat these rather well known arguments.

According to the popular view of scientific discovery, discoveries and inventions are to a large extent unexplainable, results of imaginative leaps, and are the product of the 'sparks' of creative genius. Hans Reichenbach, and Karl Popper are among the chief proponents of this irrationalist account of discovery. According to Reichenbach scientists while discovering a theory are usually guided by guesses, and they cannot name a method by means of which they discovered it. Careful study of Reichenbach does not indicate that he is altogether against the program of discovery, though he clearly distinguished the psychological context from the epistemological. We shall see below (page 91) that he allows discoverability arguments in the context of justification. Reichenbach's often quoted passage is as follows:

> The act of discovery escapes logical analysis; there are no logical rules in terms of which a 'discovery machine' could be constructed that would takeover 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 facts and a theory presented to him with the claim that it explains these facts, in other words *logic is concerned with the context of justification.*[1]

The view that logic is concerned only with the context of justification is what we call epistemology-minus-synthesis. The possibility of a logic of discovery is therefore ruled out. Following him Karl Popper maintains that the initial stage of conceiving or inventing a theory neither calls for logical analysis nor is 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.[2]

He further goes on to say that "every discovery contains an irrational element or a creative intuition". He quotes Einstein, who says that there is no logical path leading to scientific laws and they can only be reached by intuition, based upon something like an intellectual love of the objects of experience. More than this Popper even makes room for faith in the context of discovery, when he says that

> [S]scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes even quite hazy; a faith which is completely unwarranted from the point of view of science.[3]

These remarks suggest that scientific discovery is necessarily irrational, and it is a matter for psychological inquiry. The only scope left for philosophy of science is to carry out testing of theories devised by the greatest of great minds.

Is this distinction merely serving the purpose of clarifying the scope of philosophical analysis with respect to knowledge? But if it is only the testing of theories that falls in the scope of epistemological analysis, then there is a danger of losing the ground for any epistemology of science. This possibility exists because methods of testing are not on any surer ground. If there exists no surer ground for consequential testing—sufficient difficulties with consequential testing of theories have already been pointed out—then no possibility of epistemology remains, for the context of discovery has already been regarded as abandoned

[1] Reichenbach 1938, *Experience and Prediction*, p. 231, our italics.
[2] Popper 1959, *The Logic of Scientific Discovery* p. 31.
[3] *Ibid* p. 38.

from epistemology. In fact this has taken place in the recent past. We are familiar with the views challenging the very idea of methodology and epistemology.

If it is already well known that the consequentialist view has failed in the appraisal of scientific theories, one might ask, why then call it the received view? It is still the received view insofar as the problem of discovery is concerned. Most scholars criticized the hypothetico-deductive method for reasons of its failure in the context of justification and certainly not for their views regarding the problem of discovery. Its failure in the former context did not affect the popular stand taken by scholars at large against the methodology of discovery, because most scholars seem to be convinced that no epistemological significance can be given to the context of discovery. (See § 4.5 page 101 for the statement of Larry Laudan in this connection.)

It is possible that the failure of finding a methodology for testing theories could have been because we did not take the context of discovery seriously. Certain factors in the latter context must have had some relevance in the former context. Kuhn did suggest that psychological and social factors do enter into theory appraisal. But he agrees with, for example, Popper that there are no rules for inducing correct theories from facts.[4] Since he emphasizes "socio-psychological" dimensions in his analysis, and has criticized Popper for the latter's obsession with normative concerns, we can infer that he is also one of those who has not seen the possibility of a logic of discovery. However, though he would deny logical status to the context of discovery, he would undoubtedly be interested to deliberate, unlike Popper, about the context of discovery without hesitation. Thus it is claimed that non-logical factors do play a role in theory appraisal by the opponents of consequentialists, but the position of the consequentialists on the question of the discovery problem remained without challenge.

We will see below that some of the defenders of the discovery program have denied any water tight distinction between the two contexts. We will introduce another distinction—between the context of generation and the context of application—in place of the traditional distinction, and will argue that both the contexts have justificatory (proper epistemic) role.

In the previous chapter we have already noted the observations of Laudan regarding the rise of consequentialism and the reasons thereof. Laudan also provides a more challenging argument against the possibility of a philosophical analysis of discovery in his paper 'Why was the Logic of Discovery Abandoned?'. As mentioned in the introduction, since this thesis can be viewed as a response to his challenge, we shall present a full statement of his challenge. The challenge is based on the claim that "the case has yet to be made that the rules governing

[4] Kuhn, 1977, *Essential Tension,* p. 279.

the techniques whereby theories are invented (if any such rules there be) are the sorts of things that philosophers should claim any interest in or competence at." The challenge itself would be to show that the logic of discovery (if at all formulatable) performs the epistemological role of the justification of scientific theories. Therefore, those who profess generativist methodology must show that methods of generation *per se* do carry special epistemic weight.

According to Laudan there are two groups of philosophers, namely the generativists and consequentialists. The generativists believed that theories can be *established* only by showing that they follow logically from statements which are the direct results of observation. Bacon, Descartes and Newton are the main advocates of this thesis, for whom hypotheses and hypothesis testing functioned as heuristic devices for establishing evidential basis for genuine theories. The consequentialists on the other hand believed that if consequences are proved to be true then this provides an epistemic justification for asserting the truth of the theory.[5] The latter thesis developed after the 19th century in the light of the absence of any direct route (which mostly meant inductive routes) from phenomenal claims to deep-structural theories. These problems of pure inductivism were raised in the writing of Herschel, Whewell, Mill, DeMorgan, Boole, and Jevons during the 19th century.

It is clear that both the groups are primarily concerned with the epistemic problem of theory justification. For generativists the purpose is dual. One is to accelerate the pace of scientific advance, and the other is to provide a sound warrant for our claims about the world. If a foolproof logic of discovery could be devised, they thought, it would both be an instrument for generating new theories and, since the scientific theories are believed to be infallible it would automatically guarantee that any theories produced by it were epistemically well grounded.[6] This has been the manner in which the link between infallibilism and generativism developed. Laudan explains that most traditional philosophers subscribed to the view that legitimate science consists of statements which are both true and known to be true.[7] So long as infallibilism was in fashion, generativism had grounds for survival.

However, as already mentioned, in the early nineteenth century infallibilism crumbled, giving rise to fallibilism. It was during this time, according to Laudan, that an *unmistakable shift* took place. It took place by two mutually reinforcing developments that converged to separate discovery from justification. One of course was the increasing attractiveness of a fallibilistic conception of theories. It was based on the realization that scientific

[5] Laudan 1980, in Nickles 1980, p. 184.
[6] *Ibid,* p. 183.
[7] *Ibid.*

claims cannot be proven true whether or not they were generated by induction from the facts. Second was the acceptance of the view that evaluation of theories can be done in terms of their consistency and their testable consequences. Consequential testing did not find its way initially because of the fallacy of affirming the consequent. It could be freed from this fallacy only when the fallibilist view of theories was widely accepted. These are, according to Laudan, the factors leading to the abandonment of the logic of discovery.

Apart from the developments in philosophy of science there are certain developments in science that have played a decisive role in the replacement of inductivism and infallibilism by the consequentialist methodology. Nickles points out that the developments in modern science do not appear like the stuff for inductivists. The cases such as the phenomenological thermodynamics which gave way to kinetic theory and statistical mechanics, the Newtonian world picture gave way to the Einsteinian, and also the development of Quantum theory etc., can not be accounted for by inductive reasoning.[8] The failure of inductivism becomes clearer if we consider that any science worthy of consideration involves theoretical entities and processes which are far removed from the observational realm. Thus Laudan is correct in making the following observation:

> [I]f what we expect to discover are general statements concerning observable regularities, then mechanical rules for generating a universal from one of more of its singular instances are not out of the question. By contrast, if we expect to discover 'deep-structure', explanatory theories (some of whose central concepts have no observational analogues), then the existence of plausible rules of discovery seems much more doubtful.[9]

With these arguments, that appear certainly plausible, Laudan claims that the logic of discovery has no philosophical relevance. Therefore the challenge for any generativist today is 'Why should the logic of discovery be revived?'. In order to meet the challenge, as stated above, it is necessary to show that generativism has epistemological relevance. We will try to meet the challenge by not only arguing for the epistemological relevance of, but also proposing a method of discovery/invention of scientific knowledge. Before we begin to develop the thesis, we find it necessary to show that most of the objections, raised against the discovery program, including Laudan's that are mentioned above, are illegitimate.

Let us take stock of the various reasons given against the logic of discovery before we start discussing their credibility or lack or it.

(1) The context of discovery is restricted to the study of the actual, causal procedures underlying actual human behavior; hence it is a subject matter of psychology.

[8]Nickles 1980, *op.cit.* p. 4.
[9]Laudan, *op.cit.* p. 178.

(2) The discovery of ideas is a creative act, requiring intuition, imagination and individual talent.

(3) There is no discovery machine or set method or logic for arriving at new ideas.

(4) Inductivism has been proved wrong. And there are no pure observations free from theories, and so observations cannot be the starting point of obtaining knowledge.

(5) Generativism gave way to consequentialism because history supported the 'falsity' of infallibilism. So long as infallibilism was in fashion, generativism has grounds to survive. After the advent of fallibilism the logic of discovery has lost total epistemological ground.

We will take up each of these points and argue that all of them are mistaken.

4.2 Psychology or Logic?

We have seen that Reichenbach and Popper argued for a clear demarcation between psychology of knowledge and theory of knowledge. They likened epistemology to a normative, logical study of knowledge. While for Reichenbach logic included both the inductive and the deductive varieties, for Popper it included only deduction.

Martin Curd contends that Reichenbach's original distinction, as stated above, was quite different in nature and in application from the one usually attributed to him.[10] It is true that Reichenbach demarcates epistemology from psychology. According to Reichenbach epistemology aims "to construct justifiable sets of operations which can be intercalated between the starting point and the issue of thought processes, replacing the real intermediate links. *Epistemology considers a logical substitute rather than real processes.*"[11] T. Nickles points out that:

> Reichenbach did not deny that reasoning may occur in thinking one's way to a discovery or problem solution; the rational reconstruction of that reasoning to a theory would be an appropriate philosophical task, falling into the context of justification. ... It may happen occasionally that the original reasoning to discovery is complete and consistent, in which case the logical reconstruction will be identical with the reasoning in the context of discovery.[12]

[10]Curd 1980, 'The Logic of Discovery: An Analysis of Three Approaches' in Nickles 1980, *op.cit.* pp. 201–219.
[11]Reichenbach 1938, *op.cit.* p. 5.
[12]Nickles *op.cit.* p. 12.

Reichenbach makes no distinction between the process of discovering or generating new theories, laws, explanations and the process of justifying them. His distinction is merely between scientific activity itself and that activity as logically reconstructed.[13]

From this it becomes clear that Reichenbach's distinction of the two contexts did not deny the possibility of the logic of discovery under the context of justification.

Our response to the question whether a theory of discovery should be psychological is that it should not be. Not because psychology cannot contribute anything insightful in relation to scientific discovery, but because there are certain essential epistemological issues in the context of discovery.

Another view that goes hand in hand with the view that a theory of discovery belongs to psychology is the view that creativity is the function of a genius, and not just *any* human being. This part of the claim presupposes that discovering/inventing ability is not a character of all human beings. It is a subjective ability, for it depends on the psychological profile of a scientist. Thus epistemologists have nothing to contribute in this regard. What conditions make an individual a genius is not the concern of a theory of knowledge, for an epistemologist is concerned with validation of knowledge, assuming that all abilities are given by whatever source. This amounts to nothing more than drawing a line between concerns of natural science and the concerns of a philosopher. Consequentialists need not be attacked on this point. We, therefore, shall not argue against such a demarcation between the factual and philosophical matters. We see no threat to generativism if this part of the consequentialist thesis is conceded. Further, we will claim that a generativist thesis would emerge stronger if and only if this part of the thesis is conceded.

What needs to be attacked, however, is the thesis that the context of discovery *belongs* to psychology or biology, or any other natural science. We think that it is possible to articulate how we arrive at ideas without recourse to behavioral/biological mechanisms. Psychological or biological mechanisms are not after all the same as logical or methodological "mechanisms" (inference patterns).

Why is it that the context of discovery does not include the study of actual, causal processes underlying human behavior? None of the philosophers who argued for a logic of discovery (such as Bacon or Descartes) have meant it to be an empirical study of human behavior. Surely consequentialists know this. They are aware that they have not been able to propose a method of discovery. The method of induction to be sure cannot explain the genesis of the deep-theories modern science has produced. We agree with them on this point.

[13] *Ibid,* p. 12.

Consequentialists indeed have been quite successful in bringing home this point. Descartes' systematic method of doubt or his method of analysis and synthesis have also not been able to either generate or explain by rational reconstruction any of the major discoveries. Failure on the part of some philosophers then should not lead us to believe that there cannot be a logic of discovery. If we infer from the failure of traditional generativists that they failed because there exists no such method, our inference cannot be rated legitimate. For, even a logic of deduction has a history. Nobody would dispute the point that even atemporal formal systems need to be (and were) constructed and devised by thinkers who are actual human beings. We have seen in the preceding chapter the genesis of epistemology, and the changing methodological scene. In the course of that development many fresh ideas (mostly in the form of thematic-pairs) have been invented. On this count epistemology is no different from any exploratory disciplines. Except for the peculiar subject matter of epistemology we do not see any distinctive features that separate it from any other exploratory field of inquiry. Therefore, from the fact that epistemology has so far not offered any theory of discovery, it does not follow that epistemology has nothing to do with discovery. No biologist has given a completely satisfactory account of life. But we don't say therefore that biology is not about life. Certain questions are difficult, and the logic of discovery is one among them. Unlike scientists most philosophers suffer from the disease of being conservative. No exploration is possible if one believes that absence of evidence is the evidence of absence. We are not willing to agree that philosophy is merely *explicative* and not *exploratory*. Our narrative of the development of epistemology and scientific method in Part-I has made an attempt to bring home this impression that philosophy also has something called development.[14]

In recent times after the development of Artificial Intelligence and other computation based techniques—which we should note are dependent on whatever little we know of methodological/formal aspects of thinking—large varieties of problem solving algorithms have been implemented. Though this may not be sufficient to understand the intricacies of the complex and highly involved process of scientific imagination, we cannot underestimate

[14] A few remarks are not out of place regarding the nature of philosophical knowledge. It is necessary to keep in mind that people started thinking deductively even before deductive logic was established. The ability to think deductively is not the epistemologist's or logician's gift to human beings. His contribution is that he abstracted a pattern of thought so that an aspect of thinking can be described and understood. It is not appropriate therefore to say that if logicians have not found a logic we would have been deprived of validating mechanisms for our knowledge. Who would ever say that we need to know the physiology of digestion in order to digest food! However, the advantages of knowing our *abilities* as well as *limitations* legitimizes the activity of philosophy. Therefore absence of any successful logic/s of discovery should not lead to the inference that there are no logics. If there is any aspect of human being that is least understood by any standard it is the aspect of human thinking. We should realize that if the theories of thinking remain incipient so would the theory of knowledge. By theories of thinking we do not mean the psychology of thinking but logical patterns of thinking.

computational capabilities. The point we wish to make by considering this case is that the theory/s upon which these developments are based are *not* psychological, but are *abstract logico-mathematical theories.*

None of these developments, whether it be the abstract logico-mathematical theories behind the complex computations, or the concrete realizations of such theories, by any stretch of imagination be called 'causal processes underlying human behavior', and so be dubbed as belonging to human psychology. Responding to the allegation that the logic of discovery is restricted to psychology Kelly says:

> First, it [the logic of discovery] is not confined to the study of *actual*, causal processes. Given a programming system, the hypothesis generation procedures specifiable in that system exist abstractly in the same sense that proofs in a given formal system exist. So, the logic of discovery is an abstract study whose domain includes all *possible* procedures.
>
> Second, the logic of discovery is concerned with the investigation of hypothesis generation procedures. What adequacy comes to is a *normative* question. Desiderata include general applicability, rapid convergence, efficiency, and an ability to generate simple, explanatory, confirmed hypotheses in the short run. So the logic of discovery is a normative, abstract study.[15]

Kelly's attack on the anti-generativist position exploits the computational symmetry between test procedures and generation procedures. He says that if one pays attention to the mathematical, computational basis of the logic of discovery one would not take an anti-generativist attitude. These considerations clearly suggest that a logic of discovery is not visualized by most defenders as a theory of psychology but as a logico-mathematical theory. Therefore, it is not valid to say that context of discovery belongs to psychology, knowing fully well that most of the defenders are not looking for a psychological theory of discovery.

We agree that these arguments cannot lead us to anything close to a logic of discovery. Our concern was basically to show that consequentialists like Popper, (and possibly Reichenbach) are wrong to think that the context of discovery should belong to psychology, or natural science.

4.3 Divorce Thesis

Response to the divorce between the two contexts of discovery and justification has been different even within the group that defends a logic of discovery. Hanson argued that creation and inventiveness are not mysterious or irrational and it would be inappropriate

[15]Kevin Kelly 1987, 'Logic of Discovery', *Philosophy of Science,* Vol-54, pp. 436-37.

if we concentrate solely upon confirmation and falsification of conjectures. He argued that both the contexts have a logic. He has shown that the standards by which a theory is confirmed or refuted are not simply applied after the theory is presented in its completed form. These ideas enter into the form of thinking during the genesis of the theory. He demonstrates his point by applying Peirce's theory of abductive inference in the case of Kepler's discovery of planetary motion.[16] Hanson therefore is not against the distinction. H. Simon also believes in the distinction and holds that there is a special logic of discovery distinct from the logic of justification.[17] These views should be clearly distinguished from those of Bacon and Newton for whom the methods of discovery carry a special epistemic weight. And more importantly the methods of consequential testing, according to them, are inferior to generative justification.

Some of those who favored the discovery program such as Paul Thagard, Marcello Pera, M.D. Grmek, Robert McLaughlin etc., have attacked the divorce thesis. P. Achinstein has argued that any argument used in the initial generation of ideas could in principle be found in the context of justification and vice versa.[18] Thomas Nickles argues that such attacks on the dichotomy are not only unnecessary, some moderate distinction would be supportive of the discovery program.[19] Thus we see that even among those who are in favor of a logic of discovery there is disagreement regarding the distinction. The position that we shall defend is as follows.

In place of the distinction between the context of discovery and the context of justification, we suggest an alternative. As already stated the motivation for the original distinction proposed by Reichenbach was to separate the epistemological context from the psychological/biological context. Let us first rephrase the original distinction, to avoid confusion. The new names we suggest are the context of natural theories of knowledge, and the context of philosophical theories of knowledge. The former would include more or less all the aspects that are studied under the name of cognitive science, which is fast emerging as a new inter-disciplinary science. This includes cognitive psychology, cognitive biology, some mechanical aspects Artificial Intelligence, etc. The latter would include the context philosophers should be interested in, which may be called the *epistemological context.* So far we have not introduced anything new, except to suggest, what we consider to be better and the least confusing terminology.

We suggest now that that the epistemological context be further divided into two

[16]Cf. Hanson 1958, *Patterns of Discovery*.
[17]H. Simon 1977, *Models of Discovery and Other Topics in the Methods of Science.*
[18]Cf. Nickles 1985, 'Beyond Divorce: Current Status of the Discovery Debate' *Philosophy of Science* p. 180.
[19]*Ibid* p. 185.

more contexts, namely, *the context of generation* and *the context of application*. Since both are epistemological, justificatory reasons are to be found necessary in both contexts. Then where lies the difference? As the terms indicate, the former context refers to the mode of philosophical inquiry that addresses the question "How do we arrive at knowledge?". This context includes the philosophical deliberations on the problems of discovery and invention. Since this essay addresses this very question, but restricted to scientific knowledge, further elaboration would follow. The latter context is regarding the truth claims of scientific knowledge. It may not be immediately clear to the reader why we have used the expression 'application' to describe this context. What we have in mind relates well with the context where truth and falsity of scientific assertions is determined. We will consider a scientific assertion as involving the *application* of what is produced in the former context, the context of generation. We propose that epistemological discussions can be viewed mainly as related to either of the contexts. We propose that in the context of generation we study the problems pertaining to the production of applicable pieces of knowledge like concepts, models etc., and in the context of application we study the philosophical problems relating to the true or false application of concepts, models etc. More details and philosophical motivations/reasons for introducing the distinction will be found in the next two chapters. It is sufficient here to note that this distinction is not drawn on the same lines as that of Reichenbach's, or the usual defenders of discovery program.

We do agree with Hanson that the distinction should be maintained, but we do not agree that there are distinct *kinds* of logics for discovery/invention on one hand and justification on the other. The logic of discovery cannot be a 'machine' driven process that only yields 'products', without having any say anything about the value of the products. It is also a conscious process, in the sense that it involves volition. (Recall the discussion of Part-I, where it is noted that it is essential for a method to be a voluntary process.) It is a process that is constantly under the gaze of conscious reason, such that certain epistemic values get implanted—imposing constraints—in order to ensure that no gibberish would be generated. Therefore, we cannot have a method that *has* epistemic value and at the same time does not have a 'capacity' to filter its products. We shall argue that nothing be counted a logic of discovery if it has no in built validating reasons. Therefore, no epistemologically relevant method can be free of justificatory/validatory role. Hence, we disagree with Hanson and others who thought that discovery logics are free of validating reasons.

However, Achinstein's claim is not easy to understand in relation to our position. Because we see on the one hand that the distinction should be collapsed on the grounds of justification, but on the other hand we see a need to make a distinction. Since, we agree with him that validating reasons do enter into the context of discovery, we find affinities with him. But, he goes to the extent of suggesting that the same justificatory reasons occur in both contexts, and therefore the distinction be collapsed.[20] On this point we disagree, because the nature of *epistemic values* that enter into the context of generation differ markedly from the kind that enter in the context of application. Since this notion of different epistemic values forms part of the major claim of the thesis, we will attend to it in detail below.

To summarize the points made in this section: The distinction proposed by Reichenbach need not be challenged, though a terminological change would make it less confusing. We have proposed that the epistemological context be further divided into the context of generation and the context of application.

4.4 Theory Ladenness of Observations

Apart from the arguments based on the distinction of the two contexts, there also is another angle of attack on the generativists based on the thesis of theory-ladenness of observations. This is targeted against the generativists', especially inductivists', belief that true scientific theories can be inferred mechanically from an exhaustive collection of facts gathered without any theoretical preconceptions. It is also against the positivist's view that a scientific theory contains observable and theoretical elements, and that it is on the basis of the former that the latter gain epistemic support. We have presented the argument of Popper in the previous chapter.

Responding to the objection that all observations are theory-laden Marcello Pera says that the hypothetico-deductivists commit the fallacy of assimilating assumptions into hypotheses. He calls this the transcendental fallacy.[21]

To see the fallacy, Pera says, it is necessary first to clearly see the difference between assumptions and hypotheses. The belief that there are regularities in facts, the belief in causality, simplicity etc. are assumptions with which scientists begin their enterprise. Assumptions are part of the foundation of the enterprise, whereas an hypothesis is provisionally formulated, and is about relating one phenomenon with another. Hypotheses have empirical content, while assumptions are devoid of it. Assumptions are properly called zero principles, for they do not refer specifically to any fact.

Pera defines the 'transcendental fallacy' as the misguided step of confusing the plane

[20] Cf. Achinstein 1971, *Law and Explanation*

[21] Marcello Pera 1981, 'Inductive Method and Scientific Discovery' in Grmek 1981. p. 147.

of *a priori* conditions with that of the empirical contents made possible by the former.[22] Thus assumptions cannot be assimilated into hypotheses. The upshot of this discussion is that only assumptions and not hypotheses logically precede observations. Pera's argument leaves open the question whether hypotheses logically precede observations.

We think that theory-ladenness of observations should not be considered a valid objection against a logic of discovery, let alone inductivism, because of at least two reasons: one pertaining to the context of genesis, and another pertaining to the context of development. Even if all *scientific observations* are made later to the possession of a theory, we cannot conclude that there cannot be *non-scientific observations* that are scientifically relevant in the context of the genesis of scientific theories. (The distinction between scientific and non-scientific observations will become clear after the distinction between structure dependent and structure independent observations is introduced (See §6.2 page 163) If there exists a relation or an inference pattern connecting the prior non-scientific observations and the scientific observations, explaining the process of theorization, then the point that all observations of scientists are theoretical cannot be taken as a valid objection against a logic of discovery. This is because the nature of observations then would be different. We also consider that all science, and all scientific observations are necessarily theoretical. The distinction between theoretical and non-theoretical, scientific and non-scientific must be maintained in the context of the genesis of science from non-scientific precursors. Since we believe (and also propose) that there is a *non-inductive pattern of inference* that can explain theorization, the above mentioned objections have no implications for our thesis.

The second reason in the context of development of science is that 'theories' can beget 'theories'. Though we will not say that *omnia theorea ex-theorea* (all 'theories' come from pre-existing 'theories'), we will demonstrate below that new theoretical structures can come from pre-existing theoretical structures. If there exists a significant distinction between theoretical and non-theoretical structures, then there must exist a manner by which we can construct theoretical from non-theoretical precursors. Traditionally the believers of the analytico-synthetic epistemological theme always attempted to understand how we move from the known to the unknown. In Part-I, we have observed this in detail. No one who believed in the theme held that knowledge or theoretical knowledge can be built either from a *tabula rasa* or entirely from rational sources. We begin from where we stand, and based on that footing we further go on climbing. Popper is correct in criticizing Lockean empiricism, but his criticism does not stand against the analytico-synthetic theme, which we will also defend

[22] *Ibid.*

with certain renovations. Therefore we think that the argument based on theory ladenness may be pitted against inductive accounts of theorization, but not against those generativists who consider that all scientific knowledge is theoretical and theories can give rise to theories.

Thus, we see two movements: [1] Moving from non-theoretical elements to theoretical elements is possible in the context of genesis. [2] Moving from pre-existing theoretical elements to new theoretical constructions is possible in the context of development. These two steps are illustrated in detail in the case studies in Part-III. However, it should be pointed out to the reader that as the thesis has developed, the need to avoid the expressions 'theoretical' and 'non-theoretical' has been increasingly felt. Though we are not entirely successful in the present work, we see the distinct possibility of avoiding the ambiguous terminology in favor of a neutral one—structure dependent and structure independent (§6.2 page 163).

Another clarification needs to be made. It is one thing to say that all *scientific* observations are theory-laden, and quite another thing to say that *all* observations are theory-laden. While we see the point of the former assertion, the latter is far from being true. The latter can be *made* true by holding the position that all concepts are theoretical, which however cannot be held along with the belief that scientific knowledge is epistemically different from non-scientific knowledge. The distinction between scientific and non-scientific knowledge cannot be held without a distinction between two kinds of concepts, the scientific and non-scientific concepts. For example, when Popper proposes the demarcation criteria between science and non-science, ultimately it is the nature of the concepts that determine their scientificity, which for Popper is falsifiability. This is because a concept that has no empirical content, when employed in a statement makes the statement unfalsifiable. Therefore we see that even in Popper's model there is an underlying classification of concepts that yield statements that are falsifiable and non-falsifiable (scientific or non-scientific). Accordingly Popper must either concede a distinction between two kinds of concepts, or develop a distinction between concepts and theories.

Popper's demarcation criteria cannot sufficiently capture this requirement. For falsifiability is too weak a notion to capture the character of scientific knowledge. The reason to be stated is different from Quine's objections, but related to it. It is possible to have isolated statements that pass the criterion of falsifiability and not embedded in any system of statements, but such falsifiable statements cannot be called scientific. Falsifiability can become a delimiting factor, if it is worthy at all, only after the candidates of scientificity, whatever they may be, pass the test of *systematicity*. A statement, for example, can be regarded as systematized, if and only if it is found embedded in a system of logically compatible statements. A non-trivial notion of falsifiability, therefore, requires or should presuppose first a division between systematic and non-systematic knowledge.

Consequently we can say that all scientific observations depend on systematic conceptual connections, and, in this sense only, all scientific observations are 'theory' laden. This position rules out free observations only in the realm of science, while it holds that free (i.e., non-theory laden or non-system laden) observations are possible in the non-scientific realm. In the context of the genesis of science, the problem of how free observations enter into a system that is already existing, or/and form into an independent system becomes relevant. Systematization is known to be an essential component of scientific knowledge and cannot be neglected. We will attend to the problem of genesis in the first two Case Studies.

We present here another example to show that observations can indeed be distinguished on the basis of a distinction between pre-scientific and scientific. Tycho Brahe's observation in 1572 of the exploding star in Cassiopeia was an important discovery in the history of astronomy.[23] The prevailing theory at the time of observation was the Aristotelian doctrine that celestial matter is unchangeable. If it is true that theories necessarily guide observations, then how could Tycho observe an exploding star, contrary to the Aristotelian doctrine. 'Explosion' is a concept familiar to even a 'savage' mind. Once a meaningful concept is formed in one's mind, one can predicate it of any thing whatsoever. Thus it is not necessary to have theories, let alone scientific theories to describe facts, what we need to know is whether a state-of-affairs needs the application of a scientific concept or not. After all scientists also use non-scientific concepts in contexts where they properly apply.

Thomas Kuhn's examples also suggest that the term 'theory' is used in a place where he should have used the term 'concept'. Kuhn uses 'concept' and 'theory' interchangeably. This can be demonstrated with reference to his own contention that what is necessary to discover new phenomena is an alternative conceptual scheme. A conceptual scheme need not be theoretical. In the case study on chemical revolution in Part-III, we have supported our claim in detail.

Many examples in biology also support our distinction between pre-scientific and scientific concepts. For example only after the discovery of the notion of phylogeny (which is a scientific concept), were the earlier alpha systematics replaced by phylogenetic systematics. We consider alpha systematics as pre-scientific.[24]

Therefore it is one thing to say that all observations are concept laden, which we also hold, but another thing to say that all observations are theory laden. We therefore think

[23]Cf. R. Blackwell 1969, *Discovery in the Physical Sciences* p. 37

[24]M. Bunge 1967, *Scientific Research I: The Search for System,* p. 83.

that the thesis of theory laden observations cannot be an argument against generativism.

4.5 Epistemology of Discovery

In this section we shall respond to Larry Laudan's challenge. We think that meeting Laudan's challenge is essential for a generativist, for it really sets the direction towards which any philosophy of discovery must turn.

In Laudan's paper, mentioned above, there are two most important points that need our attention. One of them is best stated in his own words:

> The historical vicissitudes of the generators' program for establishing a logic of discovery are utterly unintelligible, I submit, unless one realizes that the *raison d'etre* for seeking a logic of discovery was to provide a legitimate logic of justification.

We agree with him that traditionally logic of discovery was expected to provide a logic of justification. Therefore, any defense of a logic of discovery must contain an argument to show how it can also provide justificatory reasons. If it is not necessary for a logic of discovery to play a justificatory role, then even if there is a logic of discovery, it is of the least consequence (if not of no consequence) to philosophers or epistemologists.

The second major point Laudan makes is that generativism has ground only if scientific knowledge is infallible. Since it is increasingly realized that scientific knowledge is not infallible generativists have no ground to revive a logic of discovery. It is abandoned for good. We think, that here lies the soft belly of Laudan's argument.

To see that Laudan is weak on this point, we need to show that the proposed connection between generativism and infallibilism is not *necessary*. It is further necessary to show that no logical or philosophical constraint prevents one from being a fallibilist *and* also a generativist.

What is the nature of the connection between infallibilism and generativism? Is it logical or historical? If the history of philosophy shows that all generativists are infallibilists, does it follow that they should remain so? What prevents them from being generativists and fallibilists? Is this an irreconcilable option? Unless one can show that the combination of fallibilism and generativism is not a logical possibility, Laudan and other consequentialists cannot assume that logic of discovery has been abandoned for good.

Further, what is the nature of consequentialism and generativism? Is it possible for some one to believe in both at the same time? Is this too an irreconcilable option? We think that generativism and fallibilism, as well as generativism and consequentialism are reconcilable, provided that we can formulate a framework in which they can be shown to be compatible. There exists only one way of showing that the stated option is impossible, which is by defining the terms in such a manner that together they become impossible. But this is not a significant option.

We agree with Laudan that infallibilism has been, and can be, more or less abandoned, but not generativism, for they are separable. We shall not throw out the baby with the bath water. From a thesis of the fallibility of scientific knowledge, it does not follow that the logic of discovery can be abandoned.

Laudan might respond saying that generativism is linked to justification, otherwise it would be "utterly unintelligible". The position of generativists was intelligible because the logic of discovery was conceived at the same time to be a logic of justification. Traditionally generativists *held* that (1) their ground consists of statements known to be true, (2) it is from what is known that we ascend to new knowledge, and (3) new knowledge is justified knowledge only because it is constructed from true knowledge. They believed that truth 'ascends' or 'circulates', to use the expression of Lakatos, from the basic statements to the newly arrived statements. This knowledge, it was believed, stands in need of no further justification. That is, simply, generativists believed in infallibilism. But after the advent of fallibilism this scheme of things fell apart, because there seemed to be no inferential link, which at the same time conserves truth between the ground knowledge and theoretical knowledge.

As we see fallibilism and generativism did not appear intelligible because they (traditional generativists) *believed* that truth is manifest; because they *thought* that truth circulates up-stream. Since today we do not believe in the dogma that truth is manifest, we could adapt fallibilism. But why abandon generativism?. Is generativism a notion that is defined historically? If so there is nothing that prevents one from changing its meaning. So we can renovate or revive generativism in such a manner that fallibilism and generativism would become intelligible. We see that this is indeed possible, and can wake up generativists to a new dawn!

The renovated generativist framework can be based on a distinction between the epistemic values attributable to concepts, conceptual structures, definitions, mathematical structures and models, such as meaning, closure, equilibrium, invariance, and symmetry in the *context of generation*, on the one hand, and those of truth and falsity attributable to scientific assertions in the *context of application* on the other. Thus, our proposal is based on a distinction between two qualitatively different kinds of epistemic values those that validate non-assertive structures which are vital in the context of generation, and those that validate assertive structures which are vital in the context of application. The relevant values in the context of generation can be generalized as semantic value while the latter can be generalized as veracity or truth.

The history of ideas also provides evidence to the fact that false theories are not gibberish. Aristotle's ideas on physics, for example, may not be believed today because they are false, but on this token we cannot suppose that they are gibberish. Since we can understand those false sciences, but not believe in them, it is clear that it is one thing to make sense and another thing to believe that that which is sensible is also true. It may be true that most of the time people, in some uncritical mood, may not take the trouble to distinguish carefully the sensible ones from the believable ones. But logically the two modes of thinking, making sense and believing are different, because the former does not force us to the latter. Because belief involves assertion, we can say that all that is meaningful need not be asserted.

The distinction that we are suggesting here is similar to the well known distinction made between *knowledge* and *understanding.* While many philosophers are comfortable with the term "knowledge", Stephen Toulmin, for example, is not happy with it. He prefers the use of the term "understanding" in place of "knowledge". As he writes:

> The basic process of scientific change has been described as 'criticism and growth of knowledge'. Rather, it should be characterized as 'criticism and improvement of understanding'. ... *The alternative phrase 'improvement of understanding' ... has the merit of redirecting our attention, away from the accumulation of 'true' propositions and propositional systems, and towards the development of progressively more 'powerful' concepts and explanatory procedures.*[25]

Though we will not go to the extreme of claiming that understanding alone is important, we will say that understanding is a necessary condition of true knowledge. How can a method of understanding, if there is one, be epistemologically irrelevant? We therefore think that it is one thing to say that there is a logic of discovery of new meaningful ideas, but quite another thing to say that there is a logic of discovery of true statements. It is easy to see that here our presupposition is that only statements/assertions can be either true or false, and not ideas or concepts. The latter are either meaningful or not.

The question naturally arises: Is epistemology the study of the meaningful? In the case of scientific knowledge the meaning of a term is always structure-dependent. The kind of structures that we come across in science, such as symmetric and equilibrium models, have undoubted epistemic value. It may be an open question as to why and how these

[25] Stephen Toulmin 1972, "Rationality and Scientific Discovery" *PSA*, pp. 390-99

structures have epistemic value. *Why is it the case that all great theories of science are models of equilibrium and/or symmetry?* On the face of it, symmetry and equilibrium are not equivalent to truth. But it is a historical fact that they have an intimate relationship. We are not suggesting that all symmetrical and equilibrium models would come out true, but we are raising the question: why are all true scientific theories symmetrical or balanced models? And most scientific terms that gain significance gain it from precisely those structures that have these properties. Therefore, we should not consider veracity alone as epistemically relevant. After all *there are certain conditions that make us realize veracity*. However instrumental these conditions may be, they remain important insofar as the appraisal of true scientific knowledge is concerned.

We think very strongly that it is possible to articulate a method of constructing models that would have the properties of invariance, symmetry, equilibrium etc., and that such a method would be based on an independent logical relation called *inversion*.

Based on these arguments we conclude that though Laudan is right in his historical thesis that all traditional generativists were infallibilists, and the advent of fallibilism led to the development of consequentialism, pure consequentialism is not the only alternative of fallibilism. Generativism, fallibilism, and consequentialism can be reconciled within a coherent analytico-synthetic framework.

In the following sections of the chapter, we shall elaborate and clarify the character of ampliative logics.

4.6 Ampliative and non-ampliative inferences

Peirce classifies inferences into ampliative (synthetic) and non-ampliative (analytic). Induction and abduction are included under ampliative inferences, and deduction is included under non-ampliative.[26] We will also follow this classification of inference, but will include a few other species of inferences, such as inversion and abstraction, under ampliative inferences.

The use of the term 'logic' for the various species of ampliative inferences would demand clarification, for most logicians, and a large number of philosophers, do not consider any of the mentioned species of ampliative inferences as proper logics. They consider deduction as the only species of logic, and in most cases deduction and logic are regarded synonymous. The main objection to calling induction etc., synthetic inferences is that they have no valid inference pattern. We shall argue below (§4.7 page 105) that this view is shortsighted and not acceptable.

[26]K.T. Fann 1970, *Peirce's Theory of Abduction* p. 7.

An ampliative or synthetic logic, it is usually understood, is that inference where the premises do not contain the conclusion. The way it is stated implies necessarily that ampliative inferences are invalid. In fact most scholars, including those who argued for ampliative methods in epistemology, such as for example Nickles, consider them to be indeed invalid. We will however differ from such a view. We will elaborate our argument below.

It is found necessary to amend the usage of the terms to our advantage by giving them a new definition. The main reason for this is that the characterization of ampliative inference has so far been dependent on the characterization of what non-ampliative logic/s are. Therefore, we attempt to give a positive characterization in independent terms, i.e., in ampliative logic's own terms.

We will use the term 'ampliative' for only those patterns of inference that generate new concepts by abstraction, or construct new concepts out of the available ones by certain logics of abstraction, such as induction and inversion, to be defined below. We consider that an ampliative inference must involve the non-assertive mode, and therefore will be necessarily conceptual. Being conceptual does not mean that no premises would be present in the inference, but that the outcome or 'consequence' would be conceptual in nature. Accordingly ampliative inference would be rated either as sense preserving or not. Unlike non-ampliative logic, such as deduction, which are truth preserving, ampliative logics are sense preserving. Thus, ampliative inferences would be rated in terms of meaning and not truth. They are valid, if and only if no gibberish is produced. Further constraints can be specified with respect to the kind of ampliative inference, such as whether it is induction or inversion.

We shall first provide below an argument to show that ampliative inferences such as induction cannot be called invalid.

4.7 Induction and Validity

Induction has been attacked as a logical process by many because inductive inference is not valid. The problem of induction consists in whether inductive inferences are justified/valid, and whether the conclusions arrived by inductive inference are certain. We think that this is based on an incorrect formulation of what induction is.

In all these discussions the terms 'valid,' 'follows' 'inference' etc., are used in a sense that is determined by deductive logic. Therefore, the problem of induction, we shall argue, emanates from a predetermined notion of deductively valid inference.

We usually define valid inference as that inference when it is impossible for its premisses to be true and its conclusions false, i.e., false propositions should not follow from true propositions. How many *kinds* of inferences do we know that pass this test of validity apart from deductive inference? None. Is there anything more to the definition of valid inference than this? Deduction is defined as: A valid argument in which it is impossible to assert the premisses and to deny the conclusion without there by contradicting oneself. There are various valid *rules* of inference within deductive logic, but all of them are provable as theorems in a (axiomatic) deductive system. Therefore, whatever is deductively valid is also a valid inference, and vice versa. That is to say that the notion of deductive validity is equivalent to a notion of validity.

Any inference that satisfies this condition of validity becomes *non-ampliative.* It may not be wrong to say that the validity condition ensures precisely that a valid inference be non-ampliative.

Therefore, when we say that inductive inference is not valid, we are not saying anything more than that it is non-deductive, or that it is *ampliative*. It says that spades are spades or non-spades are non-spades. Whoever thought that induction is non-ampliative? It is its ampliative character that makes it stand distinct from deduction. How about saying that deduction is not valid (inductively valid) because it is not ampliative? Surely it would be preposterous.

Having captured the essence of *a* logical pattern, such as deduction, we have been precluding other *'living'* patterns of inference in our thought out of not just that class which satisfies the essence of deductive inference, but from the entire class of inferences. This narrow approach has been preventing us from capturing the essence of other forms of reason, that we otherwise regularly employ.

We therefore think that the claim that induction is an invalid inference is a trivial one, and cannot be an objection against induction. Unfortunately we seem to have so far no definition of validity that is independent of deductive logic.[27] A deductivo-centric notion of validity cannot be imposed on induction.[28] If deductivists have to show that induction is invalid, then they must first prove that there exists a notion of validity, that is independent of and not determined by deductive logic. Till then their claim that induction is invalid remains

[27]How reasonable would our argument be if we say that plants are non-living because they don't have animal essence? In fact that would sound quite 'valid' at a time when we would equate life with animality. But when we find such qualities such as irritability outside the animal kingdom, we would naturally include plants too along with many other beings, as living. Here, in the biological context, we could do so because and only because, we have a definition of life that is independent of animality.

[28]Ironically it is deductivists who are behaving inductively. They had a class of inferences, which are found to be characterizable by a common abstract notion of deductive validity, based on the principle of non-contradiction. They called that class the class of valid inferences. Then they declared that all deductive inferences are valid. Any inference that does not fall in this class of valid inference is non-deductive, and also invalid, for it amounts to the same thing.

a statement of the kind that non-spades are non-spades.

Do we have laws or principles of thought other than those already incorporated in deductive logic? If we do, then we can claim to have obtained a ground for thinking of a logic independent of deductive logic. The strength of deductive logic is based on its ground in tautologies, the forms that need no content to validate them. We think that it is possible to have similar grounding for ampliative logics. In what follows we will present our attempt in this direction.

4.8 Induction as a Logic of Abstraction

We have argued in the above section that the usual accounts on induction suffer from negative characterization, especially the arguments against induction based on validity. In this section we shall first identify the distinguishable aspects involved in what we usually call inductive inference and then show that abstractive induction, an aspect of induction to be distinguished, can be shown to be based on a fundamental principle, which we call the *principle of excluded extremes*.

We regard induction as an *independent* mode of ampliative inference for *arriving* at knowledge. Philosophical problems of induction, according to our observation, arise due to the characterization of induction as an inference that produces generalizations, that too universal generalizations. Thus it is commonly characterized as a method of generalization moving from statements about individual cases to an universal generalization. This dominant statement mode has moulded the popular notion of induction. Having already stated that we regard ampliative inferences as rooted in conceptual operations of thought, induction being ampliative inference should also be rooted in conceptual operations, rather than statemental. This is found necessary not only for a consistent view about ampliative inferences in general, but also for a positive characterization of induction. The statement based characterization, we will see, has misled philosophers, who could not see the principle upon which it is based.

Any ampliative reason must be a species of abstraction, for it is only in abstraction that we can go beyond what is given. It is after all the conceptual richness that makes a certain statement more powerful than others. Induction, being ampliative, must necessarily be *a* method of *abstraction* or *conceptualization*. While abstraction is a necessary aspect of it, another possible mode is associated with it, which is contained in the usual characterization of induction as a method of generalization. It may also be called a method of generalization. We prefer calling it ‘abstraction’ because abstraction, we will show, is a necessary aspect of induction, while the generalization function of induction is a possible, and not necessary association. This characterization also allows us to move away from the perniciously dominant statement mode of thought. To avoid confusion we will use the term *inductive abstraction*. In what follows, we shall clarify and justify our proposal.

Deduction, we all know, is characteristically non-abstractive.[29] For it does not start from instances to instantiatables. Its characteristic movement is from instantiatables to instances. It is a method of obtaining particular statements (instances) and hence it is counteractive to abstraction. Induction involves, as we all know, a movement from particulars to universals. We would prefer a neutral terminology and hence would say that induction is a movement from instances to instantiatables.

Deductive inference is called valid because it is truth preserving. What should an abstractive inference preserve for it to be valid? Truth or meaning? We propose that *an abstractive inference be regarded as valid iff it is impossible for it to generate gibberish*. Since meaning or meaninglessness are the proper attributes of concepts, which are 'products' of abstractions, this is a natural choice. Since the products can take these dual values, accordingly the process can be characterized as conducive to meaningful concepts or meaningless concepts. Thus, we can say that a method of abstraction can be either + (valid) or - (invalid), depending on whether we obtain an instantiatable concept or not.

One very common method of abstraction is the method of comparison, which can be defined as follows. The *method of comparative abstraction* is defined in the familiar sense, as a method of eliminating differences and '*elevating*' similarities from a given set of object*s*. This method of comparative abstraction can be rooted in the *principle of comparison*, which can be explicated as follows:

(1) It is impossible to compare an object with another in a world where every object has all properties.

(2) It is impossible to compare an object with another in a world where no object has any property in common.

(3) It is possible to compare an object with another only in a world where objects do not share all properties, and where objects have some property in common.

These principles describe situations in three possible worlds, and say that the world (1) and (2) are so extreme that comparison makes no sense. In world (1) all objects are identical,

[29] We are not saying that deductive logic is not abstract. It is after all an abstract form of an aspect of reason. What we are saying is that it is not employed for the purpose of abstraction.

and in the world (2) every object differs from every other object, a world of unique individuals/identities. If Plato would try his method of dialectic in the first world he would obtain only one Universal, which cannot be further analyzed into any genera or species. Similarly if he tried his method in the second world, he would obtain as many Universals as there are objects, and would again fail to find genus-species relations among forms.

However, if Plato would try his method in the third world, he would not only be able to get Universals that are not as few as one, but also not as many as there are objects. He would be able to find many tokens for each type, and since in this world objects share properties, it is possible to obtain genus/species relations between the various types. Therefore, the method of comparison, a method that works by finding differences and similarities among objects, will not *yield* results in the worlds (1) and (2), while in the world (3) it would.

It is easy to see that (1) and (2) are absolute contraries, and therefore cannot be true at the same time, i.e., they cannot be put together to yield any world. If we exclude the two extremes we get a world which is a world of excluded extremes. The principle of comparison has application in this world of excluded extremes, therefore this principle can be called *the principle of excluded extremes*. Compare it with the principle of excluded middle, which is also called the principle of non-contradiction. We will regard both these principles, i.e., the principle of non-contradiction and the principle of excluded extremes, as equally fundamental in their own right. Each is independent in the sense that one cannot be deduced from the other.

If we make comparison a necessary condition of knowledge (as defined by Plato—definitional or analytical knowledge), then we can rephrase the principle, again in the explicated form, as follows:

(1) It is impossible to *know* in a world where every object has all properties.

(2) It is impossible to *know* in a world where no object has any property in common with the rest.

(3) It is possible to *know* only in a world where objects do not share all properties, and where objects have some properties in common.

We will base the method of inductive abstraction on this principle of excluded extremes (or the principle of comparison). Note that there exists a difference between the method of comparison and the method of inductive abstraction. The difference is that, for the latter it is necessary that two or more properties show a possible linkage by being present in more than one instance, i.e., correlational input is necessary, while for the former, correlations between properties is not necessary. Therefore we can say that the method of comparison is presupposed in the method of inductive abstraction.

Let us also note that abstraction requires more than one object. Since differences cannot be eliminated and similarities cannot be elevated from situations (1) and (2), it is also impossible to *count* in those worlds. This is so because counting presupposes at least one similarity in a set of objects, and also that all objects within the set be dissimilar in some respect, including spatio-temporal respects. Since mathematics is impossible without numbers, *no mathematics is possible in a world where comparison is impossible.* Therefore, the world where comparison and induction are possible is also the world where mathematics is possible. The other ampliative method, namely the method of inversion (to be introduced in this thesis), is possible only in a world where mathematics is possible. Therefore, *inversion is impossible if induction is impossible.* Thus, in the methodological framework that we are proposing induction occupies a basal place. However, we shall see that the ampliative potential of inversion is far greater than induction.

Since a possible *abstraction* is to move from object*s* to their characterizables, by isolating the similarities and differences, an impossible abstraction is to move from *nothing to anything*, i.e., from 0 to 1. This says merely that it is impossible to form a type if there aren't any tokens, and that it is possible to form types only if there are tokens. It is in this logical circle of types and tokens that the method of abstraction is rooted. Breaking this circle is not the objective of this essay, and therefore we shall not get into other possible philosophical problems at the moment.

We know that the essence of deduction consists in the following statement: It is impossible for the premisses to be true and its conclusions false, i.e., false propositions should not follow from true propositions. In a similar manner, we can capture the essence of abstraction in the following statement: *It is impossible by comparative means to obtain a type which has no tokens.* It is in this statement that the *certainty* of ampliative logics resides. The method of inversion, we will see below, has the potential of obtaining types that have possible tokens even before the tokens are given to experience. And it is here that the most significant difference between induction and inversion resides.

Abstraction imposes the constraint such that unnecessary proliferation (amplification) of types without tokens will not take place. Deductive logic, on the other hand, imposes constraint in order to remain only in the world of types which are *assumed to have tokens*, which in other words is to say: never move from truth to falsehood. We are assuming the following general interpretation of a statement: A statement is nothing but a relation between type/s and token/s. A true statement is that which *asserts* that a token belongs to a type; Since abstraction begins from tokens that are *typable* a valid abstraction is that which yields a type. While abstraction is designed to yield meaningful types, deduction is designed to preserve truth. Therefore, the former has to be ampliative, while the latter non-ampliative or conservative.

It is important to see that abstraction is ambiguous, for there are two possible inferences. One is that we can generate a type that has meaning, and second is that it immediately yields a statement in that very context of generation. This is easy to understand because for every type thus obtained, a statement asserting the type-token relation, can be constructed. Thus, every abstraction necessarily yields a statement which is a trivial application of the obtained type to those very tokens of the context of generation. For example, an abstraction leading to the type 'red' applies truly and trivially to those tokens that are red. Thus it is important to note that no context of generation can be actually free of the assertive mode. The attempt to separate the two modes—assertive and non-assertive—is based on logical reasons. The logical reasons will be made clear below.

Every object that deduction operates on must be either a statement asserting a relation between type/s and token/s, or a statement asserting relations. Unlike deductive inference, which is always a logical operation on statements, a method of abstraction does not restrict its objects to statements. We shall look at abstraction based on relations in the next chapter.[30] Having shown how a method of abstraction can be understood as grounded in the principle of excluded extremes, we shall now turn our attention to characterize *inductive abstraction*.

The objects upon which inductive abstraction can operate can be anything typable—statements, concepts, percepts, things, etc. We can abstract upon abstractions, at a level higher than the method of comparison. This higher level of abstraction or comparison can be called inductive abstraction.

The problem with the traditional treatment of induction lies in interpreting it as a method of generalization. Given that x_1 is ϕ, x_2 is ϕ, x_3 is ϕ, . . . , we infer, therefore 'all xs are ϕ, or in the probabilistic tone 'all xs are probably ϕ.' We are not claiming that generalizations cannot be obtained, by radically disagreeing with the traditional treatment. We are however saying that induction, insofar it is an ampliative inference, should not be interpreted as a method of generalization, but should be interpreted primarily as a method of

[30]Different kinds of statements can be obtained by the different possible associations between type/s and token/s. At the moment we are not diverting our attention to other methods of deduction and other possible methods of immediate inferences.

abstraction, because the products of this ampliative inference are new concepts or new types. Further we shall claim that since this abstractive aspect is based on the principle of excluded extremes, this aspect of induction is necessary. Since types are not to be rated (valued) as true or false—but as tokenable or not—we have another reason to separate the abstractive aspect from the generalization aspect. Since every type that has tokens is nothing but a meaningful type—meaningful concept—the method of induction has to be rated accordingly as *meaning generator*, and not merely as a general statement generator. We shall elaborate.

An abstracted type can not only be applied to those tokens that were part of the context of generation, but also to other tokens that are not part of the context of generation. The ampliative power of abstraction lies in this projectability of concepts. Whether the concepts are obtained by the method of comparison, or by the method of inductive abstraction, the ampliative power is by virtue of its projectability. However, the concepts obtained by inductive abstraction may or may not find more application in contexts other than the context of generation. Just on this count that further projection of the concepts thus obtained cannot be certain, the inductive abstraction does not lose its epistemic significance, or its capacity to yield *fallible knowledge*. Having already abandoned infallibilism, we are no longer looking for any methods that can generate infallible knowledge.

Projectability says merely that it is possible that all tokens of X are also the tokens of ϕ. Or in other words, it says that *it is possible that* $X\phi$—the complex correlated type—*can (and not, will) have common tokens (instances).* The conjunction of the two types, $X\phi$, projects possible knowledge about the world, and not necessary knowledge.

One might say that we are trying to save induction by eliminating the necessary mode of induction and rephrasing it in the possibility mode. We will show below that it would be utterly insignificant for induction to be in the necessary mode, because, we shall argue, it is *necessary* that induction operates in only the *possible* modality.

We have stated a few paragraphs above that the abstractive aspect of induction is necessary because it is based on the principle of excluded extremes. In the paragraph above this, we said that it is necessary that induction operates only in the possible modality. Aren't we presenting a contradictory view of induction? We think that there exists no contradiction, because since induction is based on the principle of excluded extremes the zone where inductive operations are significant is that where possibility is the 'order of things'. The point made here is that it is impossible that induction could operate significantly in the necessary mode.

To see why it is significant for inductive abstraction to operate in only the possible modality, let us construct a world such that all inductive generalizations come out necessarily true. That is whenever an object was known to have any property ϕ, ψ, ... , all such objects would always possess that set of properties. What kind of world that can be? If we find an object bearing properties ϕ and ψ for an entire day (or for any duration) in that world, according to the rule of induction which never fails in that world, we can make a generalization that that kind of object will have ϕ and ψ. In case those objects are found the next day without any of the properties, then the induction would fail. But we have stipulated that the world be such that induction can never fail. Therefore it is sufficient for the knowledge maker in that world to keep on looking and noting all correlations between properties. If any two properties happen to be together they would be together forever. All that our ideal scientist need to do is just to make note of all instances of linked properties. Can we expect any contingent events in that world? Will it be possible for an object to take on a property today, another tomorrow? We will have to answer negatively because if such an event is possible then induction would fail in that world.

To make things clearer let us construct another possible world, where induction would *never* come out true. All relations would be contingent in this world. The only possible way of knowing in that world would be to experience individually every thing, otherwise no complete knowledge would be possible. The knowledge maker cannot infer anything from anything, for no connection is necessary.

The world that we live in is neither of the first kind, nor of the second kind. It is not of the first kind because we have so much evidence to prove that induction, interpreted in the necessary mode, fails. It is also not of the second kind because some of the generalizations achieved inductively have not been proven false. Therefore, the world we live in is a world where inductive generalizations come out true in one case and fail in another case. Our objective here is not to explicate the nature of this world, which is a metaphysician's task. Since we are playing methodologist now, our objective is to show that induction would be significant only in the middle world, whether it is ours or not. Why? Because we have seen that if inductive generalizations are always true, then it would be insignificant as a method. Since input to induction comes exclusively from experience, under the two above mentioned cases induction has nothing more to say than what experience gives. All that we need is to experience and that is the end of it. There is no possible role for any inference or method under such situations. The same is true of the second case above, where induction is always a failure.

We need a method for searching truth only if truth is not manifest. We need a method like induction, in the non-necessary mode (with possible projectability), only in such a world where mere experience is not sufficient. Since the middle world is such a world, induction makes sense only there. Since experience is insufficient we need to make some inferences to reach the world. The objective of inductive inference is to know the *possible linkages*, because there exists a *chance* that some might turn out to be necessary. We therefore think that induction is a method by which we can arrive at possible linkages between properties, namely correlations. Through the method of induction we cannot arrive at certain or necessary knowledge, but only possible and fallible knowledge.

Therefore a proper inference of induction should be: Given x_1 is ϕ, x_2 is ϕ, x_3 is $\phi \cdots$ we inductively infer that

(1) It is necessary that $X\phi$ is a meaningful conception.

(2) It is possible that all xs are ϕ.

(1) is inductive abstraction, while (2) is inductive generalization. We have included both under one head of induction because they are obtainable from the same input. Here lies the ambiguity of (the context of) induction.

The most important epistemological reason for separating the abstractive component from the generalization component of induction is the following. In the event of a counter instance that falsifies (2), (2) alone would be rejected, and this act of falsification would not affect (1). The reason is obvious. Since (1) is a conception, it cannot become meaningless, i.e., cease to be a valid notion just because the properties are not necessarily linked. The only condition of concepts to be meaningful is that they have instances or tokens. For example, 'All plants are green' is an inductive generalization. When it is found false by instances of plants that are non-green, the conception of 'green-plant' has not become meaningless. 'Green-plant' continues to enjoy the status of a taxonomic category, for a large number of properties are found linked in this natural class. Neither the cognitive significance nor our knowledge became weaker by the event of falsification. In fact every act of falsification enhances our knowledge of the particular.

One of the upshots of this view is that falsification means nothing more than an incorrect application of a concept. Even if *an* assertion is falsified, the concept involved in the assertion cannot be rejected altogether, if and only if the concept was obtained by inductive abstraction. If the conceptions are obtained inductively, then in any case they are bound to stay, whether they continue to have true projection or not.

Can it happen that the conception thus formed, $X\phi$, becomes meaningless, in which case (1) will not be necessary? The sufficient condition for a concept to have meaning is that it has at least one instance. Since the concept is abstracted from the given (known) instances, it is impossible that the thus formed conception lacks any meaning. Hence the abstractive inference is necessary.

The problem of the certainty of inductive generalizations will be regarded as ill-posed because induction, as shown above, is by nature meant to yield possible knowledge and not necessary knowledge. Is possible knowledge, knowledge? Why not? Indeed it is our thesis that *scientific knowledge is knowledge of the possible.* Scientific knowledge is not a system of necessary propositions and certainly not a system of tautologies. For science to have any empirical value, in the sense that it can make any significant assertions, it is necessary that it speaks about the possible truths, and not necessary truths. This is another way of expressing what, we think, Popper says that falsifiability is the hallmark of scientific knowledge. It is ironical that it was Popper who fought against induction. For a fallibilist like Popper, who believed that truth is not manifest and that science is not a system of necessary truths, the problem of induction should have become ill-posed. Our point is that the problem of induction ceases to be a significant problem precisely because we have already abandoned infallibilism.

Can we call inductive inference a logic? If *validity* of the inference is the criterion of a logic, then we shall have to first agree on the definition of validity. As observed above we cannot agree to a deductivist's definition of validity as truth preserving, and the reasons are specified above. But, since any ampliative inference is sense preserving, we can define a wider notion of validity that can apply to both inferences. A valid inference be that which preserves some value of the objects involved in an inference, whatever that value be. Since intuitively we understand that logic has something to do with thinking pattern, the values to be preserved would be those attributable to any thought object. Statements, and concepts are considered examples of such objects, where for the former the positive value attributable is truth, and for the latter it is meaning. We, therefore, consider inductive abstraction a valid inference, and therefore a logic. Our major presupposition in the thesis is that meaning and truth are logically distinguishable epistemic values. If one were to show that they are not so distinguishable the thesis, needless to say, loses ground.

It should be noted that our objective is not to propose that the inductive method is a sufficient condition of scientific knowledge. To generate scientific knowledge we need certain other methods such as the method of inversion, to be explicated in the following chapters. It is found necessary to defend a version of induction as a possible source of scientific knowledge, because we want to show that the emergence of fallibilism gives new life to induction provided we interpret induction the way we did above. It is thus our objective to show that abandoning infallibilism has no necessary consequence of abandoning generativism. We have tried to argue that it is necessary for an ampliative inference to generate concepts, which are neither true nor false, but are applicable (projectable) or not. Therefore generativism of concepts is indeed a possible philosophical option.

It may be objected that we are surrendering and weakening epistemology by doing so. On the contrary we will show that epistemology would regain lost ground only if we *invert* our epistemological concerns from truth to meaning. Along with the rise of fallibilism in recent times, there is another trend on the rise, called the semantic tradition. Though the historians of philosophy have not yet traced the full development a commendable beginning has been made recently by Alberto Coffa.[31] We cannot here discuss the development of semantic tradition, for that is beyond the scope of this essay.

4.9 Nickles on Discovery Logics

Of all the defenders of generativism the most notable views are those of Thomas Nickles. Nickles has been defending the discovery program for more than a decade, and is possibly responsible for keeping the debate alive. Most of his contributions after 1980 have a common motivation, which is to revive the epistemology of discovery, which—after the advent of consequentialism—has been regarded as abandoned. Apart from Nickles there are many others who form a company of thinkers who think that an epistemology of discovery is possible, and have put forth their arguments.[32] Nickles has reviewed the literature more or less comprehensively. Therefore it would be a repetition to review already well reviewed works. Instead, we will comment critically on the most recent views of Nickles (1990). By doing so we will also get an opportunity to further clarify our own position regarding a large number of questions that we have not considered in the above account. Since most of the views taken by Nickles are based on the several objections against a logic of discovery, it is necessary to respond to them. However, the limitation is that unless our thesis on possible logic of inversion is sufficiently developed it would not be possible to argue our position. Therefore, we shall be content with a summary of Nickles' position, and also a summary of our matching position to give an indication to the reader where are we heading.

[31] Cf. Coffa 1991, *The Semantic Tradition: From Kant to Carnap.*

[32] While C.S. Peirce and later N.R. Hanson have been the classic 'friends of discovery', H. Simon, P. Achinstein, M.W. Wartofsky, S. Toulmin, T. Nickles, E. Zahar, N. Koertge, M. Pera, R. McLaughlin, D. Shapere, P. Thagard, G. Gutting, R. Blackwell, M. Curd, K. Kelly, among many others, have been forwarding various versions of epistemology of discovery.

According to Nickles:

> (1) there is no Logic of Scientific Discovery, but there are logics of discovery! There is no logic of discovery in the sense of a single logic underlying Scientific Method; but there do exists many logics of discovery, even in the strong, historical sense of actual use. (2) While there is no *content-neutral* logic of discovery, there are many rather local, substantive or content-specific methods that merit the name 'discovery logics'. (3) The new discovery logics that emerge in times of major historical breakthrough nearly always postdate the breakthrough. Such a logic is not the cause or explanation of the corresponding discovery; rather, it is a a methodological *part* of the discovery itself. Typically, discovery logics are rational reconstructions of results arrived at by more haphazard routes. They are worked out by critical reflection on how the substantive problem solutions were originally achieved and how these methods might be streamlined or otherwise improved. They are what I term *discoverability* logics. They are idealized discovery procedures—methods that could have been employed to make the original-breakthroughs if (contrary to fact) we had known then what we know now. These discoverability logics reduce problem solving in that domain to routine and can sometimes provide the basis of new, original discoveries.[33]

The epistemological justification for the program, according to Nickles consists in the fact that "an adequate confirmation theory must include a dose of generative justification."[34]

Further, unlike consequential testing,

> generative reasoning flows *from* what we already "know" (so-called background knowledge or positive science) *to* some other claim or problem solution. ... The generative strategy is to provide empirical support by direct construction of the claim from what we already, fallibly know, while the consequentialist strategy ... can only be indirect and eliminative.[35]

Nickles' line of providing epistemic justification is contained in this powerful statement: "*the strongest form of justification is an idealized discovery argument.*"[36] He distinguishes original discovery arguments from *discoverability* arguments. Generative justification consists in offering one or more discoverability arguments.

> The final argument amounts to a *potential* discovery argument in the sense that it constructs the theory (largely) from what is, by now, already known. Hence the counterfactual: had scientists known then what they know now, they could have discovered the original idea in just this way.[37]

[33] T. Nickles 1990, 'Discovery Logics' *Philosophica* p. 901.
[34] T. Nickles 1988, 'Truth or Consequences?: Generative Versus Consequential Justification in Science' *PSA 1988* p. 393.
[35] *Ibid,* p. 394.
[36] *Ibid.*
[37] *Ibid*.

Nickles, however, shares the position held by Pierce and pure deductivists' spirit by conceding that there are no content-increasing (ampliative) valid inferences.[38]

These thoughts deserve complete attention and assessment. Though our comments in this section will be brief we will return to them in the rest of the essay, and finally in the last chapter of the thesis we make a final presentation in the form of a conclusion. In order to make it clear that we take a position different from that of Nickles we shall briefly state the following points that match with the propositions of Nickles.

We claim that, (1) though there are logic*s* of discovery, their number is limited, and is not equivalent to the number of problem situations or domains of inquiry; (2) logic of discovery, or for that matter any logic, has to be content neutral; (3) though it is necessary that all logics of discovery should be capable of providing rational reconstructions, their testing ground is in the context of learning/teaching, for historical context can never be repeated; (4) there are content increasing valid inferences; (5) justification of knowledge is double pronged, the first prong concerns the context of generation, where we go from the known to new knowledge, and the second prong relates to the context of application, where the move is from the new knowledge to its applicability to new situations.

Having already seen the possibility of valid ampliative inferences we disagree with Nickles on the point that no such inferences are possible. Apart from the method of comparison and inductive abstraction as valid inference patterns of yielding meaningful fallible knowledge, we will see how another logic, called a logic of inversion, can also be a valid ampliative inference for construction of scientific knowledge. This logic of inversion will be founded on another fundamental principle called *the principle of included extremes* which becomes the basis of the possible logic of inversion. Since there are possibilities of developing a general notion of validity, generativists should not accept the narrow and deduction centered views of validity.

How many logics of discovery are there? Can logics of discovery be content neutral? These questions are interdependent. If the answer to the latter question is positive, then there cannot be many logics of discovery. If negative then there are many logics of discovery. Because if content determines what method be applied then there are innumerable contents as there are problem contexts or at least domains of inquiry, in the sense of Dudley Shapere.[39] Nickles argument *appears* convincing.

The reason why a completely content-neutral (a priori) method of discovery is

[38] Nickles 1984, 'Positive Science and Discoverability' *PSA* p. 21.

[39] Cf. Shapere 1977, 'Theories and Domains' in Suppe 1977, *The Structure of Scientific Theories,* pp. 518–599.

> apparently not possible for empirical science is that such a rule could teach us nothing about our world. A logic of discovery is an amplification device. Apply it to some knowledge (or to hypothetical claims) and it generates further claims. Since a completely neutral rule is one incorporating no knowledge about our particular universe much less about any particular scientific domain, we cannot expect a neutral ampliative rule to improve on blind guessing. And anything deserving the label 'logic of discovery' must do that.[40]

However, we think that, ampliative logics can, and should be content neutral. We think that the logic of inversion that we are about to propose can be formulated as a content-neutral logic, and therefore reduces the number of logics necessary for discovery.

It is not necessary for a logic to provide any content, what is needed is that it should provide the *form*. However, if it is an ampliative logic it should be providing us the *form of possible formations*. What basis do we have to think that there exists no form or at least few form/s to the methods of formations (constructions)? Most modern, and highly abstract algebraic theories of mathematics are indeed content-neutral.

Nickles' position seems to be challenging Kant's claim that synthetic knowledge is possible *a priori*. He says that he is following the naturalistic epistemologists in denying the existence of a completely neutral logic of discovery. He goes on to show that mathematical theories are not content neutral. He quotes Einstein's famous remark approvingly:

> As far as the laws of geometry refer to reality, they are not certain; and insofar as they are certain, they do not refer to reality.[41]

He argues that the syntactical instrument like mathematics

> can be usefully applied only to empirical domains of knowledge (or conjectures) that are already highly organized in just the right sort of way.[42]

We agree here, but we conclude some thing else. If empirical domains of knowledge are highly organized before syntactic instruments start 'playing', then the challenge for a believer in logic of discovery consists in finding out if there are any method/s of organizing empirical domains of knowledge. The use of the expression "logico-mathematical method" (in p. 909), and interpreting mathematics as a "syntactical instrument", suggests that Nickles considers logic to be necessarily syntactic. We have argued in the previous sections, to disprove this dominant view, that semantic methods (valid methods of ampliation) make sense. Methods of abstraction, including mathematical ones, are not syntactic, but they are methods of constructing the forms that various possible constructions can have.

---

[40]Nickles 1990, *op.cit.* p. 907.
[41]*Ibid* p. 910.
[42]*Ibid*.

The position we will defend is that there are methods of generating meaningful concepts. The input for this method/logic will be taken from what is already known. That way, there is already content to begin with. However, if the method is not determined by the nature of the content, then the method can be regarded content-neutral. Since it is precisely in this sense that deductive logic is content-neutral, we think that a logic of construction can also be content-neutral. Keeping in mind the tension between the ampliation and content neutral logic we will develop the rest of the thesis, and shall return to the issue towards the end.

Chapter 5

Nature and Structure of Scientific Knowledge

It is argued above that generativism need not be and should not be abandoned, for the arguments against generationism are not convincing. It is suggested that ampliative inferences of a valid kind can be articulated. Since it is claimed that the outcome of a valid ampliative inference is a meaningful concept, the following questions would naturally arise: What has meaning to do with scientific knowledge? Is it sufficient for scientific knowledge to be meaningful? What is worthy of science if it is only meaningful and not true? After all metaphysics is also meaningful, but not scientific. How is meaning epistemically significant for scientific knowledge? In order to show that generativism, especially the version of generativism that we are going to defend here, would be a significant proposal, questions of the above kind should be answered satisfactorily. All these questions can be answered more or less satisfactorily by a clear notion of scientific knowledge. In what follows we will develop our thesis on the nature and structure of scientific knowledge.

We have talked about induction as a method of abstraction, but we also hold the position that induction cannot be a sufficient means of generating scientific concepts. However, we have mentioned that another sort of ampliative inference called inversion would be able to generate scientific concepts. What then are scientific concepts? Is it possible to propose any demarcation criterion between science and non-science? If we are not clear on these questions, then talk of generating scientific knowledge and proposing a logic of discovery for such a knowledge does not make sense. Our discussion of this problem begins in this chapter, and we shall continue to explore for an answer till the end. The essential framework of our exploration will be elaborated in this chapter.

The problem of the nature and structure of scientific knowledge is one of the deeply involved problems of philosophy of science. The problem mainly consists in answering primarily, though not exclusively, the question: What are scientific *theories*? Are they axiomatic calculi in which theoretical terms and statements are given a partial observational interpretation by means of correspondence rules? Is it possible to delimit our analysis of theories to a rational reconstruction of fully developed theories? Or since the question is intimately tied to language and experience, should all the epistemic factors governing the discovery, development, and acceptance or rejection of theories be considered under one garb of *weltansshauung* or *Lebenswelt*? Are not theories extralinguistic entities, like propositions, which may be expressed by various linguistic formulations? Are they not equivalent to models isomorphic to physical systems or states? ...

Each question above presupposes a particular view of scientific theories, and the various ways in which these questions are formulated indicates the complexity and multifaceted nature of the issue. None of the proposals can be rejected outright, unless one delimits one's purpose at hand. Keeping in mind that our purpose is to understand the problem of generation and application of scientific knowledge we shall confine ourselves to the methodological and epistemic aspects of the issue.

5.1 Framework of Analysis: The Semantic Approach

A number of attempts have been made in answering the question of the demarcation of scientific knowledge from other forms of knowledge. We have seen in Part-I how variedly scientific knowledge (or *episteme*) was defined by different thinkers. Different varieties of criteria have been proposed, each of them making an effort to capture the essence of scientific knowledge. Some of the criteria are methodological, some are based on the substantial nature of the elements of knowledge, some are based on the nature of the sources, some on the semantic and structural features of the knowledge etc. But we have also seen how changes in view regarding the objects of knowledge caused changes in the nature of the methods employed. Most of the proposals were failures, but only if viewed as sufficient conditions of science. That is to say that none of them can be rated as completely invalid characterizations. It is not an easy task to achieve a synthesis of these approaches either. Therefore, it is appropriate to think that the problem cannot be understood completely from any one of the approaches. However, in such a situation we should make a choice based on *good reason* guided by the purpose at hand. It would also be a good reason to choose to work with such an aspect of scientific knowledge that could find at least definite linkages with other aspects. Therefore, based on the two good reasons, our choice is to identify certain necessary elements of scientific knowledge that fall within the scope of one broad thematic-pair *form and content.* The pair 'form and content' has been a philosopher's favorite. But, with regard to the present problem, certain other thematic-pairs have begun to dominate and control the discourse in the present century.

The view that we shall develop can be better stated by comparing it with the most popular approach in the philosophy of science. Most of the recent studies in the century centered their discussion of the subject based on the thematic-pair *observable (factual) and unobservable (theoretical).* For a century or so the philosophical problem par excellence has been to understand the relation between theory and fact. The problem is often posed as the problem of 'theoretical terms' and 'observational terms'. Though a number of interesting problems are posed, none of them could be resolved. The questions "What is a fact or a theory?" or "What is observable and unobservable?" remain unsolved problems to this day. Hilary Putnam wrote in 1962 that

> the almost untouched problem, in thirty years of writing about "theoretical terms" is what is really distinctive about such terms.[1]

We could safely substitute 'thirty years' by 'sixty years' today, because none of the explicatory attempts that were made from 1962 to this day are *completely* successful. However, new proposals have been made. Most interestingly, some of the new proposals consider the complexities involved in the observational-theoretical distinction as extraneous to an adequate analysis of scientific theories. For example Suppe says:

> The fact that science manages to go about its business without involving itself in such complexities suggests that the distinction is not really required or presupposed by science, and so it is extraneous to an adequate analysis of scientific theories.[2]

This is the view shared by the proponents of the *semantic approach* of scientific theories. The view escapes some of the problems rather satisfactorily. However, we cannot therefore say that the original problem—the problem of observation-theory distinction—is entirely ill-posed. There is a significant part of the basic problem, which is to account philosophically for the fact that science postulates processes and entities not directly accessible to observation in order to account for the phenomena that are directly observable. This part of the problem

[1] "What theories are not?" in E. Nagel, P. Suppes, and A. Tarski (Eds.) *Logic, Methodology and Philosophy of Science*, p. 243.

[2] Suppe 1972, "What's Wrong with the Received View on the Structure of Scientific Theories?" *Philosophy of Science,* p. 10.

persists even if we suppose that the original problem, as stated by the positivists is ill-conceived. Bas van Fraassen, another proponent of the semantic approach, states that

> science aims to find a true description of unobservable processes that explain the observable ones and *also what are possible states of affairs.*[3]

It is one of the essential features of scientific theory that it should have a capacity to deal with *possible states of affairs.* We therefore think that though science begins the 'journey' in search of principles accounting for problematic observable phenomena, it in the process constructs or creates certain structures which we normally call theories, that could account for not merely the observed phenomena, but also observable (not yet observed) phenomena and unobservable (in principle) 'phenomena' as well. Thus apart from what is actual, it could generate and account for "possible states of affairs". Here lies the *constructive* capacity of scientific activity.

The question arises, if theoretical constructs are so essential to science then why did the positivists take the project of *eliminating* them so seriously? We think that they have misunderstood scientific theories to be *abbreviations* or *nominal definitions* of a collection of basic statements.[4] They took theories to be merely new expressions introduced for pragmatic reasons. Their philosophical basis is rooted in confusing theories with nominal or formal definitions.

> To define a sign formally is to adopt it as shorthand for some form of notation already at hand. If the sign has a preconceived meaning, as in the present instances, and the definition suits that meaning, then the definition amounts to an elimination: it shows that the sign is dispensable in favor of those occurring in the definition. *To define a sign is to show how to avoid it.*[5]

This maxim (italicized sentence), is the basis of the positivists' motive, as well as confusion. They would have been right if scientific theories in fact are definitions in the above sense. But they are not, because the constructed definitions always have more 'capacity' than what nominal definitions could contain. This extra capacity contains the "possible states-of-affairs". We will argue in the next chapter that scientific knowledge contains *constructive definitions,* which are formed by a special logical relation, that we shall call *inversion,* that brings together the problematic observable phenomena and the unobservable (the created) in a single ineliminable form or construction. Their elimination would mean the elimination of the

[3]van Fraassen 1980, *The Scientific Image,* p. 3, italics ours.

[4]We have seen above (§3.3 70 the Machian influence on logical positivists.

[5]Quine 1951, *Mathematical Logic,* p. 47, italics ours. Quine uses the term 'formal definition' while Hempel uses the term 'nominal definition', but they meant the same. Cf. Hempel *Fundamentals of Concept Formation* p. 658.

essence of scientific knowledge. Only when description of phenomena are couched in these constructions, can phenomena be scientifically described *vis á vis* non-scientific descriptions. Most scientific facts are not raw data, but "hard-data".[6] For the positivists facts are basic observation statements, and not "hard-data". Scientific theories, according to the semantic view, are not applied to events simpliciter, but to events under a particular description—*structured facts*. This amounts to saying that scientific observations, and not necessarily *all* observations, are 'theory' laden, or dependent on certain constructions.

Another necessary point to note is that science does not and possibly cannot deal with complex phenomena all at once, but usually with limited kinds of phenomena and that too by employing a few of the parameters abstracted from them. That is, the abstracted parameters that are 'lifted' from the phenomena are supposed to be idealized representations or constructed images of the phenomena given in direct experience. But this is not possible by inductive abstraction (§ 4.8 page 107) as the positivists believed. Science begins with the set of idealized objects and remains there. This is precisely the reason why scientists have to create an experimental world that looks like an idealized world, and make observations in that 'artificial' world. Thus idealization and experimentation go hand in hand. In cases where real experimentation is not possible, for whatever reasons, scientists are often involved in thought experiments in order to simulate such possible worlds where their ideas appear meaningful. The history of science provides ample evidence of the fact that the rise of experimental science was necessarily associated with idealization and thought experiments (Cf. § 2.1 page 47 and § 8.3 page 255).

Not only are the idealizations constructed by scientists non-inductively obtained, but also the obtained constructions can generate a set of "physical systems", consisting of both "logically possible" and "causally possible" systems. Suppe says that scientific theory must specify which of the logically possible physical systems are causally possible.[7] We think that it is this specification that is subject to either falsification or verification. It is in this context of identifying and choosing between the logically possible and the physically possible that the difference between a pure mathematician and natural scientist lies. Here lies the distinction for example between Lagrange and Einstein or possibly between Poincare and Einstein, with regard to the discovery of the relativity theory.

Thus the semantic approach differs from the positivists' thesis on both the issues of what scientific facts and what scientific theories are. There are other important reasons that are connected with the above, which have to do with the linguistic thematic-pair *syntax and*

[6]Suppe, *op.cit.*
[7]Suppe, *Op.cit.* pp. 11–14.

semantics.

If we look at the problem retrospectively, it appears that the issue can be looked at as a debate between the syntactic and semantic approaches. Various versions of the positivist views were held mainly between the 1920s and the 1950s. Suppe calls them the various versions of the 'Received View'. From the 1950s to the 1960s the view has been facing many attacks, and it was only after that the concrete proposals of the semantic view were advanced. Though the distinction between syntax and semantics began to take ground in linguistics and language analysis soon after Gödel and Tarski, it took some time to trickle down to the problem of reconstructing scientific language.[8]

The Semantic approach, in the form of a general semantic analysis of scientific theories, was initiated by Patrick Suppes as an alternative to the Received View after the 1960s based on the model-theoretic technique as against the axiomatic technique. Earlier to this some isolated attempts had been made. For example, von Neumann's proof in 1955 that matrix formulation and wave mechanics formulation of quantum mechanics are equivalent (semantically), made use of this approach.[9] E. Beth, in 1948, had also developed semantic analysis for scientific theories and argued that this approach is more fruitful than the axiomatic approach.[10] However it is mainly due to the attempts made by Suppes and later by van Fraassen, who attempted an extension of Beth's approach in 1970, that the approach took off the ground and developed as a plausible view of scientific theories.[11] In fact the language used by the followers of semantic approach, such as models, systems or states (to be elaborated below), is rather close to the working scientists vocabulary, specially those working in the theoretical sciences. This is because of the adaptation of mathematical characterization of scientific theories rather than the remote logical vocabulary in relation to the working scientists language of positivists. At last both philosophers and scientists began to speak a common language!

For the purposes of developing an alternative generativist framework, we will argue, it is not only necessary to abandon the Received View, but also to adopt a view similar to that of the semantic approach. However, we are also benefited by the developments made in this connection by Stegmüller.

Stegmüller looks at the matter in a different way from that of the defenders of the

[8]This information is based on Suppe's elaborate introduction to the problem in Suppe 1977, Chapter I - III, p. 17ff.

[9]J. von Neumann, *Mathematical Foundations of Quantum Mechanics.*

[10]E. Beth "Semantics of Physical Theories" in Freudenthal 1961, *The Concept and the Role of Model in Mathematics and Natural and Social Sciences.*

[11]van Fraassen 1970, "On the Extension of Beth's Semantics of Physical Theories" *Philosophy of Science,* 37, pp. 325–339.

semantic approach. He classifies the views as statement and nonstatement views.[12] Although, there is some difference between the classes picked out by the two ways of classifying, the affinities outweigh differences between the nonstatement view and the semantic view. (The nonstatement view of Stegmüller is elaborated below (§ 5.3 page 132)). The only mismatch between the two manners of classifying the views is that Stegmüller incorporates the structuralist view of Suppes, which later developed into what we are presently calling the semantic approach, as a statement view, while all other statement views can be called syntactic without problem. That is to suggest that statement views minus the semantic approach is identical to the syntactic approach. We suggest that Stegmüller should identify his view as the semantic approach, or as one version of the semantic approach, or as a version of structuralism, rather than calling it a nonstatement view, because the semantic approach already has sufficient room for appraising conceptual constructions. Further Stegmüller and Suppes are both structuralists anyway. However, Stegmüller's view, as we shall see shortly, is undoubtedly richer than the versions of the semantic approach suggested. We are influenced by both the semantic approach and the nonstatement view, and we will be working toward a view of scientific knowledge that could be seen as what could emerge after further developments and simplifications of both the views are effected.

As already indicated, both the semantic approach and the nonstatement view follow the vocabulary of models and physical systems. According to these views, models and physical systems are part of the anatomy of scientific theory, while that of the Received View is described in terms of theoretical and observational terms. In what follows we shall characterize models and physical systems.

5.2 Models and Physical Systems

Science describes and explains the world in *indirect* ways, i.e., its access to phenomena is never *direct*. We will presuppose this as an essential aspect of science, without further argument at this stage. To avoid the possibility of misunderstanding, we would like to warn the reader that the distinction between direct and indirect made here is not on the basis of sensory experience, but on the basis of whether a description is made *dependent* or *independent* of a *structure*. What precisely is the nature of the structure shall be elaborated below.

As mentioned already, when scientists study 'something', an object of investigation,

[12] Different versions of statement views and nonstatement views are identified by Stegmüller 1979, *The Structuralist View of Theories: A Possible Analogue of the Bourbaki Programme in Physical Science* p. 4ff.

they do not describe all aspects of this 'something'; rather they select by way of abstraction certain parameters, among others, from this 'something'. The abstraction consists in organizing (structuring) the selected parameters into a system, which are 'lifted' from the rest of the 'something'. Such systems, as some would like to call, are idealizations. Only under experimental conditions, and with considerable approximations, can these systems be realized in the actual world. When phenomena are described using these structured parameters, the *meaning* of the description is not independent of the constructively visualized system or structure, and hence it is an *indirect description or observation.* Since scientific description cannot be true of the world without the involvement of idealization, the semantics of scientific knowledge demands *counter-factual* interpretation. Let us call a description scientific if and only if it is intelligible (meaningful) only through indirect (structure dependent) means. Now since a scientific assertion *accurately* describes 'something' only under ideal conditions, as stipulated by the structural relations, it is not true under normal circumstances. *The circumstances in which a scientific assertion is entirely correct or true is called a model.* This definition of a model is a micro-version of the usual definitions of a model. Differences with the usual definition, and the reasons for deviation are given below.

A physical system can be viewed as an idealized replica of the phenomena, which can be specified solely in terms of the selected parameters.[13] Examples of physical systems abound in science, and can be found in all disciplines: different kinds of instances of Newtonian particle systems, atomic system, dynamic systems such as oscillating or vibrating systems, thermodynamic systems, ideal gas systems, chemical equilibrium systems, physiological systems like the nervous, endocrine, circulatory systems, etc. However, there are other simpler and more general systems that scientists regularly employ at various levels of theorization, such as lever, balance, floating bodies, pendulum, etc. The role played by these simpler systems in the initial stages of the development of scientific knowledge is elaborated in greater detail below in Part-III. Most reconstructions of scientific theories have been attempted for relatively complex theories such as classical particle mechanics. We think that scientists started constructing, defining and using systems much earlier than the 17th century and can be traced back to the early origins of geometry. Our attempt in the present thesis will be to understand the genesis, development and the structure of simpler systems, by applying the semantic approach.

The function of models, in relation to physical systems, can be stated as follows: *Models are employed to represent the behavior of a certain kind of physical system.* Models

[13]Suppe 1972, *op.cit.* p. 224.

are usually devised as mathematical structures whose characteristics are obtained or specified by *definition*. For example, a Newtonian particle 'system' is regarded as a model because it is a structure that satisfies the three laws of motion and the law of universal gravitation.[14] Here the term 'system' appears in the name of a model. This terminology is unfortunate, but since most of us are used to calling most models by the name 'system' we shall continue to use the same terms. Whatever the term be, our criteria for characterizing models and physical systems is precise, which will be elaborated below.

Models can be defined with various degrees of complexity. For example, a Newtonian model can be defined for a 2-body system, which can be used to represent the physical system, Earth-Moon system, or it can be defined as a general model for a n-body system that can be used for a range of simple to complex physical systems. Given certain constant values to the parameters (initial conditions) the exact behavior of the physical system can be known for certain systems deterministically, while for others probabilistically, though in both cases this knowledge holds accurately only for an ideal physical system.

Though very general models can be constructed we will attend to very simple models, and also to very simple physical systems. This makes our problem of understanding the process of model building easier. Though the models and the systems that we will choose are simple they have sufficient complexity so that whatever we could state for them could without much problem be generalized to more complicated models and systems. However, we have other reasons for choosing simpler elements for analysis. Some of the reasons can be spelled out here, while certain others will be stated in the process at appropriate places.

Earlier (page 128) we have stated that the circumstances in which a scientific assertion is entirely correct or true is called a model, and also mentioned that this definition is a micro-version of the usual definition. The difference can be stated by comparing it with van Fraassen's definition, which is as follows:

> A model is called a model of a theory exactly if the theory is entirely true if considered with respect to this model alone.[15]

While we are talking in terms of 'scientific assertion', van Fraassen is talking in terms of a 'theory'. The motivation in deviating from the usual definition is not fundamental, though highly significant for our purpose. It is, first, to make the units of semantic analysis of scientific knowledge simpler. Secondly and most importantly, our attempt is to use terminology that needs least mention of the expressions 'theory' or 'theoretical'. The term 'theory' is

[14] Cf. Giere 1984, *Understanding Scientific Reasoning,* pp. 80-81.
[15] van Fraassen 1989, *Laws and Symmetry,* p. 218.

as problematic as the term 'law', and more or less for the same reasons. Van Fraassen, has argued rather convincingly that without using the problematic term 'laws' we can talk about and appraise scientific theories.[16] We are proposing to move a step further, in search of neutral terminology, by suggesting the use of the term 'scientific assertion' in place of the term 'law' (though not always in place of the term 'theory'). We see a possibility of describing the structure of scientific knowledge in terms of definitions, models, physical systems, scientific assertions etc., without leaving much residue, hence without requiring to talk in terms of 'theory'. The usefulness of our proposal towards acquiring a neutral vocabulary will be made clearer as we develop our views.

The first reason for choosing a smaller unit of analysis is based on the needs of a generativist. The usual examples of scientific theories, such as Newtonian mechanics, electromagnetic theory, relativity theory etc., are 'huge' structures, and it is therefore difficult to comprehend them by means of reconstruction in a significant manner. Besides, looking at these theories as one unitary or holistic structure has led to problems of a serious nature, specially in understanding the relationships, such as reduction, between different theories. We have also come to know that all applications do not involve the entire structure of the theory. More often very small components of the theory are employed to deal with some cases. From the point of view of a generativist, viewing a scientific 'theory' as one whole, and attempting to find out how such a thing could have been discovered makes it a very complicated problem. A generativist's strategy, we think, has to be, to start from the units and proceed to the whole—to understand how the whole can be constructed out of the units.

Therefore, most examples of reconstruction of bits of scientific knowledge that we shall elaborate below will be local case studies of a given field of science. This approach, we shall argue, will be more promising than attempting to understand the discoverability of mega-structures as one piece. We now offer some clarification regarding the notion of 'scientific assertion'.

A scientific assertion, in simple terms, is a statement relating a model—a type—with a physical system—a token. For example, in "Earth-Sun system is a two-body model (of Newton's theory)" the system and the model take the places of the subject and the predicate positions of a statement. The term 'Model' can be called a predicate (more precisely a complex predicate) because it describes a *class* of physical systems, all of which can be called the tokens of the model. Therefore, another way of defining a scientific assertion is that it is a statement where the subject is a physical system and the predicate is a model. A similar

[16] In the first part of his book *Laws and Symmetries* this thesis is argued in great detail. Though relevant to the matters we are discussing we shall not enter into that involved debate here.

characterization is also possible for meta-scientific statements, with a minor qualification (to be elaborated below). A scientific assertion differs from ordinary (commonsense) assertions in a special sense that *over and above the type-token relation, scientific predicates and subjects also have a structural or morphic relation between them.* This enables us to say that to have a scientific knowledge of "some thing" is to have a physical system that incorporates the phenomena which is described by an assertion employing a model. Nothing can be called scientific if the description is not structure dependent, i.e., indirect. This is our general criterion of demarcation.

Since both systems and models are constructions by the scientist, one might say, they say nothing about the *real* world. There is a partial truth in this, because while a model is constructed by employing mathematical methods and not by empirical means, physical systems are idealizations of phenomena. However, the possibility that some of the physical systems can be really actualized in the experimental world, if not in the 'open' world, provides sufficient reason for our belief in the applicability of the model to describe and explain the world around. Though due to the employment of mathematical techniques scientists can obtain models that may not have any known applications, the models obtained will have certain epistemic value because of their ability to tell us under what conditions an application of such a model could be found. A number of examples in modern physics can be cited, specially in particle physics, where scientists have devised a mathematical model prior to any empirical confirmation. The predictions that are based on such models were experimentally realized much later.

The question however remains, can there be any method or logic for generating or constructing models and systems? Since models and systems are different, and since both of them are constructions, the question to be addressed by a generativist must be able to meet both the requirements, either separately or together. In fact this is a consequence of any generativist who wish to adopt the semantic approach to scientific knowledge. We need methods of generating or creating both scientific subjects and scientific predicates of scientific knowledge. We claim that inversion has a very crucial role to play in both kinds of constructions envisaged.

The distinctive nature of the questions to be addressed in the context of discovery have been partially indicated by Suppe, who clearly distinguished between two epistemologically distinct stages in the process of theorization. One stage is the transition from phenomena to "hard" data about the physical system, and the second stage is the transition from the physical system to the postulates, etc., of the theory.

> The two sorts of moves are *qualitatively different*, the former being essentially empirical or experimental—being in effect a "translation" from the phenomena to an idealized description of it in the vocabulary of the theory's formalism, and the latter being essentially mathematical or computational in nature.[17]

Suppe correctly identifies the first stage as one which involves counterfactuals, while the second stage involves mathematical computations (techniques). He further says, that the first stage is more complex than the second. Furthermore, it is a historical fact that the former is more difficult and more time consuming than the latter.

The problem of the construction of models will be taken up in the next chapter, where we will return to the issues identified here. The problem of constructing physical systems will be taken up in greater detail in the case studies in Part-III.

Before we get into the problem proper we shall critically discuss in detail the non-statement view of Stegmüller on the structure of scientific theories.

5.3 Stegmüller's Nonstatement View

The nonstatement view has been developed by Sneed and Stegmüller in an attempt to give a methodological character to Kuhn's insightful thesis. They developed a structuralist and nonstatement view of scientific theories, which has been claimed by its proponents to provide the conceptual apparatus needed for the analysis of the dynamics and the *metascientific reconstruction* of Kuhn's notions of 'normal science' and 'scientific revolutions'.[18] Stegmüller's project is to face the challenge posed by the irrationalist view that follows from Kuhn's thesis, that a theory is never rejected on the basis of falsifying instances but is eventually dislodged by another theory (§ 3.4 page 79). He thinks that the apparent irrationality of Kuhn's view can be erased if we understand by scientific theory *not* a system of statements, but "*a relatively complicated conceptual instrument*", which he terms as the *nonstatement view*.[19] Stegmüller's reaction to Kuhn's thesis has a remarkable feature. Though Kuhn's thesis has a negative impact on the traditional cumulative logicist view of science, Stegmüller thinks that it has a constructive/positive thesis. It is to this constructive thesis of Kuhn, Stegmüller refers to by the term 'metascientific reconstruction'.

There are two sources where irrational behavior can be ascribed to scientists in the Kuhnian account. First, during the period of normal science, scientists never examine their presuppositions nor their conceptual apparatus critically even if recalcitrant 'data' are

[17] Suppe 1972, *op.cit.* pp. 15-16.
[18] Stegmüller 1976, *op.cit.* p. vii.
[19] *Ibid.*

found. Second, during the revolutionary phase one theory is dislodged by another not by comparative evaluation but by a competitive struggle between theories.[20] Thus, neither during the period of normal science nor during the revolutionary period is falsification or verification of theories coming from observations of nature/experiment. The two theories competing at the revolutionary period are not logically compatible with each other, for they are incommensurable.[21] The new theory contains nothing of the old, therefore no rational arguments can work in this context. These are briefly the sources of irrationality in Kuhn's *The Structure of Scientific Revolutions*.

Kuhn's thesis undoubtedly provides a setback to metascientific analysis or any logic of scientific discovery. If Kuhn is correct, these terms must be vacated in favor of psychology, sociology and history of science.[22]

Stegmüller's proposal can be described as a reconciliatory attempt by 'imbuing rationality' to Kuhn's conception of science

> by disposing of the *apparent contradictions* between the facts that *a person (or group of persons)* holds a certain theory and nevertheless constantly changes his (their) opinion respecting this theory.

He proposes a nonstatement view of scientific theories based on an informal reconstruction of a theory by introducing a *set-theoretic predicate.* A scientific assertion or claim, according to this view, is of the form "c is an S" where c is a name or a definite description of a concrete situation, and S is the structure of the theory.[23] For example, "x is a classical particle mechanics" is a set-theoretic predicate capturing the structure S of a theory.[24]

Stegmüller reconstructs a *mature* theory such as classical particle mechanics, as *a* concept, or *a* structure S. The structure S is defined in terms of domains, functions (both theoretical and non-theoretical) and axioms.[25] The functions (both theoretical and non-theoretical) will be replaced by constant values from the given domain to obtain an application of a theory.

A function is theoretical iff the concrete measurement of the function depends on the theory for every application of the term, otherwise, i.e., if its measurement does not depend on the theory, it is non-theoretical.[26] It is a noteworthy proposal because no explication of observability etc., are involved in this dichotomy. A function is said to be T-theoretical or

[20]Kuhn 1970, *The Structure of Scientific Revolutions* p. 77.
[21]Kuhn, *ibid* p. 101.
[22]Cf. Stegmüller *op.cit.* p. 161.
[23]Stegmüller *op.cit.* p. 38.
[24]Cf. *Ibid*, Chapter-6.
[25]*Ibid* p. 96.
[26]*Ibid* p. 45

not, in *relation* to the theory concerned. Therefore this distinction is not absolute.[27] For example, the concept 'pressure' could be a theoretical concept with respect to classical particle mechanics, while it is non-theoretical with respect to thermodynamics.[28] This relativistic, but objective, approach helps Stegmüller to say 'farewell' to the observational language.[29]

The structure S consists of 'levels' of models, such as models Ms, possible models M_ps, and partial possible models M_{pp}s. Models of a theory are entities (structures) where a theory is satisfied. The distinction between the various models is that the model M is mathematical, the possible model M_p is obtained from M by eliminating the axioms, and the partial possible model M_{pp} is obtained by eliminating from M both axioms and also theoretical functions. The extensional elements of partial possible models are assumed to be out there in the world, where every theoretical component is assumed to be eliminated. Stegmüller makes use of the initial suggestions of Ramsey and Sneed in this process of eliminating theoreticity.[30] One clear distinction we find between the semantic view as presented in the above section and Stegmüller's view is that on the latter's view the elements of partial possible models (physical systems) are considered to be completely devoid of theoretical content, while according to the former view physical systems are also theory impregnated. Here, we think Stegmüller continues to be influenced and seems to be convinced by the positivist's program of eliminating theoreticity for the purposes of reconstructing and interpreting theories.

The relationship between the various levels of models in the structure S can be best understood by saying that the factual-content is highest in partial possible models, lowest in models and intermediary in possible models. We can say that the theoretical and abstract component of models gets successively reduced from M to M_{pp}, and the factual content, which can be defined as the inverse of theoretical content, gets successively increased from M to M_{pp}. Thus in this structure S, we can say in most general terms that form and content are inversely related. Though this is our interpretation of Stegmüller's definition of structure, we think that Stegmüller would agree with this general formulation. This general formulation, we think, not only gives a pattern to what is proposed by Stegmüller, but is also simple enough for the understanding the proposal of the semantics of scientific theory.

The structure S is neither true nor false, for it is not an assertion but a conception. When we apply a structure to a *concrete* situation by making the assertion "c is an S", we obtain a statement which can be either falsified or verified, for only statements are true or

[27] Cf. § 3.1 pp. 40ff.
[28] *Ibid* pp. 46-67.
[29] *Ibid* p. 50.
[30] Sneed, J.D. 1971, *The Logical Structure of Mathematical Physics,* Ramsey F. P. 1931, *The Foundations of Mathematics and Other Logical Essays.*

false.

> *A theory is not the sort of entity* of which it can sensibly be said that it has been falsified (or verified).[31]

It is important to keep in mind that "c is a S" should be understood as a single undivided statement. The 'anatomy' of the structure S should not lead one to the misunderstanding of Stegmüller's proposal due to the statemental nature of metacharacterization, which is unavoidable. It is also important to bear in mind that all this is a description of semantic objects and not syntactic objects.

Stegmüller further defines other structures such as *core* K. The structure S of a theory becomes the core K of that theory with the introduction of (a) a function r that effects the differentiation between M_p and M_{pp}[32] and (b) a constraint C on M_p to the three models, M, M_p and M_{pp}. The core K of a theory can be expanded by further introduction of (a) a set of laws L relating the elements of the core K, (b) a constraint C_L on the theoretical functions of the laws that restrict the theory in its various applications and (c) an application relation α between the set of laws L and the set of intended applications I.[33]

The relation α is a many-many relation, whose domain *must* be a subset of partial possible models M_{pp} and whose range comprises of a set of laws L. The relation α restricts absurd applications of laws by guaranteeing that the law actually holds.

A theory, then, consists of a core K, and a set of intended applications I. The I consists of *physical systems* or M_{pp}.[34]

A set of physical systems never have as their domain, *mere individuals*.[35] It must include objects which are described in a certain way, i.e., the objects in a physical system must be *related* according to certain non-theoretical functions.[36] In other words, the physical systems are extensional structures, vis a vis., a theory which is an intensional (conceptual) structure.

By the introduction of two more additional conditions in the definition of a theory we get a complete formulation. One of them is that there should be cross linkages between various applications of a theory (as specified by (5) below), and the other is that the various

[31] *Ibid,* p. 19.
[32] *Ibid* p. 109.
[33] *Ibid,* p. 115.
[34] Feyerabend suggests that "T-facts" would be a better term than 'physical systems' to bring out the point that T-facts are relative to theories. This he suggests would be in consonance with Stegmüller's relativized dichotomy of T-theoretical and T-nontheoretical. Feyerabend 1977, 'Changing Patterns of Reconstruction' *British Journal for the Philosophy of Science* Volume 28, p. 353n.
[35] Stegmüller *op.cit.* p. 163.
[36] *Ibid.*

applications be *homogeneous*. Putting everything together the definition of a scientific theory is as follows:

X is a scientific theory only if there exists a K and an I such that

(1) $X =< K, I >$;

(2) $K =< M_p\,, M_{pp}\, r, M, C >$ is a core for a theory;

(3) $I \subseteq M_{pp}$;

(4) each element of I is a physical system;

(5) if $\mathcal{D}$ is a class containing exactly the individual domains of the elements of I, then for any two elements $\mathcal{D}_i$ and $\mathcal{D}_j$ of $\mathcal{D}$, $\mathcal{D}_i$ is linked with $\mathcal{D}_j$;

(6) I is a homogeneous set of physical systems.[37]

Stegmüller comments that the conditions (4) and (6) might involve pragmatic elements.[38]

Given this reconstruction of what a scientific theory is, it is now possible for Stegmüller to tackle the irrationality problem, mentioned above, resulting from Kuhn's thesis.

A theory, according to Stegmüller, can have a series of expansions E_1 at t_1, E_2 at t_2 ... , but the core of a theory and the set of intended applications remain constant. Here is a possible identifying criteria of a scientific theory. The same theory can be used "at various times to assert different central empirical claims."[39] It is also possible to reconcile Popper's falsificationism and Kuhn's conception of science, because a scientist can continue to believe in E_i, one of the extended applications, and not believe at the same time E_{i+1}, on the grounds that it is not part of the set of intended applications I. Therefore continued belief on the part of the scientist despite recalcitrant evidence is in no way irrational.[40]

The set of intended applications does *not* constitute a *fixed* domain, and it is given intensionally via a set of paradigmatic examples which satisfy only Wittgensteinian family resemblance.[41] In the event that a theory fails completely with regard to one specific 'application', that can be removed from I, without affecting the rest. The set I of intended applications determines what Kuhn calls paradigmatic examples, which in turn determine a paradigm.

The nature of change that takes place during revolutions is from one core to another. With regard to the incommensurability of rival theories, in the context of revolutionary

[37] Cf. *ibid* p. 165.

[38] We shall discuss the problem of deciding about the sufficient conditions for physical systems in greater detail below, because we claim that inversion plays a decisive role in delimiting a physical system. The problem of deciding homogeneous systems would not exist in the framework that we are adopting, for we think it is an artificial problem.

[39] *Ibid,* p. 168.

[40] *Ibid*.

[41] Cf. § 13.2, pp. 173–177.

science, two different theories will have *different* cores that have no logical relations possible. Thus, Stegmüller says, incommensurability is a trivial consequence of the nonstatement view of theories. We shall critically discuss his proposals in the next section.

5.4 Critical Appraisal of Nonstatement View

Stegmüller's work received mixed response from both Kuhn and Feyerabend. Kuhn welcomed Sneed's formalism commenting that even an elementary structural form of Sneed captures significant features of scientific theory and practice notably absent from the earlier formalisms.[42] He further comments that

> if only simpler and more palatable ways of representing the essentials of Sneed's position can be found, philosophers, practitioners, and historians of science may, for the first time in years, find fruitful channels for interdisciplinary communications.[43]

We will return to Kuhn's critical comments after considering Feyerabend's, Pearce's and van Fraassen's.

Feyerabend argues that both the claims of Stegmüller (1) that Kuhn's thesis would cease to have irrational consequences if we adapt the nonstatement view by rejecting the statement view, and (2) that nonstatement view no longer has problems with theoretical terms, incommensurability of rival paradigms etc., can be defused by showing that

> (A) that the features rationalized by Stegmueller occur in Kuhn but not in science and/or (B) that these features can also be explained by the statement view.[44]

Scientific theories as reconstructed by Stegmüller, Feyerabend says, are not immune to removal by facts but only immune to certain types of removal such as refutation.[45] Therefore, the difference between the statement view and the nonstatement view "lies in the *circumstances* that bring about the demise of a theory." The statement view methodologies, such as the Lakatosian methodology of research programs, have already shown how immunity of a theory can be accounted for rationally.[46]

Feyerabend refers to certain episodes in science which can be better explained by the statement view. For example:

[42] Kuhn 'Theory-Change as Structure-Change: Commenting on the Sneed Formalism' in Butts, R.E. and Hintikka J. 1977, *Historical and Philosophical Dimensions of Logic, Methodology and Philosophy of Science,* p. 290. Since Stegmüller adopts Sneed's formalism, Kuhn's comments on the latter apply also to the former.

[43] *Ibid,* p. 291.

[44] Feyerabend 1977, 'Changing Patterns of Reconstruction' *British Journal for the Philosophy of Science* **28** p. 359.

[45] *Ibid.*

[46] *Ibid* p. 360.

> The early quantum theory can be regarded as a paradigm (and was regarded as a research program by Lakatos) but there is no fixed underlying structure, no core in the sense of Sneed and Stegmueller, very fundamental assumptions such as the law of conservation of energy and momentum may be dropped and picked up again, and this is not just in special domains, as would be the case with special laws and expansions, but where ever the paradigm is applied. Moreover, this feature can be found not only in this particular period of the history of science which was rather unruly but in more settled periods as well.[47]

A statement view, such as Lakatos, can deal with cases like these, because Lakatos's core is a loose cluster of statements and is not as rigidly defined as the core, which permits an exchange of statements in such a manner that without change of paradigm the member statements in a cluster of statements can change. Therefore certain historical episodes of science can be better understood by the statement view. This is over and above the statement view's ability to handle other features of Kuhn's thesis, which both the statement view and nonstatement view can handle. Effectively, therefore, the statement view is better placed. Also, in the 'context of application' very rarely do scientists employ the entire structure of a theory.[48]

But, then there are advantages in the Sneed-Stegmüller reconstruction, which "puts into relief certain features of science that almost disappear in the statement view." They are:

> One minor example which is not mentioned by Stegmueller is the role of diagrams and models: chemical formulae are compared and combined according to strict rules but it would be somewhat artificial to regard them as statements. Of course, they can be used to produce statements, but they are not statements themselves and transformations leading from one formula to another do not go through a statement phase. An even more important example is the role of *a priori* elements in our knowledge. Categories, forms of perceptions, are structures which again give rise to statements (Kant's synthetic *a priori* statements) without being statements themselves.[49]

From these critical observations of Feyerabend one very important point emerges, which we further wish to exploit in the present thesis. The point can be put as follows: There exists a territory (the territory roughly demarcated by Feyerabend in the above quotation) that the nonstatement view alone can access. There are certain territories, specially those in the context of application, where the statement view has better access. And, insofar as the 'middle' territory, that part which both the views can handle well, is concerned, it would remain unproblematic for methodological reconstructions. Stegmüller, so far has not shown how the nonstatement view would work in that territory where it alone has access. All his

[47] *Ibid* p. 361.
[48] *Ibid* p. 361.
[49] *Ibid* p. 359.

attempts are focussed in explicating the constructions of a finished scientific theory, and its applications, where the statement view also has a better hold. Therefore, we think that the strong point of nonstatement view remains undemonstrated. He seems to be working in a context that does not belong to a defender of the nonstatement view. We shall further strengthen this observation as we go through the critical comments of Thomas Kuhn.

David Pearce argues that the debate between the statement and nonstatement view is not substantive, i.e., an issue over which one can be judged better than the other. He thinks that

> the force of Stegmüller's advocacy of the nonstatement view would be dissipated were it to turn out that the two positions are after all only equivalent ways of saying the same thing—what you can do with linguistic concepts you can also do with structures, and conversely.[50]

Though, the structuralist approach based on Suppes' notion of *informal* set-theoretic predicate is able to treat the structure and dynamics of real scientific theories, Montague's rigorous *formal* axiomatizations of scientific theories "may be considered both philosophically valuable and a practically useful tool within the formal language approach to the programme of reconstruction".[51] Further, he says that:

> Since a scientific theory is customarily *written down* , e.g. its empirical laws are expressed by sentences of a *language*, considerable amount of technical dexterity is required to paraphrase away its linguistic features in order to represent it in set-theoretic form.[52]

Pearce's attempt on the whole has been to show that the point of set-theoretic structuralist/nonstatement approaches depends on the linguistic basis of science.[53] Thus Stegmüller's view does not address anything more *in principle* to the formal statement based model-theoretic reconstructions of scientific theories. In fact, Stegmüller also says that the Montague line of reconstruction of a scientific theory is *in principle* possible, though, he says, that we need super-super Montagues to achieve the task. These observations suggest that except for the factor of degree of difficulty there seems to be not much difference between the two views with respect to reconstruction of a finished scientific theory. We take encouragement from this situation to further strengthen our earlier conclusion that Stegmüller is trying to prove his mettle in a wrong context, the context of reconstructing a mature scientific theory and

[50]David Pearce 1981, 'Is there any theoretical justification for a nonstatement view of theories?' *Synthese* 46, p. 2.

[51]*Ibid* pp. 3-8.

[52]*Ibid* p. 7.

[53]*Ibid* p. 34

the context of application. We think that the context where a nonstatement view would fare well is the context of theory formation and not *after* a mature theory has been formulated.

Is it possible to define a set-theoretic predicate before a theory is historically developed as a finished product? No, because it is reconstruction, it is post-hoc. However, is there any epistemological gain in the exercise? Yes, there seems to be some, because Kuhn's observations about scientific revolutions appear more rational. But, what if Kuhn is proved wrong? Stegmüller seems to be presuming the truthfulness of Kuhn's observations, which can be challenged. What is the worth of the nonstatement view independent of Kuhn's thesis?

Since the anatomy of a theory is rather clearly stated, it would have a role to play in deciding whether something is a scientific theory or not. But again, Stegmüller's reconstruction is possible only for mathematically matured fields of inquiry such as physics. Is it necessary that only physics be considered the proper way of doing science?

Is there any implicit method that would generate the elements in the set of intended applications? If that were possible, then the reconstruction can suggest a logic of discovery. But no such role of the nonstatement view is suggested. Besides, if the identification (discovery) of a physical system is theory or structure dependent, i.e., T-theoretical, pure *a posteriori* identification of it would be impossible. It follows from Stegmüller's account that any problem of application of a theory can be said to be solved only after the successful incorporation of a physical system under a theory is achieved. Therefore, Stegmüller's reconstruction would help only in reinterpreting an application after the problem of application or in other words the problem of discovery is actually solved. Once the problem of discovery is solved, all the relevant epistemic factors, whatever they are, would already have entered and done their job. If the above observations are correct, then there seems to be no special and substantial epistemic role for the nonstatement view, apart from providing a semantics for a finished theory. Where lies the significance of the nonstatement view? The places identified by Stegmüller do not appear to be satisfactory. Kuhn thinks of certain possibilities which we shall discuss below after presenting van Fraassen's critical comments on the nonstatement view.

While reflecting on the question of what sort of a thing a scientific theory is, van Fraassen tells us why he disagrees with the proponents of the nonstatement view. He defends a view that a scientific theory must be the sort of thing that we can *accept* or *reject* and *believe* or *disbelieve*.[54] Commenting on the view of Stegmüller that a theory is not the sort of thing which can properly be said to be true or false, he says:

[54]Bas van Fraassen 1989, *Laws and Symmetry,* p. 190.

> This looks like a high price to pay. Don't we believe, assert, deny, doubt, and disagree about theories? And do such propositional attitudes not presuppose at least that a theory is the sort of thing which can be true or false?

A theory is undoubtedly an object for epistemic evaluation. For van Fraassen it becomes all the more important because theories can be appraised on the basis of certain *a priori* qualities such as symmetry. (We will discuss separately the relation between symmetry and scientific knowledge below.) But it is not impossible for a defender of the nonstatement view to incorporate such features. It is also possible to reinterpret such "propositional attitudes". A defender of the nonstatement view, for example, could reinterpret (and van Fraassen himself sees this possibility) belief in a theory as a belief that the theory possesses a certain relation to empirical reality, say by having relations to a set of applications.[55] Similarly other propositional modes mentioned above can be reinterpreted.

Is it not possible to provide epistemological appraisal of concepts? Say as meaningful or not, as empirically relevant or not, as acceptable or not, as successful or not. The nonstatement view is not claiming anything other than saying that a scientific theory be understood as a complex concept (or predicate). We do make choices between concepts, just as van Fraassen chooses 'acceptability' rather than 'truth' as a better expression in certain contexts. Just as all instruments are not good for hunting, not all concepts are good for 'hunting' truth. Since it is possible to rate or value concepts as being useful or not, depending on the context, it is indeed possible for a nonstatement view to meet van Fraassen's objection.

Further, is it not possible to introduce a dichotomy between scientific and non-scientific concepts? If it is possible it would become another level of epistemic evaluation of concepts (theories). We are aware of purely empirical and positivistic suggestions, such as cognitive significance based on verification, proposed by positivists and falsification, proposed by Popper. We think that along with empirical evaluation another complementary non-empirical (logical) evaluation would make such a dichotomy possible. Symmetry, van Fraassen's favorite, is certainly one of the possible candidates for achieving such an objective. The necessary relation between symmetry and inversion would make this new line of epistemic evaluation all the more interesting. More about this later (page 6.9).

Since theories are viewed as structures by both Stegmüller and van Fraassen and since symmetry is a property of structures, we do not see why their positions cannot meet on the issue of the structure of scientific theories. We think that their meeting would have the effect of mutual reinforcement, and no one is required to pay any high price. There seems to be little or no room for differences between the structuralists and the constructive empiricists.

[55] *Ibid,* pp. 190-191.

Another cause of trouble in the nonstatement view, according to van Fraassen, is regarding the capacity of scientific theories to say what the world is like. Scientific theories are indeed about the world, otherwise they can't be considered scientific. They are not just about the observable aspects, but also about the inaccessible and unobservable aspects. But at the same time the function (or the power) of science is not just to describe and explain our experienced world, but also to describe and explain other possible worlds. Thus van Fraassen says:

> What does the theory say the world is like? *and* What does the theory say the phenomena are like? Since the phenomena are just the observable part of the world, and since it is logically contingent whether or not there are other parts, it follows that these questions are not the same. Indeed, the second question is part of the first, in the sense that a complete answer to the latter is a partial answer to the former. The 'non-statement view' appears to deny the intelligibility of the bigger question—but the question seems intelligible.[56]

The distinction between 'the world' and 'the phenomena' is inevitable, specially with regard to scientific knowledge, which cannot restrict its access to the world of phenomena alone. It is not clear, however, why he thinks that the nonstatement view denies or should deny the intelligibility of the bigger question. The set of intended applications, according to the nonstatement view, *need not* be equivalent to the set of observable phenomena alone. Besides the nonstatement view also speaks of physical systems as members of the class of applications, and not mere phenomena. This is also the point of the semantic approach van Fraassen defends.[57] Therefore, we think that van Fraassen's objections are as unreasonable as Stegmüller's belief that the semantic approach is a statement view. The only notable distinction between the semantic view and the nonstatement view is in the latter's notion of possible models M_p, (which is also the opinion of Kuhn) and therefore we should reconsider the latter as an enrichment of the semantic approach rather than as a radical departure. The worthiness of the nonstatement view, let us repeat, consists in contexts other than those of reconstruction of finished theories, such as the reconstruction of theory formation. For the purposes of reconstructing finished theories the semantic approach is as good as the nonstatement view or any other statement views, whether the informal type of Lakatos or the formal variety of Montague. Since van Fraassen and other defenders of the semantic approach also believe that the nature of scientific knowledge consists in establishing the relation between the models on one hand with the physical systems on the other, whatever differences still

[56] *Ibid,* p. 191.
[57] Cf. *ibid,* p. 222, and also Giere 1979, *Understanding Scientific Reasoning,* and Suppe 1977, *The Structure of Scientific Theories.*

persist between them would not be of a substantial kind. They should therefore realize that there are more affinities than differences and if they come together it would be for mutual benefit.

Symmetry as a property of scientific construction, to be elaborated below, can without trouble be incorporated by Stegmüller et al., and a *conceptual view* (if not the nonstatement view) can be incorporated by van Fraassen et al. After all symmetry is a property of either a structure or a relation. An assertion is asymmetrical because the predicate 'includes' more than just the subject mentioned by an assertion, for it is a *type*, while the latter is a *token*. Definitions, models and systems can be interpreted as structures, all of which can be called symmetrical. We therefore, do not see any problem, of either a formal or a substantial kind, in viewing definitions, models and systems as nonstatements. We can always view—with appropriate interpretation—scientific assertions (call them laws or theories or whatever) as statements. What is required, to the best of our understanding, is that van Fraassen should and could without problem admit that definitions, models, systems etc., are nonstatements, in the sense that they are constructions or structures. And Stegmüller should and could without problem admit that scientific assertions, whether big or small, are indeed statements. The resulting view obtained by the reconciliation of the semantic and the nonstatement views, would give rise to an enriched picture of scientific knowledge. Therefore, for most purposes the semantic approach and Stegmüller's structuralist view could come closer than van Fraassen and Stegmüller think.

Another point requires clarification. The 'bigger' question mentioned above—'What does the theory say the world is like?' or what van Fraassen calls "the foundational question *par* excellence": *how could the world possibly be the way this theory says it is?*—does not mean 'What does science say the world is like?' or 'How could the world possibly be the way this science says it is?'[58] This may sound like a trivial clarification. But seeing the distinction between the two has significant implications. We think that science *does not* say, and it *cannot* say, just in one piece (theory) what the entire world is like. Science *does* and *can only* say what *some of the systems of the world* are like. A scientist can, with the help of a theory, undertake to describe or explain a *taxon* of systems but not all systems at one go. We define a 'taxon' as a class of homogeneous objects or systems that are structurally alike.[59]

The above claim that science approaches the world piecemeal cannot be proven acceptable unless we prove the following proposition untenable: All systems cannot be assumed

[58] van Fraassen *op.cit., p. 193.*

[59] Cf. Rom Harrè 1970, *The Principles of Scientific Thinking,*

to be homogeneous or compatible to one mega-system. The only possible reducibility is at the formal level, therefore devoid of content. Different theories relate to different sets of systems, or in Dudley Shapere's language, different scientific theories have different domains.[60] Reducibility without loss of content is a myth, as is also generatability of all physical systems, from one mega-structure (or mother structure), which we can relate to our phenomenal world. Analysis and synthesis, whether at the conceptual level or at the ontological level, is the only known technique of science, which can be realized only at a local paradigmatic level. Global or architectonic systematization is not a feature of natural science. For natural science it is essential, unlike metaphysics, to be paradigmatic and problem solving at a local level. Even if one could prove formally that all theories are reducible to one, that would be at the cost of empirical content. What is scientific knowledge without empirical content? We will return to these questions below.

We will end this section with Kuhn's critical comments on Sneed's and Stegmüller's proposals. Kuhn's comments are indicative of a context where a nonstatement view is worthy of consideration. Kuhn reads significant sense in Sneed's and Stegmüller's proposals not in terms of the manner in which they have been presented but by *inverting* the order of presentation.

Sneed and Stegmüller start reconstruction by selecting an established theory such as classical mechanics, presupposing the criteria for identifying a theory. Then by examining the theoretical and non-theoretical functions which are distinguished on the basis of the conditions specified above (133), they introduce constraints for obtaining the specification of theoretical functions. Kuhn remarks in this regard that the novelty of Sneed's (and Stegmüller's) approach consists in the role of constraints. He wonders about the possibility of inverting the order of their introduction.

> Could one not . . . introduce applications and constraints between them as primitive notions, allowing subsequent investigations to reveal the extent to which criteria for theory-identity and for a theoretical/non-theoretical distinction would follow?[61]

In order to see how significant in fact is this possibility, let us see why Kuhn sees this possibility.

Kuhn observes that of the three models, M, M_p, M_{pp}, except for the second, M_p, all have parallels in traditional formal treatments. M_p's can be obtained by *adding* theoret-

[60] Dudley Shapere 'Scientific Theories and their Domains' in Suppe 1977, *op.cit.,* pp. 518ff. More on domains, and reducibility below.

[61] T.S. Kuhn 1977, "Theory-change as Structure-change Comments on the Sneed Formalism" in Butts and Hintikka 1977, p. 295.

ical functions to M_{pp}'s. Another way of saying is that M_{pp}'s can be obtained by *dropping* theoretical functions from M_p's. We can describe the latter as *top-down transformations* and the former as *bottom-up transformations*. Stegmüller's presentation follows the order of top-down transformation. We will see below that these transformations are *really invertible*. This is possibly what Kuhn has in mind. For he observes that:

> Except in the case of fully mathematized theories, neither Stegmüller nor Sneed have much to say about how M_{pp}'s are, in fact, extended to M_p's.

That is, they have little to contribute in the order of bottom-up transformation.

The bottom-up transformation (from M_{pp} to M_p) is very relevant for *reconstructing theory formation*, if not for reconstructing a well formed theory. Since, a method of generation consists in solving the problem of reconstructing theory formation, the possibility of inverting the order presented by Sneed and Stegmüller is crucial for the problem at hand. Kuhn makes the following three assertions, which we think are supportive of our view:

> First, teaching a student to make the transition from partial potential models to partial models is a large part of what *scientific*, or at least physics *education* is about. That is what student laboratories and the problems at the ends of chapters of textbooks are for. The familiar student who can solve problems which are stated in equations but cannot *produce equations* for problems exhibited in laboratory or stated in words has not begun to acquire this essential talent. Second, almost a corollary, the *creative imagination* required to find an M_p corresponding to a non-standard M_{pp} (say a vibrating membrane or string before these were normal applications of Newtonian mechanics) is among the criteria by which great scientists may sometimes be distinguished from mediocre. Third, failure to pay attention to the manner in which this task is done has for years disguised the nature of the problem presented by the meaning of theoretical terms.[62]

Kuhn further says in a footnote, which appears at the end of second assertion, that traditional reconstruction does not have a step from M_{pp} to M_p. The importance of Sneed and Stegmüller's work is that it at least shows how in a fully mathematized theory M_{pp}s can be extended to M_ps.

These observations of Kuhn show the direction toward which a generativist should work. The patterns of learning and the patterns of discovery have many features in common. If it is necessary to *learn* geometry before statics, it is also necessary to *discover* geometry before statics.[63] The necessity of introducing certain concepts while learning, before certain

[62] *Ibid,* p. 291, italics ours.

[63] Kuhn' observations in this regard are as follows: "Textbooks of advanced mechanics lead plausibility to that identification of the theory, but both history and elementary pedagogy suggest that statics might instead be considered a separate theory, the acquisition of which is prerequisite to that of dynamics, just as the acquisition of geometry is prerequisite to that of statics." (*Ibid p. 296.)*

other concepts, indicate to us the mutual dependency of logic and semantics. We think that it is possible to demonstrate that the original course of discovery of an idea gets reenacted in everyone's mind, though no one-to-one mapping of the events is possible.[64] This lack of complete matching is not a problem because what we are seeking is a mapping in terms of form and not content.

We are reminded of an analogous situation in the case of organic evolution, often stated as the biogenetic law: *ontogeny recapitulates phylogeny*. For example, the stages of embryonic development of higher vertebrates have a relationship analogous to the phylogenetic development as envisaged by the general theories of organic evolution. In this process, no real bodies of hydra, worm, fish, or frog can be seen developing. What we do see, however, is a structural transformation from simpler forms to more complex forms, passing through the essential stages. Thus, the path to discovery and the path suggested for learning can be looked at as being isomorphic despite difference in the time scale.

One important difference, however, between the context of learning and the context of generation should be indicated. Learning is more often a guided process by already learned people, whether teachers or whoever, whereas original discovery always has an element of *personal guidance* , which is what we often call, creative genius. If scientists are distinguishable from the mediocre, as Kuhn is suggesting in the above passage, on the basis of their ability to move from M_{pp}s to M_ps, the steps involved in this phase are undoubtedly relevant for unraveling the problem of the generation of scientific knowledge. The latter can better be described as the manufacturing of scientific knowledge. One might say that originality of initial discovery is because of the *unaided* character of the process. Though it is true that the original discoverer does not get any aid from *recognizable* sources, completely dismissing the role of any aid would necessarily lead to mysticism about the process of discovery. Therefore, we tend to call the nature of guidance involved in original discovery a 'personal guidance' to indicate that there exists an inward source of a *guide* or *teacher* within every genius. The best pedagogical methods, therefore, are also those which make an individual more independent. It is the role of method, whether professed by the renaissance humanists or by the ancient Greeks—to create independent individuals.[65]

[64]The context of learning is most often not as interesting as the context of discovery. This is possibly because the latter context is never reenacted fully or even partially in the former context, which makes learning very dull. Good generative reconstructions of ideas, we think, would prove to be of great use in science education.

[65]We should however note the irony implicit in this humanist spirit. In actual fact what we have seen is that most scientific knowledge has been employed to make most of humankind more dependent on the mega-systems civilization has created, rather than in the direction of liberating the individual from the misery external systems impose on being. On the one hand science seems to be based necessarily on institutions, but on the other hand it has this professed objective of human freedom. We are not heading to resolve, reconcile or even expose these ironies or contradictions any further in this thesis, for that would be more in the direction

In this thesis we are trying to make room for a methodological study of the inverted order which we have indicated above. The set of partial models as stated above, consists of physical systems. Observing physical systems such as those analogous to balances, floating bodies, pendulums, etc., is not a trivial matter. In fact, it is in itself an insightful discovery, often half solving the problem. We therefore suggest that in the formation of scientific knowledge we need to consider another step, which is *from phenomena or from facts to T-facts* (following Feyerabend's suggestion) *to physical systems*. We agree with Kuhn that creative imagination is also needed in the step from M_{pp} to M_p, and incorporating a non-standard M_{pp} to an M_p. However, we think that to visualize unorganized phenomena in terms of organized phenomena or physical systems also requires creative imagination. The case studies presented below demonstrate this point more fully. Further, most revolutionary developments in science are due to the accomplishment of this step. While Kuhn is correct in saying that puzzle solving in the normal science phase consists in the step M_{pp} to M_p, in the revolutionary phase that eventually results in a new conceptual scheme, the step involved is from (unorganized) phenomena to physical systems.

Commenting on the condition (4) in the above definition of a theory given on page 136 above, Stegmüller says:

> Which conditions are *sufficient* to qualify something as a physical system is as yet an unresolved problem. ... For the time being a scientist must rely on his intuition and experience in deciding what a physical system is.[66]

We are heading toward proposing a solution to this problem in the next chapter. Here we would like to make a few more observations.

In the context of the generation of physical systems, two kinds of problems can be distinguished. First, what is the sufficient condition to know that some x is a physical system? Second, what is the sufficient condition to know that some x is a member of a class of M_{pp}, which is equivalent to the set of intended applications I of a scientific theory? The second problem is different from the first because not all physical systems are members of a single theory. Different scientific theories have different domains of applications, i.e., different physical systems, though some amount of overlapping is often found. We think that most of the anomalies of a theory get resolved ultimately when proper incorporation of a set of physical systems into another theory (often a new theory) is achieved. Since it is the set of applications that determine the identity of a scientific theory, a proper methodological answer

of a critique of scientific rationality. We think that it is one thing to say what scientific method is, and quite another thing to assess whether science as a social phenomenon has actually achieved its professed objective.

[66] Stegmüller *op.cit.* p. 163-164.

to this problem, if possible, is desirable.

The first problem continues to have relevance throughout the course of science. The problem of delimiting a physical system, both in the experimental context and the theoretical context, is a serious problem that scientists face regularly. In most cases realizing a physical system amounts to obtaining a closure. We will see below, in the case studies, how inversion plays a very decisive role in solving this problem.

A few more significant questions that are vital in understanding both the structure of scientific theories and particularly inter-theory relations, such as reducibility, may be stated in this context. Can a physical system be incorporated in more than one scientific theory? Can we have two different theories with a common set of physical systems as their intended applications? These questions are related to the question: Can one and the same 'fact' be explained by more than one theory? The questions are generally answered affirmatively by many philosophers. Though these questions appear simple, we think that the usual affirmative answers presuppose an incorrect view of what scientific theories are. We will, with qualification, answer in the negative to this set of interrelated questions. We have discussed this separately in the last section (§ 5.5 page 151) of this chapter.

Regarding the dichotomy of theoretical/non-theoretical, the Sneedian suggestion is not devoid of problems, though it looks the least problematic. Take for example, the use of expressions such as 'non-theoretical functions'. As already stated above, 'non-theoretical function' means, in the sense of Sneed, that a function is independent of a specific theory, and not non-theoretical in any absolute sense. Since theories are structures, can we interpret theoretical concepts as structured concepts? We may not get into any problem by doing so. However, we cannot interpret non-theoretical concepts as non-structured concepts. For non-theoretical does not mean non-structured, but only as not being dependent on a specific theory.

Therefore, when Sneed and Stegmüller are talking about the problem of eliminating theoreticity, they are not talking about eliminating structure. Every function or concept that is relevant for science is structured. Thus, though they appear to be proposing a definition of theoreticity, they are actually not doing so. Unless they specify which structures can be passed as scientific their proposal would not have much significance. For example, a function can be called theoretical with respect to theory T_1, and the same function can be called non-theoretical with respect to theory T_2. In both cases the function would be called scientific, for both T_1 and T_2 are scientific theories. So the significant part of the question remains: What makes a function scientific? or What makes a theory scientific? Which structures can be called scientific and which not? It is here, we think, that the weakness of Sneed and Stegmüller's suggestion lies. A very crucial question is left untouched. They start with what all of us call a scientific theory, and in relation to that introduce a distinction between theoretical and non-theoretical, without telling us the basis of selecting a body of knowledge as a scientific theory. Therefore, though they appear to have solved the problem of theoreticity, they have done so by neglecting an epistemological problem par excellence.

We have two proposals to make in this regard. First, we propose that 'theoretical' and 'non-theoretical' be replaced by the neutral and less ambiguous terms 'structure-dependent' and 'structure-independent' respectively. The second, is the proposal of an answer to the question: which structures are scientific? Though the suggested alternative terms are clumsier than the original terms, they however are clearer in conveying what is intended. The same terminology can be used even in the context of observation, where we can start using structure-dependent-observation, and structure-independent-observation, in place of theory-laden and theory-free observations. The second proposal is more crucial, for it is to directly confront the major epistemological question mentioned above. This brings us to the heart of the matter of the thesis. We shall propose to resolve this problem in the next chapter.

Apart from the suggestion of inverting the order of reconstruction, Kuhn appreciates Sneed's and Stegmüller's route to holism, while resolving the problem of circularity that one faces in the interpretations suggested for theoretical terms.

Since, according to Sneed and Stegmüller, a theory represents a set of applications, including a set of exemplary (paradigmatic) applications such as for example planetary motion, pendulum, free-fall, levers, balances and so on for Newton's theory, all of which can be connected together with the help of basic laws and the set of constraints. Each application or a subject-group of such applications can also be reconstructed as independent theories. But only in a connected set of all applications can the theoretical terms be interpreted without circularity.[67]

> If a theory, like Newtonian mechanics, had only a *single application* (for example, the determination of mass ratios for two bodies connected by a spring), then the specification of the theoretical functions it supplies would be literally *circular* and the application correspondingly *vacuous*. But, from Sneed's viewpoint, *no single application yet constitutes a theory,* and when several applications are conjoined, *the potential circularity ceases to be vacuous* because distributed by constraints over the whole set of applications.[68]

[67] *Ibid*, pp. 292-293.
[68] *Ibid*, p. 293, italics ours.

This sort of reason has given rise to holism, held by a majority of philosophers of science, including Kuhn and Stegmüller. Kuhn, being a holist himself, is applauding the route taken by Sneed with regard to the problem of reconstructing theories. However, we shall see that circularity is a problem only for the reconstruction of a finished theory in the top-down order, and not for a generativist whose order of reconstruction is in the bottom-up order. While on the one hand Kuhn suggests that the order of transformations be inverted, on the other hand he finds a need for a holistic interpretation of theoretical terms. This we see is unnecessary, because the circularity problem is an outcome of a lack of a satisfactory methodological reconstruction in the inverted order. We shall elaborate.

From a given set of problems, say of the problem of lever, floating bodies etc., if a theory (in this case, Statics) develops, such a theory can *survive* independent of other theories, like dynamics, though it cannot be independent of geometry. Such an independent theory can be reconstructed without recourse to any theory of dynamics. And the 'small' set of applications get local support from the 'small' theory, and the theory gets support from the applications. This circularity of mutual support looks like a logical problem only if the transition from unstructured data to structured data—the transition from phenomena to physical system, and from physical system to mathematical model—is not taken into account. If this process is properly understood, then we would get independent support for the theory without circularity.

The usual accounts of reconstruction have been trying to find noncircular interpretation of theory or theoretical terms without recourse to the context of generation of the theory, and the result was 'net-working' to get a holistic picture of a theory. Of course, initially philosophers tried to understand the formation of theories through inductive reason. Induction could not account for the 'leaps' involved in the process, and hence this led to the abandonment of the generativist solution to the problem. The failure of induction is well known, and we have dealt with it already. From the failure of the inductivist, this option of avoiding circularity cannot be regarded as closed, because there are noninductive options. Therefore, we think that holism is not the only way out for the problem of circularity involved in providing semantics for theories. Another reason is that the holist option is not attractive, because though net-working would save the logician locally from the problem of circularity, by linking with other elements in the theory, the theory as a whole, as a single body, 'hangs' without proper interpretation. The problem is avoided without actually solving it.

We shall tackle the problem of circularity by a non-inductive generativist interpretation of the elements of scientific knowledge, namely definitions, systems and models. And thus we see no necessary reason for adopting holism, insofar as the problem of circularity is concerned. Holism, however, can be invoked in other contexts where it is found necessary. We are saying this to make it clear that we are not against holism in principle, but we are merely arguing against a wrong reason for holism in the present context.

5.5 Theories and Domains

We have raised some questions above: Can we have two different theories with a common set of physical systems as their intended applications? Can one and the same 'fact' be explained by more than one theory? Can a physical system be incorporated in more than one scientific theory? Kuhn has also made few interesting observations regarding the problem of identifying (delimiting) the body of a theory. Is it justified to include an independent theory, such as statics, as a part of another theory, such as dynamics? Can we consider the classical formulations of mechanics and electromagnetic theory as constituting a single theory?[69]

We shall begin by considering these questions with a discussion on Dudley Shapere's views on the matter. Shapere has made an interesting suggestion that one should take the view that the body of scientific knowledge is more or less many independent theories with their different domains, identifiable on the basis of "good reason".[70]

If we consider a sophisticated area of science, such as physics, at a particular stage of its development, we find that the object for investigation is broadly subdivided into different fields, such as electricity, magnetism, light, etc. These related objects of investigation are considered to form *domains*, bodies of related items.[71] The formation of such domains is not an automatic process. What are the grounds for considering the items of a domain as being related to each other to form a unified subject-matter? An answer to this question is not straight forward. It is indeed a necessary aspect of the question of formation or genesis of scientific theories. The complicated nature of the issue may better be stated in Shapere's own words.

> Although in more primitive stages of science (or, perhaps better, of what will become a science), obvious sensory similarities or general presuppositions usually determine whether certain items of experience will be considered as forming a body or domain, this is less and less true as science progresses (or, one might say, as it becomes more unambiguously scientific). As part of the growing sophistication of science, such associations of items are subjected to criticism, and often are revised on the basis of considerations that are far from obvious and naïve.

[69] *Ibid* p. 295-296.
[70] Dudley Shapere 1977, "Scientific Theories and Their Domains" in Suppe (ed.) 1977, *op.cit.* pp. 518–570.
[71] *Ibid* p. 518.

> *Differences which seemed to distinguish items from one another are concluded to be superficial; similarities which were previously unrecognized or, if recognized, considered superficial, become fundamental. Conversely, similarities which formerly served as bases for association of items come to be considered superficial, and the items formerly associated are no longer, and form independent groupings or come to be associated with other groups.* ... Even where the earlier or more obvious associations are ultimately retained, they are retained only after criticism, and on grounds that go beyond the mere perceptual similarities or primitive uncritical presuppositions which formed the more obvious bases of their original association.[72]

What are these *items* that are associated together in a domain? Are they 'facts' or extensional objects, such as physical objects or systems, or intensional objects such as concepts, models etc? Are they problems? From the examples and characterization given by Shapere it is more likely that what he has in mind can not so easily be considered a cut and dry answer characterizable either as extensional or as intensional. His characterization suggests that the item is an "information" such that a domain can be talked of as the total body of information. Each item is not a problem, because, as he says, one aspect of finding unity among the items is a problem that affects all items.[73]

What are the grounds that assemble the items together into a domain? Not *any* relationship should count as reason. The relationship must be *well grounded.* One well grounded reason, Shapere suggests, that unifies various items into a domain is the systematizability of the items in an order, such as the taxonomic or evolutionary order.[74] There can be other kinds of orders such as functional (covariational) order that form the basis of a domain. A theory, then, is considered an answer to the problem the domain is generally facing.

We consider that this manner of approaching the problem of understanding the nature of scientific theories is worth pursuing. We agree in principle with the basic line of suggestion for the *generation of domains* as based on some systematization of the items. Some smaller domains may become items of a larger domain by virtue of another level of order than can be obtained within the smaller domains. This could be related to intertheoretic reductions that often take place. We will follow more or less this suggestion of Shapere while working out our proposal for the generation of theories in the next chapter.

Is it not good enough to proceed toward answering the problem of identifying or delimiting the class of physical systems that ultimately become the set of applications of a theory? Here we are fusing the apparently similar notions—the set of applications (intended

[72] *Ibid*, p. 521.
[73] *Ibid* p. 525ff.
[74] *Ibid* p. 534ff.

or realized) as equivalent to Shapere's notion of domains. The answer is not yet specific enough to 'logicize' the matter, but certainly far ahead of the uncertain answer Stegmüller gave to the problem. It falls somewhere between pragmatics and a methodological solution. Following this approach would help us put forward our thesis of relativizing both meaning and truth. Here we shall outline the idea.

Why shouldn't we look at different theories as different bodies that can be more or less independently believed or disbelieved? Is it not possible to consider dynamics as a separate theory and statics as another theory? Let us make these questions clearer by an analogy. Take different organisms as different theories. The phylogenetically-later organisms are dependent on the former organisms, because without the former the latter could not possibly have come into existence. Similarly the generatively-latter theories are semantically dependent on former theories. However, the organisms are independent bodies in the sense that each has more or less adapted to different environments. They are different bodies because they are structurally different. The genotype of different bodies is different, despite their phylogenetic dependence. The survival of the organism does not depend on whether the former organisms are extant or extinct.

Isn't it reasonable enough to think that scientific theories are also independent bodies in the sense that each has its own domain of application? Theories do show structural difference though they are genetically dependent on former theories, just as different species of organisms differ anatomically, though there exists a clear phylogenetic relationship with other species. Since it is possible to believe or not believe in one theory and not others, in a given context, the theories appear more like independent bodies with their own semantic content. This way of looking at things appears more plausible because, despite formal reductions that are often obtained between different theories, the reduced theories never lose their independent 'existence'. This looks more true to the way scientists operate with theories. Euclidean geometry, Newtonian theory, classical electromagnetic theory, etc., have all been *stated* to be dislodged by more general theories. However, the facts look quite different if we see the manner in which practicing scientists continue to employ those so called 'dislodged' theories even today.

The worthiness of the nonstatement view is that it allows us the possibility of holding to Euclidian geometry at time t_1, and to non-Euclidian at time t_2, depending on the object that one is studying at a given time, without involving any problem of rationality. The same we would say for Newtonian and Einsteinian theories. Einstein and few others of his genre might have dislodged Newtonian theory from their mind. But not the entire community of scientists. Scientists continue to work with Newtonian theory, Euclidian geometry, natural numbers, integers etc., whenever they find them relevant. The gestalt shifts that take place appear to be within the mind of some individual scientists, and not in the scientific community at large. The dislodgement thesis is not even historically true. Which mechanical engineer would say that he has dislodged Archimedian statics from his mind? Machines are constructed even today by employing the same foundations.

We think that Kuhn's account is true to what happens in the mind of a scientist, especially the sort of geniuses like Einstein who could have gestalt experiences. There are many scientists who can solve problems in different fields such as Newtonian physics, relativity theory, quantum mechanics etc., and know how to switch from one conceptual scheme to another depending on the problem at hand. Therefore we think that Kuhn's thesis of theory dislodgement is a psychological dislodgement and is not an entire dislodgement from the body of scientific knowledge.

We think that it is possible to reconstruct the so called scientific theories as several independent *fields* or *domains* of inquiries identifiable by the distinct *taxon* of physical systems or the set of intended applications. Falsifications of a field of inquiry would delimit their boundaries, and would never make them them entirely false. We have, to the best of our understanding of the history of science, not a single field of scientific inquiry that went into oblivion because of falsification. With regard to theories such as phlogiston chemistry and Aristotle's physics which have been totally abandoned, we would say that they cannot be called scientific at all, since our criterion of demarcation of scientific knowledge is that the field or domain of knowledge must have acquired a level of inverse systematization. Since phlogiston chemistry and Aristotle's physics have no such structure they have been totally abandoned. (We have demonstrated this claim in the case studies in Part-III.) Not a single instance can be found, to the best of our knowledge, of a theory that has acquired a level of inverse systematization and has been shown to be abandoned completely.

It is easy to see that in this picture *both meaning and truth are localized* and relativized with respect to the structure and the domain of applications in which it can find value. By relativizing the interpretation of theoretical/nontheoretical terms Sneed and Stegmüller have already *localized* meaning. However, since they believed in the dislodgement of one theory by another, except in the crisis period, we do not see the simultaneous occurrence of competing theories.

This we think is a mistaken picture of science. Not only do several theories survive simultaneously, due to their independent structure and domains, science cannot be accommodated in just one theory, such that *that* theory could dislodge the rest of the domain specific theories. All formal unifications are at the cost of *leakage* of semantic content and therefore remains a logical exercise, though it has different epistemological lessons to teach. If we wish to be true to history and the practice of science, the correct picture of scientific knowledge requires not only a localization of meaning but also of truth. This localization of truth cannot be correct if the thesis of dislodgement of one theory by another is correct. Therefore, this *apparent* dislodgement stands in need of explanation.

Chapter 6

Inversion

6.1 Inversion in Mathematics

The role played by inverse operators and formal (algebraic) equations in understanding the extensions of the number systems is rather well understood by both mathematicians and historians of mathematics. We will extend and generalize this understanding to all branches of scientific knowledge. In the process of developing the thesis we elevate the idea of inversion to a logical and methodological status. The story of the development of the various systems of numbers, therefore, can be regarded as our first case-study of the thesis. Presenting this in the beginning of this chapter would not only make the introduction of the idea easier—because most of us are familiar with the development of number theory, but this example is also useful in supplying many terms related to the idea of inversion that we shall adopt for developing the thesis. Before we turn to the case of mathematics, a few more comments may not be out of place here regarding the need to open up a methodological and philosophical study on the idea of inversion.

Though mathematicians and scientists have been *applying* the idea of inversion and in this sense are rather familiar with the idea, the fundamental and philosophical significance of the idea in logic, epistemology and philosophy of science must be regarded as *unexplicated*. This dissertation can be viewed as an attempt to fill this lacuna. We have worked towards elevating the idea in order to provide for it at least as fundamental a place, (if not more), as negation presently occupies for a logician. Since the nature of inversion, as we will see, is necessarily *constructional* or *synthetic*—i.e., ampliative—the stated elevation of inversion to logical status would undoubtedly alter the received view that logic is essentially *analytic*. In this sense what we are going to present below should be viewed as an attempt towards articulating a logic of the synthesis of abstract ideas. And since scientific 'theories' are constructions based on abstract ideas what we are presenting can be seen as an attempt to throw open an idea such that more rigorous statements of a logic of scientific discovery can be worked out in future.

We think it will not be unfair to claim that there is no precedence to a proposal of this nature, to the best of our knowledge, at least in the history of philosophy. Prima facie evidence of this can be that we do not normally see any entry of the term 'inversion' in either the contents or the index of any common books or treatises on the methodology of science.

It may however be noted that the term 'inversion' is used in traditional logic, but in a different sense. This usage is in the context of immediate inference based on the form of categorical propositions A, E, I, and O. Two statements in this context, can be said to be equivalent in the sense that if one is true or false, the other is also true or false. So it is possible to transform certain statements in a valid manner to yield equivalent statements. For example, the statement "All physicists are mathematicians." and its inverse "Some non-physicists are non-mathematicians" are equivalent. Here in this context the transformation consists in changing both the subject and the predicate to their contradictories. Other simpler transformations such as *obversion* and *conversion* are also possible.[1] We will, however, be using the term 'inversion', first, *not as a relation between subject terms and predicate terms*, second, the terms that are inversely related *are not part of a statement*, but a complex conception or structure or nonstatements (§6.4 page 172), and third, *the inverses are not equivalent expressions* for conveying the same information, but are complementary.

We shall start with what is already said about inversion in connection with the development of number theory.

Noted historian E.T. Bell writes that inversion is "one prolific method of generating new numbers from those already accepted as understood".[2] According to Felix Kaufman "the simplest path towards understanding the so called extensions of the number concept lies through the operations inverse to addition, multiplication and potentiation".[3] Richard Dedekind, one of the great mathematicians of the last century, says that the problems peculiar to inverse operations have been the motivation for a "new creative act."[4] An appreciation of these remarks will be easy to any one who followed the course of the development of number theory. We shall give a brief outline of the pattern in which the development took place

[1] Cf. Cohen and Nagel 1936, *An Introduction to Logic and Scientific Method* Chapter-3, § 3, p. 57ff. Also Carney and Sheer 1980, *Fundamentals of Logic* pp. 263–266.

[2] E.T. Bell 1945, *The Development of Mathematics* p. 172.

[3] Felix Kaufman 1978, *The Infinite in Mathematics* p. 91.

[4] Richard Dedekind 1901, *Essays on the Theory of Numbers* p. 4.

in a manner that highlights the role of inversion in the process. It may be made clear that our concern here is to see the role of inversion in the process, and hence we shall not give a historical account, and shall concentrate only on those crucial moments where inversion enters into the process. Therefore, the account that follows is quite a 'compressed' reconstruction.

Our acquaintance with numbers begins with natural numbers, 1, 2, 3, ... Centuries of our engagement with numbers resulted in an increase in the knowledge of them, which consists in discovering new operations, properties of the operations, interrelationship between different numbers and operations etc. As a result of this the so called *new kinds* of numbers were introduced into the number 'line'.

We shall start *creating* new knowledge of numbers from what we already know. Therefore, let us suppose that we know what natural numbers are and also how to increase any given number by one unit. With this preliminary knowledge we can define an operation called addition: starting with any natural number a we count successively one unit b times. The definition can be formally represented by $a+b=c$. We can also define another operation called multiplication: adding any natural number a to itself, b times in succession, represented by the formula $a \times b = c$. Using the definition of multiplication we can define yet another operation called potentiation or raising to a power: multiply any natural number a, starting with one, b times in succession. This operation can be represented by the formula $a^b = c$. In addition let us also suppose that the operations have some or all of the properties such as associativity, distributivity, commutativity etc. Since our interest is not in developing a rigorous axiomatic system for natural numbers, we need not bother presently about the status of these properties, whether they are given in the axioms, or theorems, or definitions etc.

It is however important to discuss the *principles of closure* that are usually specified with respect to each operator. The above operations can be said to be closed, because the result of any of the operations mentioned above with the set of any natural number is always a member of the set of natural numbers. These operations do not or can not generate any non-natural numbers. In modern algebra the very idea of an operation implies the principle of closure: An operation on a set, S, is a rule that assigns to each element of S precisely one element of the same set S. The operations that are closed with respect to the set of natural numbers are generally called *direct operations.* We will discuss below how aptly this term 'direct' applies to these operations, and why these operations follow the principle of closure. A simple answer can be that because the set S is determined on the basis of an operator. This is to say that the closure, in the literal sense of being a delimitation, is *operational.*

Contrasting this operational closure with 'classificatory closure' may bring out the essence of most mathematical *kinds.* When we delimit class membership on the basis of a property being common to all the members, we get a 'classificatory closure'. All that we need here is just the commonness of one property, and therefore ordinary classification gives no guarantee as to the homogeneity of the members.[5] However, in the case of operationally closed classes, the members cannot but be homogeneous. We will see below that this is accountable on the basis of a special kind of identity called *invariance*.

We will argue below that this operational closure is not only central to mathematics, but also central in defining abstract objects in natural science. Though the system of natural numbers has an end in itself, it becomes the source of new set of problems. The nature of the source of problems is highly significant in understanding the nature of *scientific change*.

Though all natural numbers have a successor relation to each other, this relation is *unidirectional*. All the operations that we have mentioned above are such that the result of an operation is always greater than or equal to either of the numbers involved. That is, given an equation $a + b = c$, if a, b, and c be any natural number then $c > (a \vee b)$. The only direction of 'growth' therefore is towards greater and greater natural numbers.

Apart from this limitation fresh problems crop up, because a few problems of very great importance can not be answered. For example: What values of b satisfy equations such as $a + b = c$, $a \times b = c$ and $c = a^b$, given the values of a and c.? Such problems are commonly called *inverse problems*, as against the solvable problems of the system, which may be called *direct problems.* In order to solve these apparently solvable problems, we need to introduce a new set of operations, commonly called *inverse* operations. The inverse operations corresponding to the above *direct* operations are subtraction, division, root and logarithm. However, by merely adding the inverse operations only some of the inverse problems can be solved. The following are such problems: (a) $b = c - a$, where $a = c$, (b) $b = c - a$, where $a > c$, (c) $b = c/a$, where $a > c$ and $a \neq 0$, (d) $b = \sqrt[2]{2} = 2^{1/2}$, (e) $b = \log_{10} 2$, and (f) $b^2 = -1$. In order to distinguish this new set of inverse problems from the former set, we shall call the former *general inverse problems* and the latter *special inverse problems*.

Solving such inverse problems has always introduced new peaks in mathematical development. In his *Essays on Number Theory* Dedekind says that the performance of inverse operations proves to be limited, while that of direct operations is always possible. He further says:

> Whatever the immediate occasion may have been, whatever comparisons or analogies with experience, or intuition, may have led thereto; it is certainly true that

[5]Taxons, unlike classes, do contain homogeneous members.

> *just this limitation in performing the indirect operations has in each case been the real motive for a new creative act*; thus negative and fractional numbers have been created by the human mind; and the system of all rational numbers there has been gained an instrument of infinitely greater perfection. [6]

By 'indirect operations' he meant 'inverse operations'. Here Dedekind is referring to the development of rational numbers, and hence he refers only to the negative and fractional numbers, but the same can be said about the other cases also. The limitations that he is pointing to here are nothing but the special inverse problems with respect to subtraction and division (problems (b) and (c) mentioned above). He is one of those mathematicians who believed in the reality of only the natural numbers, the rest are "created by the human mind". Given the usual interpretation of 'creation' as an act of mind (mental construction), we may say that new 'kinds' of numbers are 'theoretically' and indirectly obtained. Our knowledge of them is indirect, in the sense that the mind's constructive role is involved. Our knowledge of natural numbers, on this view, are considered 'real' and direct.

The number systems constructed, starting from integers, have for every number an inverse number i.e., the new number systems thus introduced necessarily contain inverse elements. This inverse relation gives rise to bilateral symmetry of the number systems. In such well formed systems all the fundamental operations, including inverse operations, are always performable with any two individuals in the set S, yielding a result, which is also an individual of the set S, with the only exception of division by zero, thus making all possible questions well-posed and meaningful. Thus we can interpret the story of the development of numbers as the story of inverse problems, both general and special, leading to the corresponding enrichment of the system, where the general inverse problems needed the introduction of new inverse operations, while the special inverse problems needed the inclusion of new 'kinds' of objects into the S leading to the modification of the principle of closure, in order that all algebraic equations can be made solvable. We will regard these *two* as separate additions, though interrelated.

Our motivation in distinguishing the general and specific inverse problems requires some explanation apart from the reasons specified above. When we approach a special inverse problem it is possible to define an inverse operation allowing certain exceptions, i.e., by introducing certain restrictions such that the result of the new operations still belongs to the older set of natural numbers, thus making the special inverse problems ill-posed. Though these restrictions might appear artificial, if we look at the actual historical development we find that most mathematicians have initially shown tremendous reaction against accepting

[6] R. Dedekind 1901, *op.cit.* p. 4. Italics ours.

Special Inverse Problem	'New Entities'	New Number Systems
(a) $b = c - a$, where $a = c$	Zero	Positive integers
(b) $b = c - a$, where $a > c$	Negatives	Integers
(c) $b = c/a$, where $a > c$, and $a \neq 0$	Fractions	Rational Numbers
(d) $b = \sqrt[2]{2} = 2^{1/2}$, and $b = \log_{10} 2$	Irrationals	Real Numbers
(e) $b^2 = -1$	Imaginaries	Complex Numbers

Figure 6.1: Inverse Problems Leading to the Construction of New Systems of Numbers

the created 'entities', because some of the inverse numbers like negatives and irrationals could not be given any phenomenological interpretation.[7] Most mathematicians decided to operate with limitations rather than enter into, what they thought, a totally imaginary and mystic world. Centuries had to pass before satisfactory interpretations were provided by giving not only operational significance but also a geometrical interpretation to the *new* numbers. Ultimately mathematicians had to make room for the new candidates because these restrictions made the problem solving power of the operations highly limited. It is this limitation that motivated mathematicians to 'create' new 'kinds' of numbers. Therefore, special inverse problems, which needed for their solution the *creation of new entities*, bear greater significance than the general inverse problems, specially from the point of view of the intellectual turmoil that can be caused by the nature of scientific change in the minds of thinking people.

Once we are in possession of a formal system that operates on a delimited set of numbers including the inverse elements, it becomes unnecessary, from a formalist point of view, to say that the system has two operations—direct and indirect, because the operation is defined to operate in a set that contains inverse elements. However, from a generativist's point of view the problems that made the definition of inverse operations necessary are prior to those special problems that made the creation of inverse operations necessary, i.e., general inverse problems are prior to special inverse problems. The generative potential of inverse operations gets masked in a formalist reconstruction, where the definition of an operation goes hand in hand with the specification of a closure principle which delimits the set without ever naming the elements.

The table shows which inverse problem led to the creation of which kind of number and the corresponding new set of number systems invented.

The above sketch of the development of our knowledge of number systems is ex-

[7] Cf. E.T. Bell *op.cit.*

tremely brief. Apart from inverse problems there are other motivations and also other justifications for introducing new kinds of numbers leading to the development of new systems. One of the most significant of them, which may be noted here without discussion, is that of the geometrical interpretation and application of number systems. Also the much involved discussions on finite and infinite on the one hand, and continuous and discrete on the other are extremely vital in getting a complete picture of the development. By pointing to this we wish to warn the reader that our sketch has been deliberately, though not deceptively, made simple, to meet the objective of highlighting the role of inversion.

A number of questions of philosophical significance can be raised in this context. What is the nature of the change of knowledge that took place in the development of our knowledge of numbers? Is our knowledge of numbers moving towards 'verisimilitude' in the world of numbers? Is there progress? In the course of development, did we *redefine* the operations to get better and better operations or construct entirely new notions? Did we also *modify* the principles of closure, to get a better operation each time or did the development consist in the *multiplication* of the number of kinds of numbers, operations, etc.? Are we moving toward greater and greater economy, because given the algebra of complex numbers we can solve all algebraic equations? In what sense do we say that real numbers include the naturals and rationals? Are we, after all this development, in possession of one or more 'theories' of numbers? Did we "falsify" the "theory" of rationals when we moved on to the "theory" of reals? Did we *dislodge* one "theory" by another in the process?

These questions are deliberately formulated in a manner such that the analogy with the usually discussed problems of philosophy of science, specially in the context of scientific change, becomes clearer. One might at once say that the development of mathematical ideas is one thing, and the development of natural scientific ideas is another. Some others might say that to the extent that most of modern science is unquestionably involved in abstract constructions, understanding in terms of a developmental model of mathematics might not pose much problem. But, to the extent that natural science has to deal with phenomena that are nearer to direct experience, the analogy may breakdown. However, we strongly think that the history of natural science has sufficient evidence to suggest that the analogy is neither superficial nor misleading. We cannot get into the problem of change of knowledge in greater detail, for our primary concern is to demonstrate the possibility of a logic of discovery. However, there will be occasions where we shall indicate in anticipation what our position would be with regard to these questions.

It is necessary to make another observation regarding the nature of the number systems that are eventually generated. The set of natural numbers are *not symmetrically structured,* while other systems of numbers, integers, rationals, reals and complex numbers are symmetrical. The reason is obvious. The presence of inverse elements (numbers) in the latter systems of numbers introduces this new *epistemic value* to their structure. In what sense this is epistemic will become eventually clearer. Since these systems are symmetrical they can be constructed as *groups.* The structural features of these abstract objects called *groups* have certain properties that can be extended to other symmetrical structures that we often find in the sciences. In the following sections, we shall be using the primitive vocabulary of group theory. In Appendix-A properties of groups are presented.

6.2 Structure-Dependent Concepts

We have mentioned above that it is possible to talk about scientific knowledge without the use of the unclear terms 'theoretical' and 'nontheoretical'. Though it is possible to use the same terminology and make the sense clearer by redefining the terms, it is always a better approach to name them differently, for it would cause least confusion. Another reason why we think that the term 'theory' makes epistemological analysis difficult is because in most uses of the term it refers to a very large body of scientific knowledge. For reasons already explicated we will defend here a view of scientific knowledge which is seen as a collection of a large number of independent structures, each with its corresponding domain of application. We therefore propose a distinction between *structure-dependent* and *structure-independent concepts* to replace the traditional distinction between theoretical and nontheoretical concepts. The distinction that we are proposing here is not the same as the observation-theory dichotomy, because, on the basis of the distinction being proposed, certain notions which we will regard theoretical may be observational according to the older dichotomy, and vice versa.

In order to bring out the distinction we shall compare the structural features and certain applications of natural numbers and integers. Consider a situation of loss and profit in a monetary transaction. In order to know whether a person incurred a loss/profit of some money in a transaction, we need to relate (compare) one state-of-affairs with another. Since we can make sense of the meaning of being in a state of loss/profit/balance only in relation to another state-of-affairs or situation we can say that the meaning of some terms, such as 'loss'/'profit' gets generated only *within* a relationship. We can call the form of that relationship a *structure*, and the terms that find their meaning in relation to the structure can be called *structure-dependent.*

On the other hand consider a situation of counting currency notes when we receive them at a bank counter. Each time we count we count them directly, and quite independently of other states-of-affairs. In this case the amount counted will not have any 'tag' attached, except possibly the units of currency, unlike the above case, where special mention is always made of whether the amount is to be regarded as loss or profit or balance. Since the description of the latter situation does not involve any other situations, we call this *structure independent.*

In the former case the three different possible situations can be described in terms of positive, negative or zero situations, depending on whether a person is in profit, loss, or balance. This should be the nature of the state-of-affairs to apply a system of integers because there is a clear *isomorphism* between the three possible states and the three 'kinds' of integers.

It is important to note that when a person says he is in a 'zero' state of economy in the former case, he does not mean that he has no money at all with him. Rather he is referring to a no loss and no profit situation (or neither-nor-situation). So '0' when used in this case has a different meaning from '0' when used to refer to an empty purse. *The two meanings of '0' mentioned are incommensurable, for they are entirely different measures.*

Thus while counting currency notes at the bank counter, we don't make use of negative numbers, nor do we use positive numbers, we use numbers pure and simple. This therefore is a state-of-affairs that needs natural numbers. When integers and natural numbers are used in entirely different situations, how can we say that integers are 'superior' to natural numbers? For a mathematician whose main concern is to solve algebraic equations, to be in possession of integers, rationals, reals, etc., is naturally preferable.

Are the operations involved in the two cases the same? No, because in the former case where we need to state either loss or profit, some *calculation* is necessary, while in the latter case we engage in only simple *counting.* Isn't calculating different in nature from counting? Counting may be involved in calculating, but calculating involves more than counting. Calculation is structure-dependent, while counting is not.

Doesn't counting involve any structure? After all the series of natural numbers also has a structural form. But the 'linear' order of natural numbers and denomination of currency are presupposed in both the cases. Since what is common to the two situations does not enter into consideration when we make comparison, the stated distinction is independent of these other situations. Thus the crucial point that determines the distinction is whether the structure that is involved is a pattern suggesting any situational variance or not. We will see below that proportionality relations that scientists often assert between varying parameters are structure-dependent in this sense. Since natural science, in the state in which it is today, is inconceivable without proportionalities, this distinction is crucial for bringing out its essence.

Another example may make the intended distinction clearer. When we apply color concepts to describe objects, each application of a kind of color to a state-of-affairs is independent of the other, because each color forms a separate category. Different color concepts generally do not possess any structural relationship with one another. We can say that our knowledge of colors at this stage is *amorphous,* in the sense that the place of different colors in that abstract class of all colors is not in relation to any other colors. However, when we start understanding some relationship between colors, such as when the three primary colors are seen as giving rise to the rest of them, our knowledge enters into structural form. This structure has an order that can lead to what can be called 'the chemistry of colors', and is still not ordered in a manner that can suggest any mathematical order.

On the other hand when a scientist says that an object is emitting a radiation of a particular frequency, our knowledge of that object's radiation property is structure-dependent, because it presupposes the structure of a wave with a specific frequency, wavelength, amplitude, and velocity of propagation. Here in the specific use of the term 'frequency' its meaning is not independent of the other properties a wave would have. This structure dependence is usually formulated in the form of a mathematical equation because the structure is a relationship that exists between other parameters of a light wave.

Though we might say that emitting a specific frequency by an object means it has a specific color, this 'equation' or 'reduction' is misleading just as different uses of '0'. Once we understand the coextensional relationship between a color concept and a particular frequency, there occurs a transformation in our knowledge of what a color is. This is similar to the kind of transformation that took place in the case of numbers. Though we continue using the same symbols they do not refer to the same object. In ordinary usage color concepts are used to describe objects, while in the case of scientific usage though such a description is possible, the uniqueness consists in the ability to describe *the object color*. That is to say that different kinds of radiation distinguished are different independent objects that scientists talk about. In this sense the objects of scientific inquiry can be stated to be qualities. The process of transforming a quality into an object of study is essentially what is involved in making a quality measurable.[8] Since the knowledge of light in terms of frequency, wavelength etc., is based on proportions (see §6.10 page 195), while the notion of identity employed in the case of colors is of the type-token kind (see §6.5 for distinction of types of identities, page 174) no

[8]This is what, we think, is meant in *hypostasizing* qualities as explained by E. Meyerson, E. Cassirer and recently by E. Zahar. See §6.7 below.

identity of the notions can be claimed. In a similar manner the common man's usage of the term 'massive' is different from the scientist's usage of the term 'mass'.

The nature of the distinction that we are proposing is more or less *absolute,* unlike the distinction between theoretical and non-theoretical functions proposed by Sneed and Stegmüller, which is relative. As already stated we think that all scientific knowledge is structure-dependent, while commonsense knowledge is structure-independent in the relative sense (because commonsense knowledge is not free of abstractions though free of scientific abstractions). Since we see a possibility of demarcating scientific structures from non-scientific structures on the basis of inversion, we think that the dichotomy being proposed should ultimately enlighten us on the nature of scientific and non-scientific knowledge. The kind of relativization as proposed by Sneed and Stegmüller would help us in understanding the distinctions within the body of scientific knowledge, and cannot help us to distinguish science from non-science. This is because their definition of a scientific theory does not answer the question: What character is in the structure of a theory as a function of which it can be said to be a scientific structure? We propose that *inversion* is that character which differentiates scientific structures from non-scientific structures. This is our *specific* criterion of demarcation.

In the sections that follow, we introduce the sense in which inversion becomes an essential character of scientific knowledge. In the section that follows we shall contrast negation with inversion in an attempt to demonstrate that inversion is as fundamental as negation.

6.3 Inversion and Negation

Although the idea of inversion is well known and has been used in many different contexts we will start from preliminaries because of its pivotal role in the thesis. This is also essential for making explicit the sense in which the idea is seen as an essential *logical* relation for a possible logic of construction.

The fundamental nature of the operation negation is well known to philosophers and logicians. Deductive logic is impossible without negation. We say this on the basis of the well known fact of logic that with $\sim$ (negation) and either $\wedge$ (*and*) or $\vee$ (*or*) we can get a functionally complete system of logic. However, one might say that logical reductions to one primitive operation, such as *alternative denial* and *joint denial,* have shown that a functionally complete system of logic can be obtained with only one operation (only one *connective,* to be precise).[9] These claims are interesting when we look at the matter from

[9] Cf. Quine 1951, pp. 45-49, for the definitions, truth tables and a general discussion of the connectives

a formal mode alone, i.e., viewing them as only symbols without interpretation—the only interpretation being the truth tables.[10] However, when we look at the manner in which we start making sense of the above mentioned primitive connectives, we tend to see them as species of denial or negation. Though the names suggested by Quine, *alternative denial* and *joint denial,* are to be formally viewed only as *names* of connectives defined in a specific manner, all *applications* of them suggest that the primary mode of *denial* or *negation* is implicit.

Take for example, the case of joint denial $\downarrow$. The sentence $(\phi \downarrow \psi)$ is true when both ϕ and ψ are false. Therefore, it is suggested that $(\phi \downarrow \psi)$ be read as 'Neither ϕ nor ψ'. In other words, it is equivalent to the sentence $(\sim \phi \vee \sim \psi)$. Though formally the reduction to one primitive is successfully achieved, the only cases where such primitives can be *applied* are cases where an equivalent form of the sentence would *necessarily* contain negation. Since all well-formed-formulae where the joint denial $\downarrow$ occurs can be translated into an alternative form where $\sim$ and another connective (such as $\wedge$ or $\vee$) *necessarily* occur, we can conclude that it is impossible to construct a deductive system without any primitive connective that either explicitly or implicitly involves negation.

There is another fundamental reason to regard that deductive reasoning is fundamentally dependent on negation. Of the three principles of logic—the principle of identity, the principle of contradiction and the principle of excluded middle—the latter two employ explicitly the operation negation. *Nothing is an assertion unless we deny at the same time the negation of the assertion.*

Can we conceive of any alternative logics that are not based on the above principles? Is negation the only species of opposition? Aren't there other ways of opposition that we regularly employ in our thought? Since negation is fundamental to any assertion, no logic of assertion can ever be conceived without it. Are we capable of thinking without employing any assertions? If the assertive mode of thinking excludes nothing, then it is legitimate to say that we can't think without making any assertions. But fortunately our thinking abilities are not limited to the assertive mode alone. We have a mode of thinking (we may also say we have a special mode of inference) that is neither inductive nor deductive.[11] This alternative logic is a logic of construction (synthesis), and is therefore necessarily ampliative. The structures

alternative denial and *joint denial.* Also Copi 1979, *Elements of Symbolic Logic* pp. 281-282.

[10] In fact the discovery story of these connectives will be a good instance of constructive thinking.

[11] We have already argued above (§4.8 page 107) how inductive logic should be viewed as a logic of abstraction, and that it is based on *the principle of excluded extremes*.

that are constructed belong to the logical category of concepts. Since the outcome of the logic being developed is a concept, and the concept being a structure, the logic can be called *constructive abstraction* to distinguish it from *inductive abstraction* (§4.8 page 107). However, since we are specifically going to talk about the possibility of articulating a logical mode of constructive inference that is based on the logical relation, inversion, we will call it *inversive abstraction*.

Though modern logic has done remarkably better than traditional logic with respect to relations, the synthetic role of certain relations have not come to light because of the predominant tendency to view logic only as a tool of analysis. Dealing with logic always in a propositional or assertive mode has led to a state where even talking about the possible patterns of non-assertive modes of thinking means to certain thinkers 'illogical'.

The inference called inversive abstraction is based on a species of logical opposition called *inversion*, just as deductive logic is based on a species of opposition called negation. The modest objective is to convince the reader that there exists the possibility of formulating *at least* one more mode of ampliative inference that is not inductive and being ampliative certainly not deductive. We will present a tentative and non-rigorous formulation of what is being visualized.

It is of some interest to note that the notion of inversive abstraction is not too different from what Hermann Weyl called *constructive cognition,* or *constructive abstraction* (See below §6.3 page 169). We are attempting to enrich the notion by necessarily linking it with a logical relation of inversion—towards a methodology of ampliative logic. Weyl also contrasts it with inductive abstraction. He discusses the example of the formation of the concept of mass by Galileo who defines it as follows: Two bodies have the same mass if, at equal velocities, they possess equal momenta. This definition is arrived at by mental (creative) and experimental construction and is not inductively arrived at. In this case experimental manipulations are made, unlike in the realm of numbers where intellectual manipulations are made. These experimental manipulations make numerical determination of characters possible. Historically this was a turning point, because, before Galileo only geometrical characters are known to be amenable to numerical determination.[12] In the process mass became the dynamic coefficient according to which inertia resists the deflecting force. Motion

[12]Describing this Weyl says: "This is a step of great importance. After matter was stripped of all sensory qualities, it seemed as first as though only geometrical properties could be attributed to it. In this respect Descartes was wholly consistent. But it now [after Galileo] appears that other numerical characteristics of bodies can be gathered from the laws to which changes of motion in a reaction are submitted. Thus the sphere of properly mechanical and physical concepts is opened up beyond geometry and kinematics." (Weyl H. 1949, *Philosophy of Mathematics and Natural Science* p. 148.)

according to Galileo, depends on the struggle of two [opposing] tendencies, inertia and force, force that deflects the body from the path dictated by inertia.

This conceptualization of mass is markedly different from the Aristotelian way of ascending from particulars to universals, where only the really existing objects are concerned, for it is inductive.

> In the mathematical-physical or 'functional' formation of concepts, on the other hand, no abstraction takes place, but we make certain individual features variable that are capable of continuous gradation, ... , and the concept does not extend to all actual, but all *possible* objects thus obtainable.[13]

Therefore induction, as already noted in the above chapter, cannot explain the genesis of notions such as mass. For such concepts we need *constructive abstraction*. Weyl's characterization of it is as follows:

> 1. We ascribe to that which is given certain characters which are not manifest in phenomena but are arrived at as the result of certain *mental operations*. It is essential that the performance of these operations is held universally possible and that their result is held to be **uniquely determined by the given**. But it is not essential that the operations which define the character be actually carried out.
> 2. By the introduction of symbols the assertions are split so that one part of the operations is shifted to the symbols and thereby **made independent of the given** and its continued existence. There by *the free manipulation of concepts is contrasted with their application,* ideas become **detached from reality and acquire a relative independence**.
> 3. Characters are not individually exhibited as they actually occur, but their symbols are projected on the background of an **ordered manifold of possibilities** which can be generated by a fixed process and is open into infinity.[14]

The themes that we are presently developing are more or less contained in the above points on abstraction. In the context where the mental operations are performed, scientists are hardly concerned about the application, and thus they are in a nonassertive mode of thinking. Another point to take note of is regarding the role of the *given.* (See the underlined portions in the above quotation.) Though in constructive abstraction scientists begin from the given, they eventually get "detached" to acquire "independence". This becomes essential in order to transcend the limitations of inductive knowledge. The idea is not only to understand the given, but also to understand the "manifold of possibilities". Thus:

> All knowledge, while it starts with intuitive description, tends toward symbolic construction.[15]

[13] *Ibid,* p.150.
[14] *Ibid,* p.37-38. Boldface is ours, italics are original.
[15] *Ibid,* p. 75.

Weyl further says, citing Dilthey, that the scientific imagination of man is regulated by the strict methods which subject the possibilities that lie in mathematical thinking to experience, experiment, and confirmation by facts.[16] Thus we 'inherited' a lot from Weyl's insightful thoughts. In order to further this line of thought, however, it is insufficient to prove the strict methodological (logical) character of construction. We suggest that inversion, being a logical relation and—most importantly—being a constructive relation, contains the secret of a logic of construction. In what follows we present in what sense inversion plays a crucial role in this context.

We will use the term 'inversion' for a special kind of relation where the two terms that are oppositely related are opposite, or inverse, by virtue of a third term. In other words, the notion of opposition that is involved here is *relative* to a third term. Metaphorically speaking, here we not only have two opposite poles, but also a center. The three terms involved will be called a *triad*; the structure thus formed will be called *inverse structure*; and the mode of thinking that leads to such a structure will be called *inverse thinking*. Since it is by virtue of the third term that the specification of the inverse relation is made possible, the inverse structure will be identified by the name of the third term, and the third term of the structure will be called the *identity element*.

For example, -2 and $+2$ are inversely structured with respect to the identity element 0; 2 and $1/2$ are inversely structured with respect to the identity element 1. These examples, and the use of inversion in mathematics is well known. However, the point of the thesis is to demonstrate that it is the same inverse structure that gives shape to all scientific knowledge. Many examples from natural sciences will be presented below.

Both polar thinking and inverse thinking yields structures that put together the opposite terms. The nature of opposition involved in these special cases is such that the opposites are not viewed as contradicting one another, but are parts of the same structure. Since the opposite terms necessarily belong to one single structure they are both applicable together at the same time to that structure. The terms that are related by negation are not applicable to the same thing at the same time, as stated in *the principle of non-contradiction*: nothing can be both P and $\sim P$ at the same time. *The terms that are related by either inversion or polarity are applicable to the same object at the same time.* We will consider this principle sufficiently fundamental; it therefore needs to be added to the list of *principles of thought*. We will call it *the principle of included extremes*, for the opposites are included without contradiction in a structure.

[16] *Ibid,* p. 151.

We will illustrate the fundamental significance of the principle of included extremes in the genesis, development and the structure of scientific knowledge.

6.4 Coordinates of Scientific Knowledge

The relation between a type and its tokens can be said to be a *one-over-many relation*, which is also the relation between universals and particulars. This relation usually establishes a *hierarchical* order of things, for the two things so related belong to *different levels*. On the other hand a *one-to-one relation* brings two things of the same level into an order. Such an order is usually referred to as *ordered pairs*. Classification of things is a good example of one-over-many relation, while functional relations provide excellent examples for one-to-one relation.

The distinction between the two types of relations is rather well known, and hence hardly requires any further illustrations. However, we find it insightful to view the matter in a different manner. Since one-over-many relation introduces difference in the level of things related, we tend to depict such a relation by placing the things one above another in a *vertical* manner. Let us therefore call the systematization that is established by the employment of one-over-many relation a *vertical systematization*. An axiomatic system is a vertical systematization because the axioms and the theorems have a one-over-many relation. The structure of a hypothetico-deductive model is also vertical for the same reason. Taxonomic systematization is another example of vertical systematization, because the classes are arranged in several levels—one higher class including the subclasses in a nested manner.

Likewise, since a one-to-one relation is obtained between things of the same level, we can visualize them side by side in a single *horizontal* plane because the things thus related belong to the same level. Therefore, let us call the systematization that is obtained by a one-to-one relation a *horizontal systematization*. The various parameters that are functionally related in a *physical system* can be viewed as an excellent example of horizontal systematization, because all the parameters thus related are located in a system *together*, hand-in-hand, to form a sort of a horizontal 'plane'. For the same reason models also are horizontally systematized. However, the relation between a model and a physical system, as stated in a scientific assertions, is not horizontal but vertical. Because one model can have several physical systems as its instances, and as stated in the above chapter (§5.2 page 127), the proper relation that obtains between them is of the one-over-many kind. There can be several levels of models that can be placed one upon the other, depending on their generality (level of abstraction). For example, the three models, M, M_p and M_{pp}, described by

Stegmüller are structures related in a vertical order, though each kind of model is horizontally systematized. We shall shortly return to this point below.

Another kind of systematization should also be noted for the sake of giving a more or less complete picture regarding the kinds of systematizations obtainable in science. This third kind is *evolutionary systematization*. This kind of systematization is achieved by mapping the taxonomic order of systems on one hand, with a temporal *antecedent and consequent relation* of them on the other hand. The manner in which different organisms or systems have been located in a temporal evolutionary scale on the basis of the *antecedent and consequent relation* constitutes an excellent example of this third kind.[17]

These three kinds of systematizations can be viewed as the x, y, and z *coordinates* of scientific knowledge. The map of scientific knowledge that we are going to draw will make use of this manner of visualizing the different ways of scientific systematization. In this thesis, however, we will not deal with the vertical and evolutionary systematizations, for the objective of our study is to highlight the role of inversion in the generation of scientific objects such as definitions, models, and systems that come under the horizontal kind of systematization.

This distinction between the horizontal and vertical systems can now throw more light on the nature of the distinction between what is and what is not a statement. We will regard a statement (or an assertion) as an instance of a vertical system, because types and tokens belong to logically distinguishable categories of intension and extension respectively. We think that *nothing is a statement if it is not a relation between an intension (type) and extension (tokens).* This notion of statement can be applied to both individual statements and general statements, because in case of the general statements the place of extension will be a class of tokens, while in the case of individual statements it will be a token. Meta-level statements can also be interpreted in the above manner. Even for a relational statement this specification is sufficient, because in such a case the extension will be a pair, or a triad, etc., depending on whether the predicate is diadic, or triadic, etc. In fact all scientific assertions must be relational statements, in this sense, because—as stated above (§5.2 page 130)—the model and physical systems are stated to be related by isomorphism, over and above the type and token relation. Thus we think relational statements are special kinds of subject-predicate statements. A complete argument for this claim cannot be worked out here. We will however presuppose this, and develop the rest of the thesis on this basis.

We will regard any structure or system as a *nonstatement* iff the relation between

[17]Though we originally intended to illustrate this kind of systematization as a separate case-study in the thesis, we could not ultimately incorporate due to time constraint.

the terms is horizontal, i.e., the terms belong to the same level. The terms may all belong to the extensional category—as in the case of a physical systems, or the terms may all belong to the intensional category—as in the case of models and scientific definitions.

It is our claim that *scientific knowledge is a product of both horizontal an vertical systems.*[18] We further propose that horizontal structures of science, namely models, scientific definitions, and physical systems, are structures constructible on the basis of inversion, while vertical systems of science, such as taxonomic and hypothetico-deductive systems, are constructible on the basis of negation.

Horizontal structures are necessarily *nonstatemental* in nature. Stegmüller's models are horizontal structures, though each of them is related to the next level of models in a vertical order. It is this possible relation that enables the metalevel statements. In order to see the relevance of this observation let us consider the nature of the work of a physicist, theoretical physicist, and a pure mathematician. The levels which a theoretical physicist, for example, would mostly be dealing with are always above physical systems. His concern is usually studying the properties of the objects of a theoretically modeled world (possible world). The statement of a scientist when engaged in this sort of work can be stated to be applicable to the simulated world alone. A mathematician can be said to be working at other levels higher than a theoretical physicist. The statements made at this higher level would be descriptive of the models that become instances of the abstract algebraic structures. More higher levels of abstract engagements can further be identified where foundational attempts resembling those of Felix Klein's *Erlanger* program, or of the French structuralists' program of Bourbaki. At the lower level when a scientist states a relation between a model and the realizable physical systems that are believed to belong to *this* world, we get what can be called the scientific assertions which are either true or false. All other statements possible at higher than this level can be said to be providing the semantics (conditions of truth and falsity) to the possible applications of the various levels of conceptions.[19]

The difference in the levels of the various possible statements that scientists could (and do) make is another reason why a large structure such as classical particle mechanics should not be considered a single nonstatement (structure) as Stegmüller suggests. Therefore, we think that the defenders of the nonstatement view should make room for the various possible statements scientists make, by following our suggestion of vertically ordering the

[18] Since we are not elaborating the third evolutionary system at present, and we cannot not visualize at the moment what kind of picture might emerge after incorporating the third 'dimension', the above statement may be regarded tentative.

[19] It may be less confusing if we could conventionally name the different levels of statements possible by identifying them with the level involved.

various possible models. The number of levels of models need not be just three as Stegmüller suggests and we see no *a priori* reason for such definite specification. The defenders of the semantic approach should allow for enrichment of the view by distinguishing the different levels of models. We think that the conditions suggested for distinguishing the statement and nonstatement components on the basis of vertical and horizontal systematization should be acceptable to the defenders of the semantic approach and nonstatement view. With these suggested alterations, we see the possibility of lessening the problems of adjustments between the two structuralist positions.

Though the scientists' ultimate objective can be perceived as dealing with *this* world, their engagements higher up in the world of abstractions can in no way be regarded as non-epistemological on the ground that they do not deal with 'hard' truth. Most conceptions, i.e., nonstatements, of science have taken birth in these fertile contexts. Though several examples can be cited, the example of the abstract construction of the group of invariant transformations of velocity by Lorentz and its subsequent application by Einstein to the case of light can be regarded a paradigmatic one. To distinguish the nature of their achievement we can say that Lorentz *invented* the concept of velocity invariance, and Einstein *discovered* an application for the former's invention. This is one of the areas where a generativist has to search for the possible patterns of discovery (or invention), the other area being the genesis of physical systems from phenomena, as suggested in the last chapter, which will be dealt with in greater detail in the case-studies.

Most of the attention of philosophers of science has been paid to studying the logical properties of vertical structures or systems, which any way are definite constituents of scientific knowledge. However, the study of horizontal structures has not attracted many thinkers in mainstream philosophy of science. The study of horizontal systems, models, definitions, and physical systems, as important constituents of the anatomy of science has only recently, after the failure of the positivist's attempts, attracted the attention of the followers of the semantic approach. We think very strongly that these new non-traditional categories of understanding scientific knowledge will provide a significantly richer framework for future studies on this subject.

6.5 Types of Types

One common notion of the identity of an object or a class of objects emerges out of understanding similarities and dissimilarities between properties. This is more or less based on *comparing* one object with other objects in its environment. A reformulated definition of universals as offered by Socrates (see above §1.1 page 14) is obtained in this manner. The form of this definition shows clearly how an understanding of this notion of identity depends on the operation of negation. It is common practice to interpret this notion of identity in terms of a class—the property shared by a class of objects. This is more or less the same thing as what the type stands for in a type-token relation. Therefore, let us call this notion of identity *type-token identity*.

We are however interested in highlighting another notion of identity, that has become rather central to science, including mathematics. This notion of identity occurs in almost every discipline of science and is so predominant that it would be rather surprising to know how little this notion is employed in characterizing scientific knowledge. We would like therefore to give full attention possible in explicating the epistemological significance of this identity as well as its logical relation with inversion.

This identity, unlike the other, does not emerge out of the persistence of a quality in object/s, it emerges out of *change* or *variability* or *flux* or *transformation* ... of object/s. This identity refers to the *invariant pattern of variation* by capturing the *substance* of change. Right from antiquity the problem of explaining transforming properties of things has been a riddle, which ultimately finds consolation in this special notion of identity. We will call this vital notion *invariant identity* or simply *invariance*, though it is also known by several other names, such as *equivalence*.

We can find examples of invariance abundantly throughout quantitative science. Detailed discussion with further characterization and illustrations will be found throughout the following text. Therefore we shall be content with two simple examples here. Motion is usually regarded as a property of things. However, science deals with it as if it is in itself an object, because a scientist is not interested in the object that is moving or the kind of object that is moving, but motion *per se*. Galileo—as a true scientist—was interested in the *substance of motion*, and the notion of *inertia* and *acceleration* have thus became the first invariant properties of motion to be discovered. How inertia became a parameter of motion will be elaborated in the case-studies.

Weight can be characterized as a measure of quantity of matter of an object. However, the weight of the same object may change from place to place, hence it is variable. Therefore weight cannot be regarded as a satisfactory measure for quantity of matter by a scientist due to its variability. The discovery of *mass* as an invariant identity for a specific quantity of matter solved the problem. Likewise almost every measurable dimension of science has a corresponding invariance.

We are not very certain about whether this identity is the same as Leibniz's notion of the *identity of indiscernables*. We have therefore decided to develop the notion independently to avoid confusion. Another reason for doing so is that we are going to essentially link this notion of identity with inversion. This linkage to the best of our understanding has precedence only in modern mathematics and in the *genetic epistemology* of Jean Piaget. It may also be pointed out that anticipation of this notion of identity can be found in the writings of Ernst Cassirer, Emile Meyerson and Herman Weyl. However, we cannot at the moment either trace the history or explicate clearly the affinities or differences of this special notion with those of other thinkers, except with Piaget. In Piaget, more than anyone else, the notion acquires a very special significance specially in the context of generation, as well as its connection with inversion.

There is another notion of identity in which philosophers have shown a lot of interest. An object might undergo changes or variations in one or more than one property over a period of time. Take for example, water, which may appear in different shapes, different states like vapor, ice, etc. Despite these changes one or more property of objects may remain unchanged. The Aristotelian name for such properties is the *essential* property, as against the *accidental* properties that are taken to be contingent. This notion of identity continues to enjoy attention even to this day.[20] Kripke would interpret this kind of identity as that property of an object which it will have in all possible worlds, and therefore such identities are asserted by using *necessary* or *apodictic modality*, as against *possible* or *problematic modality*.[21] Since scientific knowledge is also regarded to be about the essences of things, this notion of identity of things has been regarded as a significant notion of identity of scientific concepts despite a number of philosophical problems. All natural kind terms are regarded as of this variety. Since it is easy to recall this notion of identity by the term 'natural kind', let us call this variety of identity of things as the *natural-kind-identity.*

We will regard invariance and natural-kind-identity as two *distinct* notions based on the interpretations given, and this distinction is indeed very vital for understanding the nature of scientific knowledge. Natural-kind-identity, as stated above, is that 'portion' or that aspect of an object or an individual that is *unchanging* in its history. In the case of a class of objects it is that *commonness* obtained after excluding all contingencies. On the other hand invariant identity is about the essence of a *changing* property of an object. Since most natural-kind *entities* are members of a *taxon,*[22] they are obtainable as a result of taxonomic

[20]Modern essentialists like Kripke, Putnam etc., have attempted to defend a version of essentialism.

[21]S. Kripke 1972, *Naming and Necessity* in *Semantics of Natural Language*, ed. by Donald Davidson and Gilbert Harman 1972.

[22]Taxon is not merely a class. Though all taxons are classes, all classes are not taxons. Taxon may be

systematization. Invariance, on the other hand, 'refers' to the essence of a property, rather than an individual. In other words, *invariance is properly attributable only to attributables of an object.* Therefore, we think, the two notions, natural-kind-identity and invariance are significantly different, though both are very useful in characterizing scientific knowledge.

6.6 Inverse-Definite-Descriptions

A description when used in a manner that it would pick out one and only one object, we call a definite description. In scientific communication scientists are mostly engaged with general concepts or objects, and hence definite descriptions are seldom used. It does not therefore mean that scientists do not engage in any specificity or that they do not refer to any identities. The clarity that is usually attributed to scientific thinking is also because of the *definiteness* of scientific assertions. Sometimes they need to locate a point in space in an unambiguous manner. And some other time they need to individuate one specific conceptual object in a 'world' of concepts. Thus, there is more than one level at which scientists need to talk in specific terms. Is there any specific method of achieving this scientific exactitude?

Kripke's notion of *rigid designator* would be one possible answer.[23] A number '9', for example, can be rigidly designated by the expression 'square of three' (3^2 or 3×3). But an object like the number 9 can be rigidly designated in an infinite number ways, and by any of them we can designate the object definitely. For example, instead of 3^2 we could have used $10 + -1$, $90/10$, $5 + 4$ etc.

We have seen above (§A page 319) that each group has a unique *identity element*. That identity can be referred to by either a finite or an infinite class of descriptions depending on the kind of structure used to designate an identity. For example, in the case of integers under the operation addition, 0, which is the identity element with respect to addition $+$, can be referred to by $(+1) + (-1)$, $(+2) + (-2)$, $(+3) + (-3)$, $\cdots$. Thus we can interpret a group as containing or as generating a finite or infinite class of *inverse definite descriptions* referring to an identity. Each such description consists of a pair elements which are inverse to each other with respect to a definite operation giving rise to an identity. Any one of the descriptions of this kind are sufficient to achieve the reference for the identity. This special kind of description with inversely related elements is a unique way of describing objects. We will call the class of such descriptions which designate an identity definitely by the name

defined as a *multidimensional* class, because it is a coextension of necessarily more than one property. The members of a taxon share constitutional or organizational or structural similarity.

[23] Cf. S. Kripke 1971, *Identity and Necessity* in Ted Honderich and Myles Burnyeat 1979, *Philosophy as it is* p. 467ff.

inverse-definite-descriptions. For the identity (object) 0 the following is the class of inverse-definite-descriptions:

$$0 \sim \begin{bmatrix} +1 & -1 \\ +2 & -2 \\ +3 & -3 \\ \vdots & \vdots \\ +n & -n \end{bmatrix} \tag{6.1}$$

where $\sim$ is the relation *invariant identity* (See §6.5 page 174) or *equivalence*, and n is any integer.

Just as 0 can be identified in an infinite number of ways using the inverse elements, similar set of identifications are possible for other numbers. In the case of number 9, the class of descriptions that would identify number 9 can be obtained, as above, either employing an additive operation or with a multiplicative operation. For generating the class of inverse-definite-descriptions for number 9 (as a member of integers) the generation process can be described as follows:

$$9 \sim [K + (9 - K)] \tag{6.2}$$

where $\sim$ is equivalence relation, and K is any integer. Thus for the integer 9 the following, for example, are the inverse-definite-descriptions:

$$9 \sim \begin{bmatrix} 10 & -1 \\ 5 & 4 \\ -247 & 256 \\ \vdots & \vdots \\ K & \bar{K} \end{bmatrix} \tag{6.3}$$

where K and $\bar{K}$ designate the inverses. Since the pairs 10,-1; 5,4; -247,256; $\cdots$ are not inverses with respect to the simple operation of $+$, but the special additive operation with 9 as its identity, it is appropriate to designate the operation differently. We will designate the above complex additive operation as $\oplus_9$. Using this notation the above equation becomes:

$$9 \sim [K \oplus_9 \bar{K}] \tag{6.4}$$

The following generation procedure produces a structure of a class of additive inverse-definite-descriptions for any given integer n,

$$n \sim [K \oplus_n \bar{K}] \tag{6.5}$$

where K is any integer. Similarly one can work out a structure of inverse-definite-descriptions for any number whether the number belongs to a set of integers, rationals, reals etc., by appropriate stipulations. The class of inverse-definite-descriptions generated for an object may be obtained by employing either an additive operation or multiplicative operation. For generating inverse-definite-descriptions for a specific real number r the procedure is as follows:

$$r \sim [R \otimes_r \bar{R}] \tag{6.6}$$

where R is any real number.[24]

What is the significance of viewing the matter in the manner suggested above? According to the view of science being developed here, one of the main pursuits of science is to discover inversely systematized structures. The nature of objects scientists deal with are in this view dependent on inverse systematization. The possibility of inverse-definite-descriptions presupposes structure-dependent or indirect knowledge of the object. Structure-independent knowledge or direct knowledge of things can not have inverse-definite-descriptions because inverse relation necessarily constructs a structure. Since we claim that all scientific knowledge is structure-dependent knowledge and the kind of structures we find in science are inverse structures, we claim that *to have scientific knowledge of an object is to have inverse-definite-descriptions of that object.* In this special kind of relational manner of identifying objects, i.e., by inverse-definite-descriptions, there are a family of descriptions that refer to an invariant identity. This, we claim, is a characteristic feature of the structure-dependent knowledge of science. We further claim that models, systems and definitions that we often come across in scientific discourse are examples of inversely systematized structures. In what follows we shall provide an interpretation of definitions, models and systems in terms of inverse-definite-descriptions. This interpretation would enable us to explain the role of inversion in the structure and generation of scientific knowledge in terms of inversion.

[24]It may be noted that the elements that form inverse-definite-descriptions may also be defined as a group structure. For example, the set of all elements that form inverse-definite-descriptions for 0 and 1 can also be constructed as groups under operations $+$ and $\times$ respectively. For numbers other than 0, it is possible to construct inverse-definite-descriptions using the general formula 6.6.

An identity element n can be defined with respect to an operation $\otimes_n$ and the set G has the elements

$$[\cdots n/4n, n/3n, n/2n, n/n, n, 2n, 3n, 4n, \cdots].$$

The structure $< G \otimes_n >$ can be shown to be a group. The set G contains nothing but the elements that form inverse-definite-descriptions.

The inverse-definite-descriptions given for the identity 9 in the thesis using an additive operation $\oplus_9$ is not a group. However it does not therefore cease to be a inverse-definite-description. It will not be possible to show for numbers other than 0 that the inverse-definite-descriptions will form a group using an additive operation. Group structure is desirable, but nor necessary, because it has interesting properties such as closure.

6.7 Inverse-Definite-Descriptions and Scientific Knowledge

We shall first provide an interpretation of scientific definitions. In modern science it is usual to express definitions of most concepts in mathematical form. Needless to say only quantitative notions can be expressed in mathematical form. In this section we will attempt an analysis of *functional* definitions of quantitative concepts employing the notion of inverse-definite-descriptions, introduced in the above section. In the course of this section the distinction between definitions, models and physical systems would become more clear and distinct. We shall take the example of the definition of momentum to illustrate our point.

The definition of momentum is given in terms of mass and velocity. The equation expressing the relationship between momentum P, mass M, and velocity V is given by

$$P = M \times V. \tag{6.7}$$

For any given value of momentum P_i there can be a class of inverse-definite-descriptions, because mass M and V of a particle are covariant when P is invariant. The following structure is a representation of the identity P_i in terms of inverse-definite-descriptions:

$$P_i \sim [M_x \otimes_P V_y], \tag{6.8}$$

where x is any value from the domain of values M can take, and y is any value from the domain of values V can take. Since these values are *magnitudes* they are not to be considered mere numbers. This is another reason why the operation here is not the usual multiplication $\times$, but a specific operation $\otimes_P$, relating the elements of the domains V and M in a one-to-one manner, and the elements related as one-to-one are inverses with respect to the operation.[25]

This description of P_i suggests that P_i can be obtained by various invariant proportions of velocity and mass of either different bodies or of the same body. When we look at each inverse pair of values as referring to different bodies, we don't get a very significant picture. However, if we consider that these various pairs of values of mass and velocity are the values that a single body takes as time progresses, we get a very significant and interesting situation. We get a *dynamic system* where the velocity of a body varies inversely to its mass by keeping its momentum invariant. Thus, the sequence of inverse-definite-descriptions, in this case, constructs a dynamic state-of-affairs. This state-of-affairs can be a *model* of a scientific assertion which states the invariance (or conservation) of momentum.

[25]Let us clarify here that the usual interpretation of a function in terms of one-to-one relation is not being disputed. We are making a further point by stating that an one-to-one relation be interpreted as an inverse relation.

The equation (6.8) therefore can be seen as representing the structure of a model. This is a model because this is the kind of possible world or state-of-affairs where invariance of momentum comes out entirely true. This is in accordance with the definition of a model given above (§5.2 page 128): *The circumstances in which a scientific assertion is entirely correct or true is called a model.* The equation (6.8) in the present formulation is ambiguous, because it may be seen as representing for two different state-of-affairs—for various particles with the same momentum and the same particle with different momentum. This can be avoided by imposing a relevant constraint.

The case of rocket propulsion is an actual *physical system* where the rocket takes on covarying values of mass and velocity, while the momentum of the system is invariant. As the rocket accelerates (i.e., takes on increasing values of velocity) the mass of the rocket takes decreasing values, but the momentum of the system remains conserved. This physical system is an instance of the model of the theory of conservation of momentum. While it is not very difficult to find an instance where mass is constantly decreasing, by increasing the velocity, the same model can account for a reversible phenomenon. That is, when the body constantly gains mass, the velocity should constantly be lost. We may not find any physical system where such a situation can be observed. But, such a situation is indeed a possible state-of-affairs. Most physical systems are therefore only *partial replicas* of a theoretically constructed model. While it is possible to theoretically construct many possible worlds, all of them may or may not be actually obtained. Here lies the potential and uniqueness of inverse reasoning as against inductive methods of obtaining knowledge.

Thus it is possible to construct a model given a functional definition of a measurable dimension using the method of constructing inverse-definite-descriptions. Here we have presupposed the availability of a definition. We have dealt with the construction of a definition by inverse reasoning in the case studies, where the problem of conceiving physical systems from a systematic study of the phenomena is also discussed in detail. The problem of quantification from a qualitative understanding of phenomena is more difficult than moving into the higher levels of mathematical abstraction once a functional relation is obtained. This observation can be substantiated from a number of instances from the history of science. The initial quantification of a problem often takes centuries, while further investigations based on the initial break-through goes normally at a relatively quick pace. Therefore, we have concentrated more on the role of inverse reason in the initial break-throughs in the case studies. This is not to suggest that the latter does not pose any serious problems, but only that the nature of the problem is different.

We shall proceed further to discuss the role of inversion in model construction. Given a functional definition, such as (6.7), we usually transpose the terms algebraically to get the following equations from (6.7) giving rise to the 'derived' definitions of mass and velocity:

$$M = P/V. \tag{6.9}$$

and

$$V = P/M. \tag{6.10}$$

Both mass and velocity have definitions independent of the structure such as (6.7) where they are functionally related to momentum. It is usually considered that the three equations say the same thing. This follows from our common understanding of functional definitions, according to which a mathematical equation or formula can be reduced to a consistent relation between *variables.* Consider for instance what Holton and Rolland say in this connection.

> Mathematical formulations impose several conditions upon the form and content of scientific work. For instance, those who were formerly prone to think of a postulate as a relation between cause and effect must instead come to regard it as a relation between variables. Thus in an expression of the type $X = YZ$, we can just as well write $Y = X/Z$, or $Z = X/Y$, and there is no way of telling whether X or Y or Z is a cause or an effect. In other words, it is on the whole more fruitful to think of an *interaction* rather than a simple *causation*, and to ask what factors is X *related* instead of what *causes* X.[26]

This passage is correct insofar as it describes the nature of transformation that took place historically, from causal analysis of terms to functional (mathematical) analysis of terms. Here the difference between the former interpretation of a postulate in terms of the cause-effect relation (causal correlation) and the latter interpretation where the variable terms are related by *covariance* are clearly well stated. But it is important to distinguish the level at which we can do the algebraic transpositions freely (without much constraint) from other levels. When scientists construct a model from the given definitions, they need to impose certain constraints on the values the variables in the definition can take, so that some definite conception be obtained. In the world of absolute variables no differentiation is possible, and since without differentiation no conception is possible, we say that in order to obtain an identity, a conception, it is necessary to regard certain terms invariant. However, by changing the constraints now on one term, and now on another, different models can be obtained. Since a model is a model only if it is a definite construction, we think that this methodological stipulation is necessary, i.e., at a level where scientists deal with a definite

[26] Holton and Roller 1958, *Foundations of Modern Physical Science*, p. 224.

model, they cannot regard certain terms in the equation as mere variables. In fact, we will argue that, they need to regard at least one term as *invariant. Nothing can be a model if every parameter in it is regarded as a free variable.*

Another way of viewing the matter is to state that a scientist is different from Parmenides or Anaximenes, who regarded the world as a Being that is undifferentiated. At a mathematical level we possibly behave like them, but when we begin to construct local paradigms, we cannot afford to remain in an undifferentiated realm. In this context, let us contrast our position with that of Meyerson's principle of identity as stated by Elie Zahar:

> According to Meyerson, all explanations, whether scientific or commonsensical, spring from one basic tendency of the human mind; namely the tendency to deny diversity and change; or to assert the existence of constants behind the fleeting appearances; or to explain the Many in terms of the One; or to subsume the flux of Becoming under the immutability of Being. The best formulation, in my opinion, is that the human mind inevitably tends to deny diversity and assert sameness or identity both in space and in time. For the human mind, only the undifferentiated One is real, everything else is appearance.[27]

It is correct to say that scientists have a tendency to look for invariance, but the nature of invariance they look for is different from the kind of Parmenidian invariance. Scientist's do accept a variety of invariant identities, each of them independent from the other. Though scientists create undifferentiated abstract structures, they are ultimately employed to construct specific models with local applicability. Metaphorically speaking, they do go up to get a heavenly view of things, but only to return with a divine image of things. It is therefore not correct to say that "only the undifferentiated One is real". A correct description of the scientists' activity would be to say that only *the extra-phenomenological differentiation is real.* What is extra-phenomenological is elaborated in §6.10 below.

The nature of the work of the scientist changes in the very act of stipulating the domain for a given term. By doing so he has already come a step nearer to specificity, by stepping out of the free 'world' of algebra. Of the three terms present in the equation, we can consider one of the terms as constant or invariant, allowing the other two terms to take on different values from a specified domain.[28] But how free are the other two variables after making one of them invariant? Since the other two terms are covariant, fixing one of the values will mean a lot of specificity, because sufficient definiteness will be introduced in the structure such that the obtained structure can now be descriptive of a state-of-affairs.

[27]This is as stated by Elie Zahar 1980, 'Einstein, Meyerson and the Role of Mathematics in Physical Discovery' *British Journal for the Philosophy of Science* **31** p. 9.

[28]For convenience we have taken only definitions involving three terms. Similar analysis can however be extended to those with terms more than three.

Since there are three variable terms in the above definition of momentum, we can generate three general statements by considering one of them a constant as follows: (a) When momentum is kept invariant, mass and velocity are covariant; (b) When mass is kept invariant, momentum and velocity are covariant; and (c) When velocity is kept invariant, momentum and mass are covariant. These three general relational statements obtained from the given functional definition *do not say the same thing.* That is to say they describe three distinct and independent states-of-affairs. It is not necessary that if one of the statements is true the other two also be true in the same world. However, whether actually true or not a theoretician can construct a possible world where they all come out true.

In what sense are these three statements independent? We usually consider anything a statement if it is an assertion about a state-of-affairs. Two statements therefore can be considered independent statements if what they assert is about different states-of-affairs. The three statements (a), (b) and (c) are independent statements in this sense. Since the statement (a) is true in the situation described by the model represented by the class of inverse-definite-descriptions for momentum as specified in the equation (6.8), we shall consider here the generation of a state-of-affairs where the other two statements come out exactly true.

Consider that for a mass M_i the following class of inverse-definite-descriptions is given:

$$M_i \sim [P_x \otimes_M V_y]. \tag{6.11}$$

If each pair is considered a unique description of different masses then the model describes a possible world with several bodies, all of them with same mass, but in different states of motion. This model is descriptive of a system with discrete entities. A more significant model obtains when each pair is considered to be referring to the same particle at different times. A body of mass M_i thrown vertically up-wards and then its free-fall is an example of this model where in both cases of upward and downward motion the covarying pairs describe the motion of the body in a unique manner. Though we have obtained this class of inverse-definite-descriptions (6.11) from the same functional definition, when the constraint is on the mass term of the structure (definition), the obtained model—also a structure—is distinctly different.

Similarly we can consider this time the class of inverse-definite-descriptions for a given magnitude of velocity V_i:

$$V_i \sim [P_x \otimes_V M_y]. \tag{6.12}$$

This could be descriptive of a discrete state-of-affairs where all bodies with different masses and momenta moving with the same velocity, whether at the same time or at different times.

In a different construction of a model where the values are given to the same body, the situation is rather difficult to conceive. What can be an application of such a model? Do we know of any particle or body that shows variation of mass and momentum over time in such a manner that the velocity is kept invariant? We can possibly construct a space vehicle with an internal device (say a builtin rocket) to control the momentum in such a manner that it produces a constant velocity. Since the velocity is constant the state of motion is actually an inertial state, but of a unique kind. Whether we obtain a system of this kind in this world or not, the constructed model has a definite meaning and can remain a pure construction till we can find an instance. Since inverse-definite-descriptions can construct models with or without immediate application, the methodology is significantly ampliative. Unlike induction, where no conception can emerge without application, in the case of knowledge constructible on the basis of inversion, concepts can be conceived much ahead of discovering any application. The method of inversion therefore is a theoreticians' tool of constructing possible worlds. These examples are very simple. But they inform us of the constructing potential of the inverse relation.

If from a single functional definition so many models—all of them descriptive of different state-of-affairs—can be constructed, then it is needless to say that with so many functional definitions that scientists have in hand, a manifold of models can be generated. Some of the constructions thus obtained may only have projected (potential) application. Since all these models are structures and can have potentially many applications, we will regard them as *complex-predicates* or *complex unsaturated propositions.* As already mentioned the role of inversion in obtaining functional definitions will be taken up in the case-studies.

Another point to take note of is the significance of several inverse-pairs. Any one inverse-definite-description out of the class of them can, logically speaking, designate the invariant identity. But that is not a sufficient condition for either verifying or falsifying most scientific assertions which are assertions stating proportionalities. In order that a statement asserting proportionality be verified or falsified, it is necessary to consider many cases because the relation asserted is not a relation between constants but about an *invariable covariant relation* between variables. To designate, for example, the *state* of a phenomenon all parameters cannot remain constant. Nothing is a *state* if all descriptive parameters of it are constant. Therefore, P_i will be a scientifically significant identity only if it can have *more than one inverse-description.*

Therefore for statements asserting proportionality such as mentioned above, it is required to have a class of inverse pairs in order that it be a significant scientific statement.

This should be given special significance because the essential nature of scientific assertions as well as scientific objects (models and physical systems) emerges out of this. Since most scientific assertions are assertions of proportionalities, the conditions under which they become true are conditions in which a class of inverse-definite-descriptions can be obtained.

It follows from the above stipulations that the case where all the three terms of an equation are constants, cannot in isolation become either a falsifying or verifying case of a significant scientific assertion.[29]

Consequently we suggest the following interpretation of universal constants. Take for example the case of the universal constant, such as the speed of light. The constancy of the speed of light is the invariant relation or the nature of covariance between mass and energy, as stated in the famous equation: $E = mc^2$. Universal constants become very significant constraints in the construction of models because no possible world can ever be constructed where the value of the constants, if relevant in that construction, can vary. Though momentum, mass, energy etc., are conserved quantities, they however continue to be variables. This difference between the constancy of universal constants and the conserved quantities can be stated to be between a global and local *constraints on possible world construction*, where the former is global and the latter local.

The invariance of primitive notions such as velocity, acceleration, etc., is more local than that of momentum, energy, etc., in the sense that a world where momentum is invariant is a world where velocity, acceleration etc., become variable or locally invariant. Likewise, in a world where energy is postulated to be invariant, momentum becomes a variable or only locally invariant. In a world, such as ours, where the speed of light is postulated to be *constant* the invariance of mass and energy also become local. Universal constants under this interpretation become the unique identities of the postulated world.

These relations between local and global invariances indeed suggest a pattern for reconstructing the development of scientific knowledge. Since all identities, whether of a local or global kind, can be understood as identity elements of the respective inverse 'dimensions', as suggested above, we propose the possibility of a generativist methodological reconstruction of scientific ideas. Though we are not attempting any such reconstruction here, we do not hesitate to envisage the possibility of such a philosophical endeavor. Needless to say, in this

[29] For a mathematician an equation such as Euler's ($e^{i\pi} + 1 = 0$) is as good as a fact of the mathematical world, and therefore can be considered as a scientific statement of special significance. The significance of this equation however cannot be immediately grasped. Insofar as it is a truth of the pure mathematical world, its significance, whatever that be, is limited to that domain of inquiry. If it so turns out that the mathematical world has lot to do with the physical world, such 'truths' might be considered the ultimate constraints on possible world construction.

visualized possibility it would be inversion that would play the crucial role in systematizing knowledge.

6.8 Multiplicity of Operations

It has been suggested above that the operations involved in the inverse-definite-descriptions, though broadly classifiable as either additive or multiplicative, need to be regarded as distinct operations. The ampliative nature emerges out of the *multiplicity*—in the sense of being many—of the operations. In this section we will provide further justification for such a *multi-operations-view.* One part of the argument that is already suggested above is that each of the operations generate or construct different structures representing different state-of-affairs.

When the functional relation specified in the definitions is intended to be a generalization of relationship between the dimensions, how could one think of several operations making up *one* relation? One might say that this appears quite counterintuitive as well as unnecessary. In order to make our position appear plausible we will introduce a distinction between ampliative and non-ampliative operations.

Consider the case of operation on numbers. Given an equation $x \times y = z$, the operation $\times$ is generally considered to take any two numbers, x and y, from a given set S, and yield a number z belonging to the same set S. It would not be considered a 'well-behaved' operation if it yields a z not belonging to the set S. But what about the cases when we consider adding or multiplying things other than numbers?

Could we *multiply* 10 apples with 20 pebbles? Do we get 200 of 'appebles'? How about *adding* 10 apples with 20 pebbles? We get an aggregate of 30 objects that includes both apples and pebbles. Since we do obtain an aggregate of 30 countable objects, the operation makes sense. But there seem to be no meaningful product obtainable from multiplying apples with pebbles. There seems to be some cases where some operations do not make sense. *That is some operations do not generate any meaning, while others do.*

Let us take another case. When a line of length 20 meters is *added* to another of 10 meters, we get a line of length 30 meters. What we get can still be called a line. What happens when we *multiply* the two lines? Do we get a line of 200 meters? It depends on the context. If we are measuring the length of some thing, such as a road, with a tape of length 10 meters, having performed the operation 20 times we compute the length of the road as 200 meters. However, in this case while the 10 stands for the length, the 20 does not stand for the length, but for the number of times an operation is performed. Whether we measure with a tape of 20 meters 10 times, or vice versa we get the same length, and so the operation of multiplication has some sense in this case.

In another situation, we might multiply apparently the same kind of values, but we would report that the result is 200 *square meters.* Here also it makes sense because we are measuring the area of a surface. Are we performing the same operation in both the cases? Definitely not. In the former case, though we are using the multiplication table, actually what we are doing is nothing but addition, as many times as the operation is repeated. This is a trivial application of multiplication. In the latter case, however, we are engaged in a *non-trivial* operation. The result is no longer a length, but area. We will call such non-trivial operations *ampliative operations.* Insofar as 10 and 20 are numbers, and 200 also a number, our operation continues to be of an arithmetical kind. But when the numbers represent magnitudes of certain dimensions, they cease to be mere numbers. One might suppose that this is in no sense a startling fact that needs to be talked about in a doctoral dissertation. The non-trivial matter is the epistemological significance of the *amplification* involved in the process. When we start talking about area as a function of the sides of a rectangle, we have not made any inductive generalization, rather we have constructed (defined) a concept. This concept of area can also be identified in terms of inverse-definite-descriptions.[30]

In certain situations we may have to follow a different procedure of addition. Specially when we are operating with a vector magnitude, such as displacement. Here the direction of displacement along with the magnitude matter. Thus vector addition is another operation that follows a different, but definite logic.

Though it is possible in a higher order theory to have a very general definition for obtaining the areas of any shape, the manner in which we make sense of that higher order operations follows a definite path of generative history. A large number of differentiable operations, each with a specifiable and non-trivial meaning have entered into that process of obtaining a general theory of space. That the general theory is not just a single concept becomes clearer to us when we begin applying the 'global' formula to generate 'local' formulae. Possession of a general formula is not a sufficient condition for the mathematician's ability to solve a specific problem. His abilities depend on how good he is in generating the specific formula for the specific purpose. Generalizability of a large class of operations conceals the fact that insofar as they are general such theories remain 'deaf-and-dumb'. The context of learning as well as the context of application can demonstrate the amplifiability of such

[30]Piaget's investigations on the development of concepts such as 'length' 'area' 'volume' etc., as disussed in *The Child's Conception of Geometry* 1960, demonstrate that the nature of the operations involved in learning each of the above mentioned concepts are independently closed under distinct group of operations. Invertibility (reversibility) of the operations is one of the necessary conditions of acquiring these concepts.

general theories.

We therefore think that there are as many operations as there are different contexts of application. Every operation cannot be thought of being applicable to every situation *promiscuously.*[31] In this connection Karl Popper's views are supportive of our standpoint. He is of the opinion that the arithmetic of natural or real numbers is helpful in describing certain *kinds of facts, but not other kinds.*

> [W]e may note that the calculus of natural numbers is used in order to count billiard balls, or pennies, or crocodiles, while the calculus of real numbers provides a framework for measurement of continuous magnitudes such as geometrical distances or velocities. ... We should not say that we have, for instance, 3.6, or π, crocodiles in our zoo. In order to count crocodiles, we make use of the calculus of natural numbers. But in order to determine the latitude of our zoo, or its distance from Greenwich, we may have to make use of π. The belief that any one of the calculi of arithmetic is applicable to any reality ... is therefore hardly tenable.[32]

He is arguing there that each calculus should be viewed as a distinct semantical system. Since each calculus presupposes certain kind of operation/s, we may say that each semantical system differs from the other on the basis of what kind of operations are constitutive of the systems.

Popper argues that a proposition such as '$2 + 2 = 4$' can be thought of in two different senses. One of them is to consider it as a logical truth. In the second sense it may be taken to mean a physical manipulation (operation), such as, say, counting apples or pebbles. In the second sense the universality of the proposition becomes doubtful. Popper's examples are interesting and very useful, for they bring home several points that we are presently pursuing.

> [I]f you wonder what a world would look like in which '$2+2 = 4$' is not applicable, it is easy to satisfy your curiosity. A Couple of rabbits of different sexes or a few drops of water may serve as a model for such a world. If you answer that these examples are not fair because something has happened to the rabbits and to the drops, and because the equation '$2+2 = 4$ only applies to objects to which nothing happens, then my answer is that, if you interpret it in this way, then it does not hold for 'reality' (for in 'reality' something happens all the time) but only for an abstract world of distinct objects in which nothing happens.[33]

Two contexts become demarcated here. One is a context where nothing *happens,* and the other is a context where *something happens.* We think that the 'operation' that happens

[31] This thesis can be viewed as opposing Quine's famous thesis on natural kinds.

[32] Karl Popper 1962, *Conjectures and Refutations*, p. 211.

[33] *Ibid,* p. 212. Note that here the term 'model' is used to refer to an example of a world (possible). We would prefer 'model' for the possible world, and 'system' for the actual world where we find the model instantiated.

when rabbits or drops meet cannot be captured by the 'poor' sense of the operations supplied by arithmetic. Arithmetical operations do not have generative potential in the non-trivial sense. In order to bring out the qualitative differences between various operations in various contexts, we need to differentiate each such operation by a proper methodological procedure. We think that the above method of constructing inverse-definite-descriptions could bring it out. Since every operation has an identity, which can be uniquely described by the inverse-pairs, we could talk about the semantic differences that exist in the various operations that are possible in the 'rich reality'.

Another very crucial point is with regard to what Popper says about the transformation of semantical systems into scientific theories. He says that

> in so far as a calculus is applied to reality, it loses the character of a *logical* calculus and becomes a descriptive theory, *which may be empirically refutable;* and in so far as it is treated as irrefutable, i.e. as a system of *logically true* formulae, rather than a descriptive scientific theory, it is not applied to reality.[34]

This passage serves us a double purpose. First, it brings out the point that a semantical system gets transformed into a descriptive refutable and empirical scientific theory when applied to reality. Since a semantical system is irrefutable, it is neither true nor false—it is immune to refutations. Second, it clarifies Popper's notion of scientific theory: it is an *application of a semantical system to reality.* For him nothing is a scientific theory if it has no potential application. For us nothing is a scientific *concept* if it has no potential application and nothing is a scientific assertion if is not a statement applying a scientific concept. The nature of the difference is that Popper demarcates statements into scientific or not on the basis of falsifiability, while we are suggesting a demarcation of concepts into scientific or not on the basis of whether a concept specifies an invariant identity expressible in terms of inverse-definite-descriptions or not. In consequence we would pass semantic systems as legitimate objects of scientific knowledge because it is sufficient for them to have potential application. Growth in the number of distinguishable semantic systems is already a partial growth in scientific knowledge.

Lorentz is indeed a scientist, for he did construct a meaningful semantic system. Einstein is also a scientist, for he found a truthful application for the meaningful semantic system that Lorentz invented. Should we say, Einstein *discovered* and Lorentz *invented*? Sometimes it is less confusing if we do differentiate the activities of different scientists by different terms. We would say that both activities have epistemological relevance, unlike

[34] *Ibid*, p. 210.

Popper, who found greater relevance in Einstein's activity. Since the construction of semantic systems are based on a knowledge of proportionalities that are available from either direct or experimental knowledge, empirical constraints necessarily enter in the process of generating models. Thus though there is an element of theoretical (mental) construction in the generation of semantic systems, it is constrained by relations that obtain in 'reality'. Therefore, none of the structures thus formed can be devoid of empirical content, and in most cases unanticipated counter-inductive knowledge gets generated. Einstein's relativity theory is a good example of a non-inductive, inversion based construction, which surprised many due to its distance from ordinary understanding.

6.9 Inversion and Symmetry

Symmetry is one of the most sought after features in the pursuit of knowledge, even more so in modern science. The notions of symmetry and truth have come closer than ever in recent days as the emphasis on constructive thinking is increasing in all the departments of science. The situations where we seek explanations turn out to be those that are asymmetrical in relation to a certain structure, though this realization is mostly retrospectively obtained. In the pursuit of finding explanations, we tend to posit (create) certain relevant inverses which bring in the sought after closure. Such a closure appears almost always to be that which fulfills symmetry. Once symmetry is found we seem to be reaching a climax or culmination, and we often stop further probe—at least temporarily—for along with it arrives a sense of fulfillment and achievement. An immediate and important question comes to mind as soon as we make the statement that the objective of scientific endeavor is symmetry: What is the relation between truth and symmetry? Since, symmetry is not truth, and since truth is largely believed to be the objective of scientific knowledge, the relation between these variously stated objectives needs to be understood. This however remains a riddle, and—as van Fraassen states—this is the epistemological question *par excellence* insofar as scientific knowledge is concerned. We are not going to pursue further this question in the present thesis, for that would take us far afield. Presupposing that scientists are indeed involved in the pursuit of symmetry, we will discuss the problem of generating symmetrical structures in what follows.

Symmetry is usually defined as an operation, or an action or a transformation of a structure that does not alter the value of a certain measurable parameter of the structure.[35] *The family of transformations* that do not alter one of the parameters *is called a group.* A

[35] Feynman 1965, *The Character of Scientific Law,* p. 84-85.

group structure is necessarily symmetrical, and is defined in terms of closure, associativity, identity, and inverse elements of an operation (See §A).

Bas van Fraassen perceives the sort of theories and models that scientists construct as a result of the pursuit of symmetry.[36] Most arguments in the context of theorizing, according to him, are *a priori* symmetry arguments. If theorizing is based on *a priori* arguments then the question would naturally arise; what role remains for empirical means in the context of theorizing? Van Fraassen thinks that the *a priori* appearance must be deceptive because nothing contingent can be deduced by logic alone. However, he thinks that there is an "argumentative technique" based on two forms of "meta-principles": (a) "structurally similar problems must receive correspondingly similar solutions"; and (b) "an asymmetry can only come from a preceding asymmetry".[37] Van Fraassen's complete statement of the symmetry argument is as follows:

> In a symmetry argument one proceeds as follows: a problem is stated, and we first endeavor *to isolate the essential features of the problem situation—that is, the features relevant to the solution.* **This does not necessarily call for great insight**: the precise statement of the problem generally specifies what is intended. ... Isolating the essential or relevant structure is equivalent to defining the set of transformations that leave the problem essentially the same. These transformations are the *symmetries* of the problem. Only with these at least implicitly specified can we insist: *problems which are essentially the same must have essentially the same solution.* This is the great Symmetry Requirement, the principle of methodology that generates symmetry arguments. To put it somewhat differently: once the relevant parameters are isolated, the solution must consist in a rule (i.e. function) that depends on those parameters alone.[38]

The dependency of a rule on a parameter means that if the problem is transformed by one of the admissible transformations, i.e. by a symmetry, and the rule is applied to the result, then if the rule gives the same answer as when applied to the original, the rule can be said to be dependent on the chosen parameters.[39] These specifications are sufficiently clear for understanding the possible role of symmetry argument in *appraising the solution of a problem.* We however think that the crux of the matter of discovery consists in the *isolation of the relevant parameters*. Van Fraassen thinks that this "does not necessarily call for great insight" because "the precise statement of the problem generally specifies what is intended." Isn't this question begging? If this were the case, the crux of the matter consists in the proper

[36] *Op.cit.* p. 232.
[37] *Ibid* p. 233. Compare it with what Herman Weyl says more or less the same matter in the traditional language of cause and effect: "If the conditions which uniquely determine their effect possess certain symmetries, then the effect will exhibit the same symmetry." H. Weyl 1952, *Symmetry*, p. 125.
[38] *Ibid,* pp. 258–259. Boldface is ours.
[39] *Ibid,* p. 259.

formulation of the problem. Is it then the case that symmetry enters into the scene only after the formulation of the problem? Has it no role in the very formulation of the problem? If so van Fraassen's proposed role of symmetry in the context of solving a problem becomes a second level proof of the *invariance* of the differently obtainable structures that are already available to begin with.

If we are correct in our understanding of his proposal, then the conceived role of symmetry here can be stated to be in the context of appraising by proving the equivalence of the already available structures that were obtained by different means rather than in the original construction of symmetric structure. We are not stating that this second step is not essential. Indeed it becomes one of the conditions of acceptability. This certainly forms part of the process that provides epistemological warrant. But, how much is its relevance in the context of generation of the original idea (structure)? The crux of the matter, with respect to the generation of the original idea, does not appear to be like a case of finding equivalence of different approaches of solving a problem. Note that both the meta-principles (a) and (b), stated above, work by realizing the differences or similarities among structures that are somehow given. If this is the proper understanding of what is intended in van Fraassen's suggestion, the *fundamental question* remains unanswered, which is to know *what makes us initially conceive the structure, i.e. identify the relevant parameters in a given problem situation such that its invariance with other structures can later be worked out.* This forms the first level of the problem of discovery. In other words, as stated in the previous chapter, this is nothing but the problem of the original conception of *physical systems*, while van Fraassen's proposal seems to properly belong to the next level of constructing theories, which is from *physical systems to models.* We claim therefore that van Fraassen's proposal takes care of the second level of the problem. However the applicability of the symmetry argument in this level appears to be a consequence of the *inverse reasoning* that plays its role in the first level of the problem. Our thesis proposal then can be understood as furthering the approach well conceived by van Fraassen, and Hermann Weyl, Wigner etc. We will claim that *since inversion is a logical condition of symmetry, inverse reasoning makes construction of not only symmetrical models, but also physical systems possible.*

Symmetry is undoubtedly a property of structures, though not all structures are symmetrical. Some structures that are symmetrical are so because they have in them a symmetry making relation. *A structure is symmetrical iff the relation that makes the structure is a symmetrical relation.* Structures that are not symmetrical are so because they do not contain a relation that is symmetrical. All these statements might appear trivial, but their significance consists in delimiting our search for symmetry making properties. If the problem is to discover symmetry, then we should look for the factors responsible for symmetry, which in turn will be responsible for the formation of structures such as scientific models. Our natural choice is inversion. We postulate inversion as the basal relation that yields all the symmetrical structures, such as physical systems, functional definitions, models, etc.

What van Fraassen said about the need of the symmetry requirement is a better way of putting what is well known in terms of obtaining relevant initial conditions. According to Wigner, scientific knowledge of anything is possible only if the possibility of isolating initial conditions in *many* cases of the phenomena, and also the possibility of obtaining the *same* essential initial conditions, no matter where and when we realize these.[40] Thus obtaining initial conditions in *many* cases and realizing their *equivalence* or "sameness" is the condition of proper scientific knowledge.

The often cited example is that of mechanics, where the position and the momentum of a particle become the initial conditions, but it is never necessary to have the absolute position and the absolute time as initial conditions for the scientific study of the dynamics of the particle. Scientific knowledge therefore has its roots in the method of relativizing. In other words we may say that science begins only when relativized parameters of a phenomena could be obtained. Though this might look as a *limitation* of scientific knowledge—for we can always make only relativized judgements, it must however be seen as a break-through or wayout of the fact that it is impossible to know *absolutely* about any object that is *becoming.* Relativizing is therefore the heart of the matter of science. And obtaining invariance is same thing as obtaining a symmetry. And obtaining a symmetry is equivalent to obtaining a principle of conservation. The connection between the principles of conservation and symmetries in nature have been demonstrated by Emmy Noether. Noether's famous theorem can be stated as follows: where ever there is a symmetry in nature, there is also a principle of conservation, and vice versa. Thus the interconnection between these notions is well established, and need no further argument.[41] However, understanding the fundamental significance of these 'values' and the factors that make them possible requires more investigation. In this regard we will discuss in the next section the relation between relativity and objectivity.

[40]Wigner, E. *Symmetry and Reflections,* pp. 3-4.

[41]Cf. Wigner's *Symmetry and Reflections*, Feynman *The Character of the Physical Law*, Herman Weyl's *Symmetry*, and van Fraassen's *Laws and Symmetry* specially Ch.11, contain a clear and informal expression of the equivalences stated above.

6.10 Relativity, Measurement and Inversion

In common usage the term 'relativity' is used to express: "Well, it depends on how we look at it". Therefore hearing the term 'relativity' invokes a kind of uncertainty among common people. But in science obtaining a relativity principle actually means that a scientist has discovered a means of saying confidently: "It hardly matters whichever way we look at it." Discovering such a description is the goal of scientific activity. Obtaining a *relativity principle* is same thing as obtaining an *invariant* manner of description. Thus the 'common man's' usage of the term 'relativity' is clearly different from the scientist's use of the term.

It is also necessary to note that the absence of an absolute description does not mean lack of objectivity. The opposite of 'relative' is after all 'absolute', and not 'objective'. Learned readers might get impatient of these simple clarifications. But unfortunately, it is usual to confuse relativity with subjectivity not only among common people, but also among the learned. There is no harm repeating it here. However there exists a nontrivial matter that demands attention from philosophers of science, which is to unravel the *connection between relativity and objectivity.* Let us elaborate the problem.

When scientists say "It hardly matters which way we look at it." they mean "whichever way we measure it". So *measurement is the means of a scientist's seeing.* In fact it is only in the nature of measurement the connection between relativity and objectivity can be obtained. The beginning of measurement is an *arbitrary* choice of a *standard* or *scale,* or a 'yardstick', but it is necessary that the standard of measurement be external to the subject. In other words it is *extra-phenomenological.* It is in this arbitrary beginning as well as in the external standard that the essence of the matter lies.

In order to see that measurement is *extra-phenomenological* or *extra-subjective* it is necessary to first take up the problem: *What makes measurement possible?* In the following account our attempt will be to locate the moment where inversion enters in a significant and necessary manner in the making of scientific knowledge.

There is a sense in which we can say that each sense organ can be seen as an 'instrument' of classifying things around in the world of experience. Let us also presuppose that the use of classification is the same as conceptualization. Through the sensory 'instrument' of the eye we can classify things on the basis of their color, brightness etc. Similarly auditory, olfactory, gustatory, and tactile organs can do the same on the basis of their respective capacities. If we suppose that each organ does it on the basis of one perceivable aspect of the things around, is it possible to obtain knowledge on the basis of these instruments? If each of them *independently* classifies the things around, then the answer is *no.* Nothing can be identified unless two or more independent aspects of an object are linked. In the language of classes, this amounts to saying that it is necessary to obtain an intersection of classes to identify one or more objects as belonging to some kind. This also will be presupposed as a fairly well known condition of identification. Obtaining an intersection of the classes here means obtaining a synthesis or correlation between the outcome of different sensory 'instruments'. Since the correlation is understood to be made by another higher level 'instrument' (call it mind or subject), the knowledge thus obtained is *subjective.*

Here enters Hume, who posed the well known problem of *induction.* His interpretation of the synthesis or correlations, which we supposed as the necessary conditions of knowledge, is that it is a *psychological process of constant conjunction.* Since induction—the popular name for this process—can not provide any logical certainty, scientific knowledge—which was held to be based on inductive generalizations by his predecessors (Bacon, Newton, Locke etc.)—was gradually seen to be essentially non-inductive, without however totally denying the empirical nature of natural science. To the extent that it is empirical it is regarded also as psychological or subjective, as if empirical basis means the same thing as psychological or subjective basis. Karl Popper's denial of an epistemology of the sources of knowledge is based more or less on this identification: inductive = psychological = subjective. We think that this original attempt to base science entirely upon the inductive method was a mistaken approach.

Fortunately induction is not the sole means of obtaining empirical knowledge. There exists a nonpsychological, extra-subjective method of knowing the world around. Before we elaborate it, let us make another useful observation.

Since in the above mentioned manner of making knowledge the standard of similarity is *internal* or *private* to the subject, another problem of rooting knowledge in the inner faculties of a subject emerges. Here enters Wittgenstein's private language argument—another serious threat to empiricism based on sense impressions. Wittgenstein's way-out is to base knowledge on *publicly available standards.* He states in that connection that nothing is a criterion if it is not external—an attempt to link the very idea of criterion with its being external to the subject.

Scientific knowledge is based on the very possibility of external (extra-subjective) and inter-subjective standards or criteria. Scientific practice has found ways of transcending the limits of subjectivity by rooting the practice in a method which *transforms arbitrariness into invariance.* The very idea behind the scientist's tendency to measure things can be interpreted as a tendency to transform arbitrariness into invariance. To measure then is to transform arbitrariness into invariance. We propose that measurement, in this sense, is impossible if it is based on any subjective standard. We present the following case as an argument to support this.

Consider that we have an ordered class of things on the basis of relative hardness. While obtaining such an order, let us suppose that we have employed only our subjective standards of evaluation, such as pressing, scratching, hitting etc. By employing in the process nails, knives, diamonds etc., we can get a fineness in the gradation of things. Though we are using tools in the process of this ordering we have not so far made any measurement in the strict sense of the term. On the basis of such an order we can say that this is harder than the other, or conversely, the other thing is less hard than this. Since the same order is also the order of softness, though in the inverse order, we can make equivalent descriptions employing the order of softness. This inverse order between the opposites is very trivial in relation to the nontrivial inverse order that we will shortly state below.

Whatever be the fineness of the order obtained, we are still making qualitative descriptions. One might say that since we have a linear order of things which can be mapped to numbers, we can go quantitative by talking in terms of numbers. Merely using numbers does not make our judgements quantitative, again in the strict sense of the term. This use of numbers is trivial. And we will see below that nontrivial use of numbers enters only after the externalization of standards of measurement, and making use of numbers to express the measure of proportionality.

Just as we could obtain the above kind of order of things on the basis of hardness/softness, it is possible to obtain other orders of things on the basis of big/small (size or volume), heavy/light (weight), etc. Here again while obtaining the relative grading nothing more than qualitative order can be obtained. Though the next level of ordering continues to be qualitative, we can work out correlations between one order and another. For example, a direct correlation between hardness and weight: the greater the weight, the greater the hardness or vice versa. In terms of volume and weight: the greater the volume the greater the weight. However soon we realize that these *inductive* generalizations are not precise enough. They can be protected by introducing precision as follows: The greater the volume of a specific kind of substance, the greater will be the weight of that substance. This specification of the identity of the *third* element is very important whenever we make relative judgements. Without this specification no relational statement can be said to be either true or false. In other words, the specification of both subject and predicate is essential to make an assertion.

More importantly, this specification helps us to understand things in a much better manner. This happens when we realize that the same volume of different things do not possess the same weight. Let us remind the reader that the notion of 'sameness' that we are talking about is still a subjective judgement. A hard thing of the same weight will possess less volume than a soft thing of the same weight. We have a better way of stating this condition: *the harder the substance lesser the volume, if the weight of the substance is kept constant.* This inverse order between hardness and volume unlike the above mentioned inverse order is not a trivial one. It is nontrivial because the opposition stated here is not logically obtainable, but can only be obtained empirically and experimentally. Experimentation is involved because these statements are realized by controlling (keeping something constant) one of the aspects under study. All this is possible in a qualitative study of the matter.

What is lacking so far is the *externalizing* the standards of measurement. Though everyone of us are capable of making relative judgements of whatever significance, serious problems of inexactitude remain if we do not have extra-subjective standards. What appears hard to an academician's hand, may not appear hard to the tough hand of a worker in a farm, though to a large extent both of them would agree on the relative judgements. The strength of relative judgements, as against absolute judgements, lies precisely here. One may disagree that something is hard, but it is difficult to disagree, unless on factual grounds, when some one makes the statement "This is harder than the other". Though relative judgements are better than the absolute ones, in order that the judgements become acceptable to manifold of situations, we need to transform the relative judgements into *proportional quantitative judgements*.

Unless we embark on an arbitrary scale or yardstick or unit or standard that is *external* and not *internal* to the subject, making an invariant proportional quantitative judgement is *impossible.* What do we mean by 'external' and 'internal'? We mean by 'internal' any object that is accessible only to the subject.[42] Take for example the possibility of comparing the heaviness of two objects by keeping one each in each of the hands—hands becoming the pans of the subjective balance. The heaviness/lightness of the object is an internal experience based on the 'shift' produced by an 'internal indicator'. Though all of us may have the capability to have similar experience, for whatever biological reasons, nothing more than a qualitative judgement is possible however good be the resolving power of this internal 'in-

[42] We are aware of the philosophical problems of asserting in definite terms whether any sensations or thoughts have privileged access only to the subject, specially Wittgenstein's private language argument, which denies the possibility of such access. Nothing can be regarded a thought or a sensation if it does not have an identity of its own. It is therefore possible to defend a position that there cannot be any thoughts which are necessarily private, and all thoughts are *potentially public.* Since we can't engage in the details of this argument we will presuppose the possibility of private, but potentially public thoughts and sensations.

strument'. On the other hand when an external instrument that shows differential functional behavior to a given input, the standard of the judgement is publicly accessible.

One might say that even in this case the indicator has to be observed by the subject, and therefore ultimately it is the judgement based on our internal instrument. The power of an external standard of measurement lies precisely in making the vagaries of the internal experience irrelevant to the final judgement. Consider that a person has a curved impression of all straight things. Will his observations be invalid for making measurements? Certainly not. Because when he measures he measures proportions which are invariant of subjective impressions. All that we need for comparison is consistency in experiencing impressions. Though all of us actually have inverted images of the outside objects on our retina we make no mistake in judging our images, for in a relational perception what is important is consistency in difference. Even if one person among us is unlike the rest of us in having upright images on his retina, neither would he have any problem in his vision nor would we have any problem with him, because it is impossible for any of us to even detect this. All that is needed is consistent differentiating behavior. In a relational judgement the notion of identity that is involved is Leibniz's notion of *identity of indiscernibles.*

The act of measurement is a relational observation with a difference. The difference, as already mentioned, is that unlike personalized relational statements the relational statements of measurement are based on an external standard or yardstick. The nature of the difference is that the object measured as well as the criterion of measurement are *outside,* whereas in a personalized relational statement, the object may be outside, but the yardstick is inside. The differential functional behavior that our sense organs can exhibit, however sensitive they may be, can never break this subjective limitation.[43] The only way-out is to embark on the differential functional behavior of the measuring instruments whose 'mind' is transparent to all of us.

An analogy with language may not be out of place. The need of an *external arbitrary symbols* in language based communication play the same role in making such an act possible. In order that communication between different subjects is possible it is necessary to have an external symbol. Just as language helps us to break the circle of responding to the external world in an internalized manner, the act of measurement with external instruments help us to break the circle of internal relativized judgements. Just as the symbol and the object for which it stands are both outside the person, the yardstick and the object to be measured are both

[43]Though we are not going to pursue this here, we are arguing against gradualism of Quine, who argues that scientific knowledge and commonsense knowledge are different only in terms of degree. We are suggesting against his thesis that from common notion of kinds to natural kinds (or scientific kinds) there is a qualitative break, and so the difference is not merely a matter of degree. Cf. Quine's "Natural Kinds".

outside the person. We conclude therefore that external standard is a necessary condition of quantitative measurement—invariant proportional relation often stated as a ratio.

Another necessary condition of the measuring process is the presence of a *counteracting* or *inverting* 'ability' within the instrument used for measurement. Since the components of a measuring instrument must have some functional relation with the parameters that are to be measured, the question arises, what kind of functional relationship is necessary? We claim that without the involvement of an inverse relationship no measurement is possible.

Let us illustrate this in the case of measuring weights using a spring balance. Why do we need a spring balance for measuring weight? Our direct answer is that because the tension in built in the spring *opposes* the weight of the body, and that it does it according to a functional relation. The stretchability of the spring is *inversely* related to the tension of the spring: the greater the tension the lesser the stretching given a constant weight, and the greater the weight the greater the stretching given the tension is constant. Thus the extent of stretching becomes an index of the weight of the body. Of course, uniform calibration of the instruments is necessary for making something a standard of measurement.

Is it not possible to measure a quantity if it has only a direct proportionality with another quantity? Isn't it the case that the temperature of a body is directly proportional to the expansion or increase of the mercury column? Is there any counter expansion force in mercury? The cohesion of the 'particles' of the liquid metal happens to be such a force. The importance of the opposing 'force' can be made clearer by simply pointing out the fact that the spring balance requires to be hooked on to some static thing such as a stand. It is impossible to measure if the spring balance also moves with the weight. The only reason we need to fix it is to allow the counter force act which is in built in the spring. In the case of a thermometer it is required to contain the liquid metal inside a closed container for the same reason.

In the case of a proper balance used for measuring mass, the counter forces are visible, and does not require any interpretation. We will see in detail in the case studies how Galileo solved the problem of motion by constructing the counter balancing forces by inverse reason. We conclude therefore by stating that it is necessary for every measuring instrument to have an in built counterbalancing 'ability'.

We will now extend this condition of counterbalancing as a necessary condition of the very constitution of a sense organ, and then further explicate the point made above that measurement involves externalizing the standard of measurement. We are not going to dwell in detail on the physiology or biology of perception. We will confine ourselves in explicating the necessary structure that both a sense organ and a measuring instrument must have.

We shall first explicate the *functional structure* of the spring balance as a model of a measuring instrument. Let us call the unstretched condition of the spring the *normal* state, and the stretched condition be called *denormalized* state, and the process be called *denormalization*. Upon the removal of the weight the spring reverts back to the normal state, and this process be called *renormalization*. The two opposite processes make the instrument a system capable of functioning for the purpose of measurement. *Nothing can be a measuring instrument unless it has an in built tendency or capacity to get back to the normal state.*

A basic understanding of the neurophysiology suggests that the physiological (structural and functional) form of sense organs or neurons—our internal 'instruments' that make perception possible—have an analogous structure. We shall illustrate it in the general pattern of the physiology of transmission of nerve impulses.

The membrane of a nerve (neurolemma) when at rest, is in a state of electrical *polarization* called the state of *resting potential.* When a nerve is excited, due to some cause of excitation such as light, sound etc., the polarized state of the resting potential gets disturbed to another state called *depolarization* transforming the resting potential into *action potential,* which then becomes a signal that gets transmitted from one location of the membrane to another. The action potential generated at one location causes depolarization in the successive location. Soon after depolarization, the process of *repolarization* starts reverting the surface of the membrane back to the original polarized state. And this continues in a succession by repeated 'cycles' of the opposite processes—depolarization and repolarization. The analogy with the functional model of a measuring instrument is clear: normalization is analogous to polarization, depolarization to denormalization, and renormalization to repolarization. The difference in the examples illustrated is that measuring instrument is static, while the neuron is dynamic. This kind of model can be generally referred to as *equilibrium models,* whether static or dynamic. In fact the difference between a sense organ and a neuron, in terms of the physiological model, can be stated to be the difference between static and dynamic equilibrium. Therefore, the functional pattern of a sense organ makes a perfect analogy with the functional pattern of a measuring instrument, with of course the above stated difference that the measuring instrument is external to the subject.

What emerges out of this account is that the structural pattern based on polarization or opposition is an essential aspect of structures capable of perception as well as measurement. This also becomes an illustration of the nature of inverse systematization. *Nothing is a system of significance unless the 'opposites' form an integral part of it.* It is worthwhile repeating the point made above that there exists no contradiction in holding the opposites together in a single system, either as a conceptual system or as a physical system, at the same time. Unlike the opposition based on negation that forms the basis of a system of statements, this species of opposition has the character of *cementing* the opposites.

If it is correct to say that measuring instruments are 'sensitive' systems that are external to the subject, then we have an access to some other 'sensitive' systems other than our sense organs to 'experience' the world for us. Since measuring instruments respond specifically to certain external phenomena according to a definite functional relation, and since it is possible to obtain statements expressing proportions which are invariant with respect to a group of transformations (operations), and since such an invariance does not depend on the vagaries of subjective perceptions, the knowledge we get through measuring instruments is *objective.* Since relativity in science is connected to the notion of invariance in the sense explicated above, objectivity and relativity are necessarily connected notions.

If we are correct in stating that scientific way of understanding the world is by the method of constructing or synthesizing systems through which we can arrive at objective invariant proportions or *ratios,* then it would be appropriate to say that the rationality of science consists in this *RATIOnality* which is based on inversion and not the rationality based on deductive systematization which in turn is based on negation.

6.11 Inversion and Equilibrium

Just as symmetry is logically connected to inversion, equilibrium[44] is also connected to it in the similar manner, in the sense that equilibrium is inconceivable without inversion. Most models that scientists have constructed are either models based on symmetry or on equilibrium. *An equilibrium system is a result of composing additive inverses.*[45] In the context of generating scientific knowledge we claim that a study of conditions that produce equilibrium structures must be taken seriously.

We have noted in §5.2 page 131 that there are two epistemologically distinct stages in the process of theorization, one stage is the transition from phenomena to physical systems and the second stage is the transition from the physical system to the higher postulates and models. We claim that the first stage is often accomplished if a system of equilibrium is

[44]We are using equilibrium in a more general sense including steady-state, stability, balance, dynamic and static equilibria, homeostasis, etc.

[45]We think that there seems to be some plausibility is the idea that equilibrium and symmetry are logically distinct, and their distinction could be as sharp as the distinction between additive and multiplicative inverses. Establishing this demands a separate study.

realized in the problem context. We take encouragement from the history of science. The sciences developed by Archimedes, statics, hydrostatics, and mechanics are based on the principles of equilibrium; Galileo solved the riddle of motion by the application of a model of balance; Newton's first law (the law of inertia) and the third law (that action and reaction are equal and opposite) are principles resembling the principles of equilibrium. Various other cases can be listed: the method of balancing chemical equations; both equilibrium and nonequilibrium thermodynamics; reversible and irreversible systems; chemical kinetics; various systems maintaining homeostasis in biology; models of speciation based on population genetics; ecology; the study of dynamical systems; the entire discipline of control theory; major methodological schools such as systems theory, cybernetics etc., are all various fields of inquiry where the notion of equilibrium has played a vital role, specially in the initial stages of development. It must be possible to reconstruct the genesis and development of these disciplines based on the idea of inversion, because inversion is a necessary condition of equilibrium.

Since equilibrium is a state where the additive inverses coexist, it is a system constructible on the basis of the principle of included extremes. In the case studies we have presented an initial attempt of reconstructing some of the early stages in the development of scientific knowledge where equilibrium is involved. Appendix B (B page 321) includes the case of Population Genetics where the use of equilibrium model helped in the mathematization of a biological system.

One of the problems then of a generativist is to formulate the conditions necessary for constructing structures which have either the property of equilibrium or symmetry. In this thesis we are not pursuing the problem of constructing symmetric structures, which is highly relevant especially when one is considering the problem of building highly abstract mathematical models. We will spare the rest of the space in this work for equilibrium, which, as already proposed, has a crucial role to play in the initial stage of transforming phenomena into physical systems.

Part III

Case Studies

Chapter 7

Genesis of Scientific Knowledge

When and where did science take birth? Is this at all a legitimate question? Science is not among those things that come and go, but it is that thread-like thing that is woven by people who come and go; it is a public object that is used and developed by a large community. Access to it is through language, some might say science is nothing more than a specialized language. We can not possibly 'feel' the entire body of science. What we can however experience are certain ideas represented through symbols. So the body of science at any given point of time can be identified with a group of interconnected ideas. Hence our talk of science and its genesis has to be that of the genesis of ideas that have somehow become inalienable parts of science.

Some of the ideas that cannot be separated from science are the ideas of conservation, analysis and synthesis, the search for causes and underlying order, equilibrium, symmetry etc. It is very difficult to ascertain when the seeds are sown and at what place. It might very well be the case that they are given *a priori*. We shall not be engaging ourselves in these matters as they would take us far afield. What we shall attempt is to present interconnections between the ideas in a manner that would project the role of the logical relation, inversion, and then reveal its foundational character in the context.

Our natural starting point is around the 7th c B.C. in and in and around Greece. Whatever be the actual origin of science and its method, we have evidence at least to the effect that the intellectual soil of ancient Greece and Rome was undoubtedly fertile enough to provide foundations to the edifice called science. What these foundations are can best be known by grasping some of the presuppositions current in their times, because the foundations of any intellectual tradition, we think, consists in certain fundamental ideas that cannot be questioned; for they are not beliefs that are made but presuppositions that aid in making beliefs. These are, we claim, in the form of certain thematic-pairs (pre-suppository idea-pairs).[1] Philosophical speculation, among most philosophical schools, including ancient Greek thought, is rich in thematic-pairs. For example persistence and change (Being and Becoming), one and many, heavenly and mundane, beautiful and ugly, just and unjust, real and apparent, analysis and synthesis, etc. These thematic-pairs by being an inseparable part of the tradition, function as regulatory constraints, controlling the thinking patterns of a tradition.

A few words about our motivation in tracing the roots of science in those ancient times as far back as the 7th c. B.C. are in order. The motivation is two fold. First, as already mentioned, it is to understand the foundational presuppositions of science; the second is to denounce a prevailing and highly influential view of the present century that metaphysics and science are to be distinguished as fundamentally different forms of inquiry about the world. The positivists' program to eliminate metaphysics from science, is well known and requires no elaboration.[2] It is important to keep this at the back of our mind while studying the roots of science in what is otherwise regarded by some as non-science. While no one would deny that some ideas of metaphysical speculation could have influenced the development of scientific thinking, it would be unpalatable to a large number of thinkers if one were to say that the *foundational structure* of science and metaphysics is essentially the same. In this connection it may be appropriate to quote Wartofsky's observation on the relationship between science and metaphysics.

> Contemporary science still operates within the conceptual frameworks of matter and form, of structure and function, of laws of change and development. Like the Greeks, we postulate theoretical entities to explain the phenomena, and like theirs, our science has a deep sense of the underlying mathematical structures of the physical world. Like them, too, we are not satisfied merely to acknowledge these pervasive deep structures of our thought, but are prone to assess them critically, to pursue that rational analysis of what we mean and what we understand, which is the true and broad sense of philosophy. But also like the Greeks, we are even in the flush of our rationality haunted by the irrational, by the mysterious and the unfathomable. Their supreme intellectual vitality saw this as a challenge to reason, not as an invitation to despair. This makes them vividly contemporary, for although our science has far outstripped theirs in the *content* of our theoretical understanding and in the scope of our control over nature, it is *profoundly continuous* with theirs.[3]

While we accept a degree of difference in terms of specificity, metaphysics and science cannot

[1] The expression "thematic-pair" is a modified adaptation from Gerald Holton's expression "thema anti-thema pair".

[2] Metaphysics was dismissed as nonsensical by the positivists such as Carnap and Ayer. Though Popper didn't agree on this point, he suggested that metaphysics is meaningful, but unfalsifiable and therefore not scientific.

[3] Wartofsky, Marx W. 1968, *Conceptual Foundations of Scientific Thought*, p. 95. Our italics.

be distinguished on the basis of either meaningfulness or falsifiability. We have already argued above that, in the context of generation—when scientists are engaged in creative/constructive thinking—truth and falsity considerations alone are not the determining epistemic values.

With these observations we shall get into the main theme of this chapter—the role of inversion in the genesis of scientific knowledge.

7.1 Substance and Form

One of the highly entrenched thematic-pairs in ancient Greek thinking which gets entwined with many other fundamental thematic-pairs is Being and Becoming. Some of the entwined thematic-pairs are Permanence and Change, Heavenly and Mundane, Just and Unjust, One and Many, Real and Apparent, Universal and Particular, Subject and Predicate, etc. These pairs are to our immediate understanding semantically opposed notions. In ancient Greece the association of opposites had an order such that Being was associated with oneness, heavenly, permanence, real, etc., and Becoming with their opposites. These associations remained entwined as if they were logically necessary for a long period. It took many centuries to see the possibilities of untwining the associations among the thematic-pairs. Though initially this sort of associated order gave tremendous impetus to attempts at systematization, later developments suggest that they also constrained creative and parallel thinking. Some of the most modern developments in science can be understood as those that untwined the older associations, that were till then taken to be necessary. It is indeed possible to rewrite the intellectual history of western thought in terms of the discovery of entwinement and untwinement of thematic-pairs, old and new. The present attempt is an initiative in this direction.

The account that follows starts with one of the first attempts of systematic thinking by Thales (b 624 B.C.), who is generally regarded as the initiator of a series of speculations about the nature of the world and we shall follow the developments up to Archimedes (287 - 212 A.D.). Our attempt will not be to present a historical account of ideas of this period. Nor are we going to present any new historical details. It is primarily a *re-presentation* of those familiar ideas borrowed from works of history of philosophy and science, in order to achieve the desired objective.

The major problems that engaged the ancient thinkers of Miletus, who are known by the name *physiologoi* or the Pre-Socratics, are basically of two kinds. One may be called *the search for raw material* and the other *the problem of change*. The former problem is: What is the basic stuff or substance of nature? Those who could allow for more than one raw material asked the question: What are the raw materials or ingredients which go to form objects of the natural world? The latter problem is to explain the changes in the ever transforming world: What process is responsible for the changes in the world? What agencies control this process?

These questions might have carried different meanings if the sort of answers that were sought were mythological. The Physiologoi were, instead, seeking *physical* 'answers', that is why they were called as they were.[4]

These Pre-Socratics formulated a series of alternative hypotheses in their efforts to solve these two problems. However these hypotheses were not proposed independently.

> Rather, they [Pre-Socratics] developed in the course of criticism, each representing an attempt to overcome the inadequacies or inconsistencies of the preceding one. What emerges is a dialectic - a process of conjecture and criticism - which marks off this mode of thought from acritical commonsense and myth.[5]

In the above passage Wartofsky correctly highlights certain essential factors, such as the critical attitude, the hypothetical nature of the proposals, and the supersession of commonsense and myth (also perhaps mysticism), playing their role in the genesis of scientific thought. We wish to go slightly deeper and attempt to understand what presuppositions are at the back of their mind when they criticized one another and postulated another idea.

The two problems mentioned above gain their significance from two corresponding facts of a highly general kind that form part of the accepted knowledge of that period. The facts are that the world contained a variety of things, and that they are in constant flux. The facts can be briefly labeled as 'variety' and 'variability'. These specifically gain significance in the present context because the two synthetic methods, inversion and taxonomy, correspond to systematizing variability and variety of the world respectively. The interdependence between the two problems generates a methodological tension between inversion and taxonomy, which we shall elaborate below.

The various theories offered by the thinkers of this period reveal that what they were seeking was indeed an explanation in terms of the first principles of nature. Seeking an explanation could mean several things. First that all the *observed variety is apparent, complex and confusing*, but the underlying reality is in fact *simple*. Such explanations are premised on the belief that most immediate perceivable matter in the world is not basic but derived from some primordial substance, and that it is from this basic substance that the

[4] The Greek term for 'nature' is *'physis'*, and by *physiologoi* it is meant 'inquirers into nature'. Wartofsky, *ibid* p. 71.

[5] *Ibid.*

world is produced by a process of transformation or change, where change is taken as a matter of fact.

Let us see some of the elements of the theories of this initial phase and then return to this question in order to understand their presuppositional thematic-pairs.

One of the initial attempts at ordering the variety of nature culminated in the *taxonomy* of substances. The variety of nature according to the Greeks can be reduced to the four basic elements, earth, water, air and fire. This taxonomic systematization could have satisfied the mind, but the fact that one kind of substance can be transformed into other kinds generated a problem for this taxonomic order. Thus the reason to search for more basic elements started. If it were the case that none of these basic elements transformed into another, taxonomic ordering would not have created any problem. Thus transformation of things threatened taxonomic ordering. They searched for a theory of the natural world that should encompass the two contrasting features of stability and change substantially.[6] This continues to be an eternal tension in the pursuit of scientific knowledge.

Thales postulates that the most basic substance is seen in water—a *concrete* substance—from which other basic substances like earth air and fire come into being. Something that is concrete becomes an explanatory principle. This is in fact an interesting move of the *physiologoi*, as against the mythological and religious principles, that makes his thesis extra significant from a historical point of view. He regards water as basic because water plays a crucial part in many familiar natural processes, and it is widespread in nature. It occurs in many different forms. Weather and seasonal cycles are also understood in terms of water. It is involved in the lives of animals, plants, and human beings, is also predominantly seen in inanimate things. Since it occurs in solid, liquid and gaseous state, it must be the basic substance. The most important reason that he gives is that water is basically indeterminate: colorless, tasteless, transparent and without any shape of its own. It can however take up any color, taste and shape.

These reasons for considering water as basic, throw more light on the presuppositions behind his thinking. A candidate for the position of 'basic stuff' must have no determinate individual property of its own. Thales and later other thinkers thought that *the determinate should be explained in terms of the indeterminate*, its opposite. Though Thales was 'wrong' in considering water as indeterminate this move is remarkable because it posits that if a basic stuff has *qualities* it stands in need of an explanation, thus it cannot itself be an explaining principle. This is also Anaximander's (611-557 B.C.) central objection against

[6]Cf. S. Toulmin and J. Goodfield 1962, *The Architecture of Matter*, p. 47.

Thales' proposal. The basic stuff therefore must be of an unchanging kind. Plato joins Anaximander in criticizing Thales in *Timaeus*:

> Consider first the stuff we call water. When this is compressed, we observe it - or so we suppose - turning into earth and rock, and this same stuff, when evaporated and dispersed, turns into wind and air; the air catches fire and turns to flame; while, reversing the process, the fire will revert to the form of air by being compressed and extinguished, the air condensing once more as cloud and mist. From these, still more compressed, flows water; and from water come earth and rock again: so that (as it seems) they take part in a cycle of reciprocal transformation.
>
> Now, since no one of these material substances ever retains its original character unchanged throughout these transformations, which of them can we without embarrassment assert to be the real 'this' - the ultimate constituent of the thing in question?

Anaximander has another argument that is especially relevant here. The different kinds of matter are opposed to one another, and alternates between wet and dry, hot and cold, are at par. If everything contains water they all must be predominantly wet. Though Thales was right in looking for some unchanging element, he was mistaken in his proposal. Thus water as a basic substance was rejected by Anaximander, and he proposed that the basic stuff should be entirely characterless. His theory is that *apeiron*, the indefinite, 'secreted' water. The mechanism of 'secretion' is as follows: from *apeiron* every day substances were separated out by opposite qualities. The first to come is heat and cold by the differentiation of the undifferentiated. These in turn gave rise to earth, air, and fire.[7]

The affinities of the modern notion of *energy* with *apeiron* should be pointed out to show the significance of this attempt by Anaximander. Toulmin and Goodfield, in this connection, make the following connection: "In itself, energy is neither magnetic nor electric, neither kinetic nor potential, neither matter nor radiation; but it is capable of manifesting itself alternatively, either as electromagnetic radiation, or as mass, or as the energy of motion.[8]

Here Anaximander becomes rather more *abstract* than his master, Thales, who chose a *concrete* explanatory principle. His basic substance is—contrary to his master—imperceivable. The opposites created from the basis swing like a pendulum from hot to cold, from wet to dry, to produce the seasons of the year. That the genesis is accounted for in terms of inversely related opposites makes his thesis specially significant, for *it explains the genesis of variety as a consequence of inverse variations*. The potentiality of inversely ordered

[7] Cf. C.S. Peirce, in *Historical Perspectives on Peirce's Logic of Science: A History of Science Part-I* Carolyn Eisele (ed) 1985, p. 167.

[8] *Op.cit.* p. 50.

'structure' to generate variety is realized by him. In this way the two basic problems, of variety and variation, become interrelated, showing the possibilities for systematic endeavor. Although very general a model has been conceived, which can account and connect both the problems—variety and invariance.

Anaximenes (588 – 524 B.C.), a pupil of Anaximander denied his teacher's Boundless (*apeiron*) by saying that substance is always with qualities. And he also denied Thales' basic substance Water, by proposing Air (*pneuma*) in its place. Anaximenes did not find it necessary to postulate a *substance* like *apeiron* that is totally devoid of all its qualities. Instead he thought of *pneuma,* which being invisible, colorless, odorless in its pure form can be the basic substance. From this universal pneuma everything else can be generated. When it is homogeneous, that is when the inherent opposites are in a sort of equilibrium, the air becomes pure and invisible.[9] The effect of opposites such as heat and cold, wet and dry make it visible. His solution to the problem of variability or change is more significant, for he initiated certain themes that have been retained in one form or other to this date. His solution consists in *the principle of condensation and rarefaction*. According to this doctrine, when Air becomes rarefied it becomes fire, when condensed it becomes wind, cloud, water, earth and stone, in increasing degree of condensation.

His solution to the problem is remarkably ingenious. For he comes out with an abstract thematic-pair of *processes* rather than qualities of substances, which for him are concrete. This is unlike his teacher for whom the basic substance is an abstraction, in the sense that it is 'shaved' of all the qualities. The specific ingenuity consists in showing the possibility of an explanatory model for the changes taking place in the world.

The mechanism of transformation proposed by him, in terms of condensation and rarefaction, is an abstraction in so far as it is taken out of certain familiar concrete actions and common experience rooted in the technical skills concerned with pressure variation and from the observation of evaporation and condensation. It is cited as an example of an explanatory model developed from a technological model.[10] Human beings' action and their attempt to control and manipulate of natural phenomena could be a rich source of explanatory models. Piaget's genetic epistemology draws heavily on such actions or operations inverse to each other as the source of abstract theoretical notions.

The significance of Anaximenes' explanatory model in the present context lies in the

[9]Visibility or perceivability depends on whether an object is homogeneous or heterogeneous, is in itself an interesting idea applied in modern material science. Transparency of materials, such as glass, depend on whether the order of the elements in the material are homogeneous (pure) or not. Materials become opaque mainly because they are heterogeneous or impure. Disorder in the crystal lattice produces similar effects.

[10]Cf. Wartofsky, *op.cit.* p. 74.

fact that it is one of the first abstract theories that is semantically structured on the basis of an *inverse relation*. Though the idea emerges out of concrete human actions or operations, once it comes out in the specified form, leaving the content behind, it becomes a *symbolic form*. The advantage of this 'alienation' from the original context and content is that we become free to apply it in contexts other than the original, and also fit in content so different from the original. For example, Anaximenes' model found application in the characterization of longitudinal waves.

The emphasis that we have been giving here to Anaximenes' ideas is rather unusual. For example, Peirce observes that Anaximenes' theory is more observational than that of Anaximander, citing his idea that celestial things like sun, moon etc., are thin disks and not spheres because disc shape is better supported by air than the spherical shape. But considering an observation from mundane experience, and extending it to the heavenly spheres is more radical and crucial from the point of view of the development of science than Peirce appreciates. Though Anaximenes' was also a speculative model it was a clear instance of constrained speculation, which is a necessary tenet of scientific thinking. Applying earthy features like 'disk shape' to heavenly bodies should be regarded as an interesting turn in the history of science. Here he did not hesitate to attribute a property of earthly things to that of heavenly things, contrary to the then dominant view that heavenly things are Beings that do not Become. It may also be pointed out again that the significance of making an air like thing and not *apeiron* fundamental is again indicative of his tendency to come back to earth. This is rather essential for the development of natural science, which though it postulates abstract models, has as its ultimate objective their application to concrete natural phenomena. He may be wrong in making air the basic substance, or calling 'heavenly' spheres as 'earthy' disks, but his move possesses the character of natural science.

It may also be pointed out that Peirce makes no mention of the theory of condensation and rarefaction while presenting the ideas of Pre-Socratics. He writes only two small paragraphs of only ten lines about Anaximenes.[11] However Peirce spends much time in elaborating the quantitative or mathematical models proposed by the Pythagoreans and Archimedes. This further supports our earlier remark that the significance of Anaximenes' contributions to science is not well recognized. To our understanding the contributions of Anaximenes are necessary for developing mathematical or quantitative models, because it is in his thinking that one of the most vital ideas of making *relation* as an explaining principle emerges. Since it is well known that the basis of mathematical understanding is relational,

[11] *Op.cit.* p. 167.

the genesis of this notion in his thinking requires some elaboration.

This can be done by understanding the presuppositions behind his thesis of condensation and rarefaction. From this foundational theme begins to emerge another thematic-pair, *form and matter*. The notion of compression implies that the 'particles' of air come closer and closer, increasing the density of the substance. And the notion of rarefaction implies just the opposite. This idea is extremely crucial because the substance itself need not change in order to produce a change in 'out-look'(manifestation), but only a change in spatial *relation*ship in terms of proximity and distance is sufficient. This implies therefore that Anaximenes, knowingly or unknowingly, supposes a distinction between substance and relation, the former being concrete and the latter abstract. Thus the hypothesis proposed by him introduces a new dimension in scientific thinking making the changing relationship of the concrete unchanging substance a central feature of the mechanism of transformation. Without this idea neither atomistic nor mathematical conception of the world would be possible. This understanding appears to have support in the historical fact that Atomism, in its mathematical form developed by Pythagoras and in its physical form developed by Empedocles, Anaxagoras and Democritus, appear soon after the first phase of speculations, which are generally characterized as those based on substance as opposed to form.

7.2 Change and Persistence

The developments made by Atomists can be best understood against a backdrop of the two opposing traditions of Heraclitus and Parmenides.

Heraclitus (535-475 BC) believed that the universe is in a state of flux or ceaseless change. Every element of every substance is a union of opposite qualities, and this unit is never stable. Harmony is a result of 'stirred' union of opposites. Since fire symbolizes the notion of incessant activity, something that never comes to rest, he chooses this as the basic element. Where does he see the unity of nature? The *appearance* of things is that they are many, discrete and separate, but the *reality* underlying these appearances is the unity of this constant flux and transformation. Since this operates by necessity, the flux is ordered, and therefore it can be understood. The basic element, fire, plays the role of 'exchange of currency' in the various transformations. "All things are an equal exchange for fire and fire for all things, as goods are for gold and gold for goods."[12]

A theme that lived and perhaps may live a long time is the pair *appearance and reality*. Though the idea is tacitly supposed in the predecessors of Heraclitus it becomes manifest

[12] Quoted in Wartofsky *op.cit.* p. 75.

with him. This distinction is extraordinarily important in the development of science and also philosophy.[13] Along with this arrives another pair, *sense and reason*, and later *observation and theory*. The entire history of science and philosophy of science can be characterized by the tension between the above interrelated pairs. Despite the constant flux and transformation there is the underlying unity of the *logos* (reason) which ensures comprehensibility. The thematic-pairs, appearance and reality, sense and reason, one and many become entwined in the following way:

> what appears to the sense as *many* is ultimately *one*; thus the senses without reason are deceptive.[14]

Parmenides (b.510 BC) challenged Heraclitus by formulating the antithesis to the thesis of Heraclitus. According to him only one eternal, underived, unchangeable being can exist. It is continuous, indivisible without breaks, an indeterminate mass. His explanation of the fact that we do perceive change in the world is that the world of sense may perceive change, but it is an illusion. Perception confuses Being with non-being (non-being is not Becoming), and Being is given unadulterated only by reason. His main argument against the Heraclitian notion is that a thing cannot change its qualities. To say that it can is to say that *something is* and *something is not*, that something can come from nothing, and that something can become nothing.

We should not confuse the two thematic-pairs, permanence and change with that of Being and Becoming, since in this phase no philosopher has made such a correspondence. Both Heraclitus and Parmenides, and also the predecessors of them, were trying to grapple with the nature of Being itself, i.e., whether Being is permanent or changing. After Plato the question of whether Being is permanent or changing should never arise, because Being is by 'definition' permanent or unchanging. It can be seen easily by paraphrasing the theses of Heraclitus and Parmenides in the following way: Heraclitus says *Being is Becoming*, while Parmenides says *Being is Being*, and is only apparently Becoming. Plato develops the idea of Parmenides, and after him Being becomes entwined *permanently* with permanence, and Becoming with change.

An interesting aspect of Heraclitus is that the basic *Being itself contains both the opposites*. That opposites can coexist in one Being, provoked the *Eleatics*. In their attempts to *prove the impossibility* the most basic principle of logic, namely the principle of noncontradiction, emerges. To say that Being *is* and *is not* is contradiction. If we were to trace

[13] Cf. Wartofsky *ibid* p. 76.

[14] Wartofsky, *Ibid,* p. 76. Another famous distinction between primary and secondary qualities, to be introduced only later by the Atomists (Democritus), can be readily seen to be entwined with the above pairs.

the birth of logic (deductive), which we are not presently engaged in, we would do it at this phase of history. The tension between One and Many, Being (is) and non-Being (is-not)[15] has made possible the genesis of certain foundational principles upon which the edifice of deductive logic was later built. Zeno, a disciple of Parmenides, developed a method of proof commonly known as *reductio ad absurdum*, which had a remarkable career in both logic and mathematics.

For one thing this phase becomes a major turning point, and also a 'breaking' point, because one of the monadic logical operators, negation, comes out of the soup of opposites, and becomes a bed-rock of deductive logic. We are now attempting to show that science during the course of its development extracted another monadic operator, inversion, from that very soup. We intend to *make this* the bed-rock of synthetic logic, which, we claim, has meanwhile developed sufficient reasons of its own to become an independent logic.

The conflict between Heraclitus and Parmenides (b. 510 BC) can be interpreted as the tension between the two kinds of logical opposites, *negation* and *inversion*. The former kind cannot allow the presence of opposites at the same 'site', which the latter necessarily allows. Parmenides fashioned the former idea which is presupposed in the principle of non-contradiction. It may be possible to show that Heraclitus fashioned the latter idea, for if it is only that which can allow the opposites to be together or coexist. This is a possible interpretation, which however needs to be supported from textual evidence.

Coming back to the issue, for Atomists both the theories—of Heraclitus and Parmenides—have an element of truth. Things appear to persist and also appear to change. How is it possible for things to persist and yet change? To get out of this impasse some reconciliation is called for. Such an attempt was made, as already mentioned, by the Greek Atomists, Empedocles, Anaxagoras and Democritus, who furnished a solution. Their thesis was remarkable for its ingenuity in its anticipation of modern scientific notions. The solution mainly consists in interpreting the predecessors' notions of permanence and change as not absolute but relative notions. With this manoeuvre they could now say relative permanence and change produce no riddle. The solution of the riddle, is neatly summarized by Frank Thilly.

> [A]bsolute change, they [Eliatics] say, is impossible; so far the Eleatics are right. It is impossible for a thing to come from nothing, to become nothing, and to change absolutely. And yet we have the right to speak of origin and decay, growth and change, in a relative sense. There are beings or particles of reality that are permanent, original, imperishable, underived, and these cannot change into

[15] For later thinkers like Sartre and Heidegger the tension is represented in the form of Being and Nothingness.

> anything else; they are what they are and must remain so, just as the Eleatic school maintains. These beings, or particles of reality, however, can be combined and separated, and when combined they form bodies that can again be resolved into their elements. The original bits of reality cannot be created or destroyed or change their nature, but they can change their relations in respect to each other, and this is what is meant by change. ... Origin means combination, decay separation; *change is an alternation of the mutual relations of elements.*[16]

For the Atomists Being is indivisible, eternal, and unchangeable, and on this point they is no disagreement with Parmenides. But they disagreed with him on the belief that Being is one. Their Being is many. It does not imply that Being is divisible, because Being is as such many, and each particle (atom) of Being is certainly indivisible. The indivisible atoms are 'simple', from which more 'complex' things can be created by combination. If this is how the thesis of Parmenides is contained in the synthesis, in what way is the thesis of Heraclitus contained in it? Change or transformation of things is possible and it takes place due to separation and combination.

Also note the close relation between Anaximenes' synthesis and the Atomists' synthesis with regard to the employment of the thematic idea, form. Both believed that change or transformation is an alteration of the form, i.e. mutual relations of elements. But their synthesis employs the processes of separation and combination, and not the doctrine of condensation and rarefaction. For the Atomists the thematic-pairs part and whole, and simple and complex are vital presumptions, while for the Anaximenes they are not crucial. This move again is very significant.

With these ideas added in the ground the soil became fertile, and the 'seed' being already there, i.e. the urge to solve the riddle, we have all the necessary conditions available in the foundation for the birth of the thematic-pair, analysis and synthesis. It is represented here in its synonymous form, separation and combination.

For some Atomists like Empedocles there are four basic kinds of elements corresponding to the four basic qualities, solidity, liquidity, fiery, and aeriform. For Anaxagoras there are infinitely many elements corresponding infinitely many qualities. Most of the perceivable things of the world are made up of combinations of *all* the infinite variety of kinds of elements. The *numerical preponderance* (which is same as proportional variation) of one over another introduces distinctions among things. For Leucippus too the number of atoms is infinite. "The material of the atoms themselves is packed entirely close, and can be called *what is*; while they are free to move through void (which may be called *what is not*). By

[16]F. Thilly 1956, *A History of Philosophy* p. 41. Our italics.

coming together in association, they are responsible for the creation of material things: by separation and dissociation, for their disappearance."[17]

Greek thinkers have possibly played with all possible constructions of the world. Earlier we have seen modeling the world on the basis of the Boundless (*apeiron*), and here with the Atomists we see the bounded, uncuttable atoms, a limit beyond which no homogeneous substance could be divided, as the basis. This notion they have seen can be coherent only if there is *void*. As we have seen above in the Leukippos quotation, they did postulate void, the atoms are separated by the regions of space devoid of all properties. The necessity of void, which is *what is not*, could have been felt due to the problems posed by Parmenides in response to Heraclitus' theory. The dilemma to accept or reject void (vacuum) can be seen through out the development of science.

Though the model of Atomism with all its features has its worth it has brought with it another notion that is worthy of special consideration, which is the idea of *conservation*. They have successfully shown the *possibility of conservation in the world of transformation*. In the words of Empedocles,

> From what is wholly non-existent nothing can arise: and for what truly exists, to perish is impossible and inconceivable; for it must always continue to exist, wherever one may put it.[18]

Though truly existing things do not perish, different material things come and go as a result of combinations and separations of the basic elements in *varying proportions*, not varying substance, which is conserved.

The point to be made here is the necessary connection between the tendency to prove the conservation of substance and explain the transformation of substance with the inversely ordered explanatory model in terms of combination and separation. Changing relations of the elements is the theme fashioned by the Atomists, applying the idea of Anaximenes. Once we choose this theme as the explanatory basis, the kind of models that we can build can develop on the basis of the two possible relational variations that are inverse to each other. Whether the relation is compression and rarefaction of one basic substance like air, or combinations and separations of many elements or one element.[19] Thus the structure of the explanatory models developed by Anaximenes and Atomists incorporate *inverse variations of relations*, which we claim as necessary aspect of scientific knowledge. So far the limitation of the models presented above lies in their being qualitative. The major attempts reflect

[17] Leukippos quoted in Toulmin and Goodfield 1962, *Architecture of Matter,* p. 56.
[18] From the poem of Empedocles quoted in Toulmin and Goodfield, p. 53.
[19] Pythagoreans, we shall see below, developed the form of Atomism which is based on *one* element.

the reduction of certain qualities in terms of others. But the attempt to reduce qualities to quantities has not been shown to be a possibility. However, in the models just presented the idea of numerical proportion is lurking inconspicuously. In order that the notion of numerical proportion become a feasible alternative the idea of *unit* of measurement is necessary, which has not been developed as a part of the cosmological modeling. It may be pointed out that measurement of various quantities is already known to various civilizations much before this period (5th c. B.C.), therefore the attempts to build a quantitative cosmological model should not be confused with the very idea of measurement. Initial attempts towards quantitative or mathematical models of explanation, based on the lines of Atomists, is made by the school of Pythagoras. This was going on parallel to the development of physical atomistic theories.

In the Pythagorean tradition the elements were not taken as physical entities which could be perceived or felt, but as conceptual entities, like *number*. All things are constituted as numerical relations or ratios or proportions. The ultimate structure is mathematical. This quantitative move requires to begin with a notion of *unit* magnitude. The Pythagorean notion of unit is a confusion of both arithmetic (numerical) and geometric magnitude. This is a point-unit that cannot be cut any further, therefore atomistic, and immutable. This unit is not itself a number, but generates number. It is interesting to note that for the Pythagoreans *two* is taken to be the first number. They conceived a line to be generated out of two points, plane by three (triangle), solid by four (pyramid) etc., by relational combination of ones. The reality that is intelligible is taken to be the relation between numbers (ratio), which is also *ratio*nal.

One good example of studying proportional variation in the school is the study of the relation between the length of the string of musical instruments and the pitch of the tone of sound produced. This, opened up the possibilities for explaining phenomena by reducing qualitative into quantitative aspect. These are the initial attempts of geometrical construction of the physical world, which are so crucial for the development of modern science.

Another noteworthy point of the Pythagorean theory is that nature is a combination of opposites like limited (odd numbers) and unlimited (even numbers), one and many, rest and motion etc. This is a clear influence of the intellectual environment of that period, where to think is to think in thematic-pairs. They could not consider the importance of negativity or subtraction as opposed to positivity and addition, as the basis of modeling.

It is noteworthy that none of the *physiologoi* thought solid earth as a possible candidate for being the basic substance. Thales took the liquid water, Anaximenes the gaseous air, Heraclitus the energetic fire, but there were no takers for the solid earth. This reflects their presupposition that earthiness is not good, not just, it is regarded the ultimate mundane thing. Let us recall that Socrates was embarrassed by Parmenides who after listening to the former's postulation of the world of ideas, asked him whether the dust, mud and hair etc., the earthy things, also have corresponding Universals.[20]

Another crucial point to note is that opposites are not seen as existentially contradictory features, with the exception of Eleatics, but rather as concurrently existing features of Being. Eleatics confused the principles of thought and of Being. The condition of rational discourse is imposed on the conditions of Being. It is in the school of Eleatics that language, thought and their form have begun to become serious objects of intellectual analysis.

Before we close the section on the initial attempts at modeling the physical world the outcome of the discussion may be presented. One of the points that we attempted to highlight in general regarding the Pre-Socratics is that their thinking is regulated by opposites of different kinds. The three main models that are particularly important are the model of condensations and rarefactions (of Anaximenes), the model of combinations and separations (of physical Atomists), and the model of mathematical Atomism based on a single atom, one. All these models are developed on the basis of the possibility that inverse variations of relations can be taken as the explanatory principle for accounting for variation, and variety in the world.

In what follows we will see how the qualitatively inverse order gave way to quantitative inverse order. The latter's presence will be shown to be necessary for any development of mathematical physics, which was founded by Archimedes.

7.3 Plato and Aristotle

We have come across some of the early explanatory models based on the inverse order of highly general phenomena. One may say that for the Pre-Socratics, with some exceptions, the entire cosmos was taken as a single large and highly general process or phenomenon, and the differences within processes and phenomena were required as only apparent. Among the few specific cases where they applied their theory are the changing seasons, earth quakes, formation of clouds, the process of raining, snowing etc. According to modern standards, despite the fact that we have better theories, none of the scientific theories can account for all these phenomena. Change of seasons, and the causes of earth quakes etc., are still very far from getting accounted for by any single model. A significant fact about science emerges from this observation, which is that scientific theories are about specific phenomena, like

[20] Cf. Plato's *Parmenides*.

motion, heat, light, sound, life, etc., and within each case the generality of a theory is limited to that very case. Only within that scope and relative to it, can we speak of a theory being general or specific. Thus despite the fact that science is general, it is also specific in the sense explicated. Science and metaphysics can be distinguished on the basis of degrees of specificity or generality.

The space where science can operate, cannot be at the level of *summum genus*, nor can it be at the level of ultimate particulars. Its space is between the *summum* genus on one side, and particulars on the other.[21] Most of the world that is occupied in this space, sandwiched by the summum genus and particulars, which science has access to, is invented or discovered by certain methods and modes of scientific inquiry, such as abstraction, idealization, etc. Therefore the limitation of science is by virtue of its method. Though science has a tendency to be specific, it stops at the level of species, and to go 'beneath', if not beyond the species, is methodologically impossible. Therefore when we say that science is 'specific' it should be understood in a relative sense and not in absolute sense of referring to ultimate particulars. The tension between the generic and the specific is responsible for the birth of science and its further development. At the methodological level, the tension is between inversion and taxonomy.

This tension is also represented in later Greek thinking between Plato and Aristotle. The former's theory represents the tendency towards the highly general, abstract, and mathematical, and the method is largely hypothetical, while the latter's theory represents the tendency to root science in commonsense and everyday experience, and the method is largely inductive and taxonomic and is also against applying the mathematical method in physics. In the account that follows we shall attempt to show that this antagonism gets resolved in the development of science by a convenient 'marriage' between the two. This point however is already well understood. Therefore the question naturally arises what more is to be added to this familiar episode. Our thesis is that this 'marriage' would not have been possible without inversion.

In the thought of Plato as well as in Aristotle, the entire world is polarized into opposites, which can be characterized as *global polarization*. Since science, as already mentioned, cannot be at a global level, despite being general, there is a need to turn the analysis

[21] Both Plato and Aristotle are known to have opined so. Some interpretations however try to show that Aristotle believed in the possibility of a science that is not merely about species but about individuals. For Aristotle the essence or form inheres in the individuals. In so far as individuals can have some of the essential features that scientific knowledge can know, to that limit individuals can be object of scientific understanding. Idiosyncratic features of individuals cannot be the conditions of classification and therefore these features are outside the purview of science.

towards the local level. In the school of Aristotle the resolution of a phenomenon in terms of inversely related opposites at a local level, as in the case of balance (lever), took place. To distinguish this with the former kind we shall call this *local polarization*. This polarization made the application of Euclid's geometry (which was at that time the paradigm of mathematical science) possible in physical cases. This not only produced immediate fruits, as can be seen in the development of mechanics, statics, and hydrostatics, but has also shown a direction for inventing further models of explanation in the case of other phenomena such as motion. Brilliant minds like Galileo could receive the signals from these developments, work out the analogies, and finally begin solving the problem of motion. In what follows we will elaborate these developments.

The tendency towards dialectical thinking as a characteristic feature of the Pre-Socratics is already noted. Entwinement of the pairs of opposites reaches a climax in Plato's thinking, and is in many respects akin to that of the Pythagoreans. The pairs of opposites are entwined with Being, good and light on one side, and Becoming, evil and darkness on the other. Some of the pairs are one and many, rest and motion, straight and curved, limited and unlimited, even and odd, etc. Thinking in opposites is so prevalent that Plato's accounts about human actions, art, knowledge, ontology, are all discussed by introducing an opposition. Thus, the purely routine application of uncomprehended rules of skill on one hand is contrasted with the deliberate practical action based on insight into causality and on reflection; imitative art on one hand and creative art on the other; opinion (*doxa*) and knowledge (*episteme*); the realm of things (*pragmata*) and rational ideas (*logoi*); the world of phenomena and the world of forms.[22]

Plato's theory is a synthesis of three earlier philosophies; the mathematics of Pythagoras, the Atomism of Democritus, and the four elements of Empedocles. Plato emphasizes the Pythagorean obsession with geometrical figures, rather than their love of numbers.[23] This is indeed a positive contribution. However the fact that he was developing models of explanations that were already known, does not make it a fundamentally novel theory. When we look at his Dialogues on various subjects, such as justice, morality, beauty, knowledge, method, polis, art, techne, etc., we would know that he spent most of his efforts on subjects that are close to human concerns. It is well known that after the Sophists' 'intervention', the concern of philosophers at the time did turn from the grand models of the universe toward

[22]Cf. Dijksterhuis 1961, *The Mechanization of the World Picture* pp. 14-15.

[23]K. Popper explains that it is due to the problems of the 'irrational' which remained a great stumbling block for the development of arithmetic for a number of centuries to come. Since geometrical figures could accommodate irrational numbers, Plato attempts to give them an essential place in the ultimate triangular elements of his construction. Cf. his 1963,*Conjectures and Refutations: The Growth of Scientific Knowledge.*

human affairs. Understood from this point of view his major concern is not to develop a theory of the cosmos. However he composed a grand synthesis in his sole Dialogue in science, *Timaeus*, which should be considered as a critical culmination of the initial efforts that went on in the genesis of natural science. This Dialogue remained a major inspiration, and provided direction for later mathematicians and physicists for many centuries. Plato being what he is, his further developments of atomic and Pythagorean ideas have an order and beauty that make the work immortal.

The fundamental idea of Platonic thought, as is well known, is that the world of phenomena or perception has things that are imperfect copies, imitations of ideal forms or ideas, which are supra-sensible, and can be accomplished only by pure thought (often used synonymously with mathematical thought). Being influenced by Pythagorean views of the cosmos, his interest is in abstract mathematical constructions. In his Dialogue *Timaeus* he deals mostly with music and astronomy.

Plato's love for mathematical ordering can best be understood from his explanation of the basic elements, earth, water, air and fire. Why are there only two elements between earth and fire? It is explained as a consequence of the mathematical truth that between the cubic numbers, a^3 and b^3 there can be two mean proportionals, i.e., two terms that form with the two original terms a geometric progression (a^3, a^2b, ab^2, b^3). Cubic numbers were chosen because the objects were a creation of a three dimensional universe.

Plato's world view can be called the 'mathematical chemistry of elements' as opposed to the 'physical chemistry of elements' of the Greek Atomists. The ultimate building-block elements are two kinds of triangles, the right-angled isosceles triangle and the right-angled triangle with 30^o and 60^o degrees. The four basic elements, earth, water, air, and fire, can be formed from different but specific combinations of the two basic triangles. Four of the former kind combine to form a square, 24 of them to form the cubic corpuscles of the element earth; six of the latter kind make an equilateral triangle, 24 of them for a tetrahedral fire particle, and 120 for an icosahedral water particle. Thus one major difference, in spite of the affinities with Atomism, which stands the Platonic position on an independent foundation, is that the events taking place in the universe are not subject to blind chance. The universe was given a purpose, contrary to the purely mechanical model of the Atomists.

Despite the common framework that Plato and Aristotle share, the significant difference in their views about the ontological status of universals allows one to distinguish their positions so much so that they can even be read as antithetical views (Cf. footnote 24 page 25). For Plato universals or Forms do not 'exist' in the corporeal world, while for Aristotle they *inhere* in the actual things and processes in the world. Accordingly, for Aristotle, the reasoned experience can indeed know *this* world, while for Plato the reasoned experience can lead to understanding not this world but the world of Forms. A thing or a process is what it is by virtue of the *essence* by means of which individuation is accomplished. Aristotle's term 'form' gains a newer connotation in Greek thought. It is the characteristic that individuates an activity or an object and the way in which an object functions or operates.

The typical characteristic that we need to know for gaining scientific knowledge is, according to Aristotle, that of a *kind* of thing, and not of any character of particular things. Scientific knowledge, therefore, is about things which become proper members of a *class* obtained on the basis of those characters that are common to all the members, and not individual characters.[24] This is the *limit* Aristotle sets for scientific knowledge. Formless matter (un-classifiable) and matterless form (empty classes) are *conceptual* or *methodological* limits, which exist only as objects of thought.[25]

While Plato also believed that *episteme* consists in the relationship between Forms which can be interpreted as classes, and would also agree with the limit Aristotle sets to science, as already mentioned, he would not apply *that* knowledge to things of *this* world. Since natural science, is claimed to be about this corporeal world, it can be readily seen that Aristotle's views are closer to today's natural science. Despite all the mistakes Aristotle and his schoolmen committed, in the *Lyceum* as well as in Alexandria, as we shall see below, it was this view of science that contributed more to the study of natural order than Plato's. The latter's contributions however are commendable in the field of geometry and mathematics.

From the point of view of the present task, i.e., to see the development of the idea of inversion, Aristotle's study on motion and statics are immediately relevant. Aristotelian physics is based on the postulate that every motion presupposes a mover: all that moves is moved by something else. Aristotle's conceptions generally are close to what we observe in everyday experience. Since we apply force in order to move anything, he inductively arrives at the above principle from this. The Atomists before him postulated that atoms are in perpetual motion, and therefore the problem of the mover does not arise. As we know Aristotle is highly critical of the Atomists, and also opposed the possibility of the void. Since action at a distance is also inconceivable, the force must be in conjunction with the moving body, (*motor conjunctus*). The Atomists freely speculated about the world without

[24]That if something is of a particular kind then it would have the characteristic of that kind may sound "painfully obvious and empty", but it is the basis of formal logic of classes (class calculus) essential for the development of deductive logic, which is essentially non-ampliative. Triviality is the hallmark of the basic principles of any logic.

[25]Cf. Wartofsky, *op.cit.* p. 93.

any methodological stipulations, while Aristotle believed in the method of generalizing from everyday experience, which indeed became a major stumbling block from the point of view of giving a coherent and unambiguous interpretation of motion.

By motion Aristotle means any transition from potential to actual being. He distinguishes four species of the genus motion: 1. coming into being and passing away (*generatio* and *corruptio*) 2. altering qualities of a thing (*alteratio*) 3. increase and decrease of quantity (*augmentatio* and *diminutio*) and 4. change of place (*motus localis*). The first one involves substantial change while the latter three are accidental.

The problem of motion for Aristotle, is basically of inanimate bodies, for in the case of animate bodies the vital principle, soul, is also the principle of motion since they move by their own efforts. The motion of inanimate beings is again distinguished into *natural* and *forced*. It should be noted that his approach to every problem has the character of following the general taxonomy of things. For each category of motion a separate theory was suggested. Aristotle also had a different theory for celestial bodies, which by their very nature carry out unlimited uniform circular motions. Therefore the question of applying theories framed in connection with the terrestrial phenomena to the celestial was excluded. That different classes of objects need different explanations of the phenomena of motion, is unacceptable from the point of view of present standards of science. Thus Aristotle's obsession with taxonomic order was another stumbling block for the development of a science of motion.

Stones falling down towards earth and smoke raising up in the air are two kinds of examples of natural motion, while a stone thrown up or an arrow shot in any direction are examples of enforced motion. The earth has its natural place at the center of the universe (earth), and fire at the periphery of the universe. This classification is again based on the Greeks' ordering of the four basic elements. The two extreme elements (*elementa extrema*) earth and fire have opposing tendencies, the former gravitates, and the latter levitates, because they are extremes of the opposites heavy and light. The other two elements, *elementa media*, are placed relatively; water above earth, and air above water. Earth and fire cannot be the media for falling bodies. Motion of earth and fire can take place only in media, and motion of anything in vacuum is ruled out as impossible. Thus the main problem of motion is the problem of *elementa extrema*.

Though it impeded the progress of science of motion, the presupposition that motion takes place in a medium, has also helped to apply the principles of statics and hydrostatics to the problem of motion in general by analogy. We shall see the details shortly.

The phenomenon of falling bodies raises several problems. The observational fact that different bodies fall at different speeds needed explanation. From the instances such as a leaf fluttering to the ground and the falling of a stone and retarding of the fall when it takes place in a liquid (which is denser than air), Aristotle assumes that the time taken for a body to fall is proportional to the density of the medium and inversely proportional to the weight of the falling body. Symbolically,

$$t = k.D/W$$

where t is the time taken to fall a given distance, W the weight of the body and D the density of the medium through which the motion takes place. A body that is ten times as heavy as another must take one tenth of the time to fall a given distance. Philoponus (5th - 6th c. A.D.) rejects this principle of Aristotle precisely on observational grounds, because in reality bodies that do not greatly differ in weight do appear to have approximately the same rate of fall. This is an instance that shows that principles induced from observations can give contradictory results.

The other problem is that the velocity of a body appears to increase during fall. Since greater weight would give greater motion, the object could be thought of as gaining weight as it reaches the ground, in which case this motion is a function of distance (i.e., height). An external (accidental) factor such as distance could not have any influence because the substantial form of a falling body cannot change due to such factors. Here Aristotle is uncertain, because in the case of differential fall of objects, he says that motion can change its quality in relation to other objects, such as the medium, density being a relational property. He even proposes the proportionality relation as stated above, which is indeed a relational assertion. Therefore his theory is not free from ambiguities and uncertainties.[26]

Similar or perhaps more acute problems cropped up in his account on projectile motion. When an object is thrown up in the air, the *motor conjunctus* is not 'visible', therefore it is imperative that this be explained. His hypothesis is that after a projectile leaves the hands of a projector the body moves by the force of the successive layers of the medium, to which the projector transferred the force. That is the projectile is kept in motion by the medium. This force gets weakened as the body goes upwards, but how and why this takes place is not clear. Thus he faces problems in explaining cases where the force is invisible.

[26]Two more questions can be asked: "First: If the principle *omne quod movetur about alio movetur* is to be taken seriously, the question should also be asked: What is the *motor* of a body falling or rising respectively in a natural motion? Second: Natural falling and rising motions appear to take place with increasing velocity. How is this possible, since they are caused by a constant heaviness and lightness respectively?" Dijksterhuis 1959, *Mechanization of World Picture*, p. 169.

Aristotle's answer to the problem of bodies that move due to 'visible' force, such as pulling or pushing a vehicle on road, or a vessel on water, is also based on principles that are inductively arrived at. A vehicle moves more rapidly if it is pushed or pulled with greater force. It is countered by the resistance, which includes many things such as the weight (inertia or mass), friction of the surface. Symbolically,

$$v = F/R$$

where v is velocity, F is the force, and R is the resistance. It also faces problems because the relation, stated as above, cannot apply to cases where R is greater than F. It also follows from the relation that if $F = 0$, $v = 0$; a body which is acted upon by any force is at rest. This is usually called the ancient (peripatetic) principle of inertia based on common sense. It may be noted that the classical principle of inertia is *counter inductive* and also *counter intuitive*.

Another mistake is to divide force by resistance, as if resistance and force are qualitatively different. Today we subtract one from the other. Proper application of the mathematical operation is a vital consideration for accepting or rejecting a thesis. However, these remarks appear simple, and what Aristotle did is a mistake only retrospectively from our own standpoint. The fact remains that at that time the method of applying mathematics to the 'mundane' physical phenomena was not yet discovered. To know which mathematical operation should be applied it is essential to realize that quantities that are inversely related are of the same 'kind' or of different 'kind'. Today we consider force and resistance as oppositely acting forces, based on Newton's third law, therefore we are justified in applying vector addition of one with the other to know the net effect. If they are of different 'kind' and inversely related we should be multiplying them. Considering the state-of-the-art of the mathematics of that time it was very difficult to conceive of the operation of multiplication on dimensions that were non-geometric. With the exception of geometrical quantities ancient investigators hesitated to multiply other physical dimensions with one another. These historical observations suggest that it is perhaps dangerous to say that Aristotle was wrong because he divided force by weight. Nevertheless, the conclusions drawn follows from what he *did* state, and therefore can be properly subjected to criticism by implication, realizing at the same time the limitations of the intellectual equipment available at the time. A comparison of Aristotle's method of calculating and Galileo's method of calculating will be presented in the next chapter.

Another problem emerges when we compare the above relation with the fall of bodies. In the case of the fall of bodies, velocity is stated to be directly proportional to weight, while in the case of bodies that are pulled or pushed, the weight of bodies impedes motion. This is another instance where contradiction arises out of inductive generalizations. It reinforces the Pre-Socratics' thinking that observations based on experience cannot be the basis of knowledge of the world, because our experience is complex, contradictory, ambiguous, unclear, chaotic and what not. They took clues from certain phenomena like condensation or combinations etc., and abstracted from them (non-inductively) certain explanatory models.

We have seen that Aristotle had one account for each kind of motion. He never had one very general theory of motion. Did he achieve a correct picture within each specific kind? If that were the case the problem would have been only to come up with one general theory of motion. He faced problems of incompleteness, ambiguity, uncertainty in almost every proposed theory of each specific kind, while in some cases the problems were more acute than others. The account presented above is not exhaustive enough to claim any definite proof regarding the criticism of his theories. However, from whatever little is presented, the nature of the problems that arise are suggestive enough to draw some general conclusions.

Another comment may be appropriate. The view held by relativists, such as Kuhn, that Aristotle's theory is internally coherent seems a difficult proposition to appreciate. Asserting contradictory statements, being ambiguous and uncertain etc., are not conditions that modern scientists' would alone impose on any inquiry. Aristotle being a logician would not ignore the above criticisms had he known of them. But these criticisms could not have developed in his time, for want of an alternative conceptual scheme. After all the stringent conditions of demonstration proposed by him in *Posterior Analytics* would not have passed his own theory. However the major limitation before any logical method like Aristotle's method of demonstration is that it cannot do anything with undefined or ambiguous notions. It is one thing to say that Aristotle's conceptual scheme is different from, say, Galileo's. Surely it is. But it is another thing to say that Aristotle and Galileo had different standards of rationality. To get into the details of this very important and involved debate would take us far afield, and would be a digression from the point of view of the task of this essay, namely, to display the role of inversion in the genesis and development and structure of science. We shall however show some clear continuity in the way how the problems get solved.

Aristotle's failure to reach a coherent and unambiguous interpretation of motion is also an example of the failure of the inductive (direct) method of arriving at the postulates. This is not to suggest that induction cannot find correct correlations among phenomena. What is suggested is that different generalizations all induced from experience can lead to contradictory results. As long as the conceptual scheme is ambiguous, and the notions unclearly defined, the inductive method is susceptible to failure. But the method followed by Aristotle, his inclination to relate increase or decrease of one quantity (or quality) with another is what is desirable for the advancement of scientific knowledge. If only Aristotle's concepts had achieved adequate clarity, and freedom from ambiguity, the same method could have given a different solution. But how does one find an unambiguous conceptual scheme on the basis of which we can go further? We claim that the inductive method alone cannot generate a kind of conceptual scheme that science needs. There can be other conceptual schemes that the inductive method can generate, such as those based on classification methods. But a science that is quantitative and experimental cannot be developed out of direct methods like induction. Both quantitative and experimental methods presuppose certain indirectly obtained structures or models.

Thus the presuppositions with which Aristotle and Plato were working could not have given rise to science. Though Plato is highly abstract and theoretical he denied the possibility of the application of that knowledge to study the problem of change, for reasons already explicated. Aristotle on the other hand was constrained by his reliance on inductive generalizations, and also by his opposition to mathematizing the study of nature. Retrospectively we can claim that neither of them realized the potential of inverse reason, which not only made empirico-mathematical study of nature possible, but also resolved the epistemological tensions involved in studying the *invariant patterns of variance.* The most significant breakthrough took place in the school of Alexandria, where great scientific minds such as Euclid, Archimedes, Hero etc., attempted to resolve some of the problems in a characteristically *scientific* manner.

7.4 [Aristotle] and Archimedes

What is said so far about Aristotle's theory of motion might sound like an unsympathetic account. It should be kept in mind that insofar as the problem of motion is concerned it was only after the 17th century that we could go beyond the Peripatetics. However, this sad note is only to be heard in the case of a theory of motion. Soon after Aristotle, his students in the *Lyceum* and later in Alexandria developed models of explanation that synthesized rigorous mathematical techniques of Euclid (who also belonged to the school in Alexandria) on one hand and empirical, experimental, technological experience of craftsmen on the other. This can be achieved only by someone, like Archimedes, who assimilated the rigorous theoretical thinking of mathematicians and logicians, and an expertize in technical skills, measurement etc., necessary for conducting experiments. Archimedes was one such person, seldom can we find another of his kind.[27]

What was required for achieving the synthesis was something more than inductive generalizations and the taxonomy of phenomena. The synthesis was sought in the method of *idealization*, (a method better known to the Platonists) and *experimentation* to achieve an idealization in physical circumstances by isolating and controlling certain conditions. Both these as necessary characteristics inhere in physics as we know it today. Initial attempts in this direction took place in [Aristotle]'s treatise called *Problems of Mechanics*, to be developed later by Archimedes.

The work *Problems of Mechanics* was believed to be written by Aristotle, but from the views expressed by Aristotle elsewhere it is more likely that the work was composed by others in his school. Doubting Aristotle's authorship is plausible, because this work clearly shows an application of geometrical method to physical phenomena, against which Aristotle argued. In *Physics* (II 193b22 - 194a12) he says that the holders of the theory of the forms (the Platonists) are not justified in abstracting the objects of physics, "which are less separable [abstractable] than those of mathematics." He viewed branches such as optics, harmonics, and astronomy as converses of geometry. "While geometry investigates physical lines but *qua* physical, optics investigates mathematical lines, but *qua* physical, not *qua* mathematical."

This apocryphal work, *Problems of Mechanics* is generally taken to be the initial work on mechanics, for lack of most ancient records.[28] Since the work looks like a 'text' book it is most unlikely that it was produced in this 'finished' form without previous discussions in more primitive form on the subject. This work is also important from the point of view of a method which Greeks have developed, which is the method of reducing the unknown to the known. The method was used and developed by Euclid (§1.2 page 39) and Archimedes initially. Later this became one of the most profound methods of problem solving, which was to become popular as the method of analysis and synthesis.

Some of the questions raised in *Problems of Mechanics* by [Aristotle] are:

1. Why are larger balances more accurate than the smaller?

2. Why is it that the radius which extends further from the centre is displaced quicker than

[27]Pappus in *Mathematical Collection*, writing about mechanics comments about the intellectual equipment of Archimedes. He says that the science of mechanics consists of theoretical and a practical part. "The theoretical part includes geometry, arithmetic, astronomy, and physics, while the practical part consists of metal-working, architecture, carpentry, painting, and the manual activities connected with these arts. ... Now some say that Archimedes of Syracuse mastered the principles and the theory of all these branches. for he is the only man down to our time who brought a versatile genius and understanding to them all, as Geminus the mathematician tells us in his discussion of the relationship of the branches of mathematics." In Cohen and Drabkin 1958, *The Source Book of Greek Science* pp. 183-185.

[28]Cf. Dugas 1955, *A History of Mechanics* p. 19.

the smaller radius, when the near radius is moved by the same force?

3. Why is it that the exercise of little force raises great weights with the help of a lever, in spite of the added weight of the lever?

Let us discuss his answer to the third question first, which is a more composite case where the two former questions enter indirectly in the discussion. His answer begins by asking the question that suggests the analogy between lever and balance.

> Does the reason lie in the fact that the lever acts *like* the beam of a balance with the cord attached below and is divided into two unequal parts?

The explanation by analogy is further worked out as follows in continuation to the question:

> The fulcrum, then, takes the place of the cord, for both remain at rest and act as the center. Now since a longer radius moves more quickly than a shorter one under pressure of an equal weight; and since the lever requires three elements, viz., the fulcrum—corresponding to the cord of a balance and forming the center—and two weights, that exerted by the person using the lever and the weight which is to be moved; this being so, as *the weight moved is to the weight moving it, so inversely, is the length of the arm bearing the weight to the length of the arm nearer to the power*. The further one is from the fulcrum, the more easily will one raise the weight; the reason being that which has already been stated, namely, that a longer radius describes a larger circle. So with the exertion of the same force the motive weight will change its position more than the weight which it moves, because it is further from the fulcrum.[29]

The solution consists in many interrelated issues. First, the premiss that "the longer one moves more quickly than the shorter one", follows from an earlier proof that larger radii are displaced quicker than the smaller radii, or the points farther from the center of a circle are moved quicker by the same force and the larger radius is displaced quicker than the smaller. Since balance has three basic elements in its structure, the chord and the two weights that are isomorphic to the structure of the lever (the fulcrum and the weight to be moved and the power exerted on the arm of the lever), more or less direct analogy is possible. Once the analogy is achieved the relation obtainable is the principle of lever, as stated above (italicized portion). However simple it may appear to our intuition, more logical 'content' is required before realizing this analogy to the principle of lever.

Second, the analogy (isomorphism) between balance and circle has been established. The chord by which a balance is suspended acts as the center, for it is at rest, and the parts of the balance on either side form the radii.[30] This analogy makes the first question

[29] From Cohen and Drabkin 1948, *A Source Book in Greek Science* p. 193. Our italics.
[30] Cohen and Drabkin, *op.cit.* p. 191–192.

answerable. Since larger balance with larger arm moves more than a smaller balance with smaller arm given same weight, and since larger movement is more easily perceived than smaller movement, larger balances are more accurate (sensitive) than smaller ones.

Third, since the longer radius (the side of the lever where power is enforced) describes a larger circle, less power is needed than the weight to be moved, in the proportion of the lengths of the lever on either side of the fulcrum.

The method followed here is to reduce anything unknown to the properties of the circle, which are better known. As stated above, initially balance has been reduced to the circle, then the lever to balance. [Aristotle] writes:

> The properties of balance are related to those of the circle and the properties of the lever to those of the balance. Ultimately most of the motions in mechanics are related to the properties of a lever.[31]

The method of successive reduction has been applied later by Archimedes and Hero of Alexandria, who used the principles of lever to the five simple machines, the wheel and axle, lever, pullies, wedge, and screws.

What is the basis of the reductions from one to another? [Aristotle] thinks that it lies in the magical property of the circle.

> Someone who would not be able to move a load without a lever can displace it easily when he applies a lever to the weight. Now the root cause of all such phenomena is the circle. And this is natural, for it is in no way strange that something which is more remarkable, and the most remarkable fact is the combination of opposites with each other. A circle is made up of such opposites, for to begin with it is made up of something which moves and something which remains stationary.[32]

The cause of all phenomena is the circle because it is a combination of opposites. This reminds us of what Aristotle (of *Physics*) says about *what character should the principles have*?[33] In this connection what Aristotle says should be discussed in detail, for what he says provides a foundation for the development of our thesis too. Aristotle believes, just as other thinkers of ancient Greece, that the principles should be contraries. In *Physics* (Bk.I, Ch-V. 188a 19-26.) Aristotle elaborates.

[31] Quoted from *Problems of Mechanics* in Dugas 1955, *op.cit.*

[32] *Ibid*, p. 19.

[33] Here the term 'principle' is used in the sense of *basis*, as in the usage: "DNA is the material principle of inheritance." Principle is that from which everything else would follow, and should not be confused with the usage in which certain statements are regarded as principles. Though statements which are principles also have this connotation of basis, but they are not regarded 'causes' of all other things. There is a sense in which basic statements 'cause' other statements (consequences). Aristotle says that the antecedent and consequences are *like* causal relationship. Despite the analogy the difference however needs to be acknowledged.

> All thinkers then agree in making the contraries principles, both those who describe the All as one and unmoved ... and those too who use the rare and the dense. The same is true of Democritus also, with his plenum and void, both of which exist, he says, the one as being, the other as not-being. Again he speaks of differences in position, shape, and order, and these are genera of which the species are contraries, namely, of position, above and below, before and behind; of shape, angular and angle-less, straight and round.

Other outstanding examples of principles are odd and even, hot and cold, Love and Strife, moist and dry. Though all of them identify their principles with contraries, they however differ from one another, as we have seen in an earlier chapter, on what each one thought or chose as the principle in their theory. He says that unlike his predecessors he has, in addition to them, a reason to suppose that principles should be contraries. In continuation to the above passage he provides the reasons:

> It is plain then that they all in one way or another identify the contraries with the principles. And with good reason. For first principles must not be derived from one another nor from anything else, while everything has to be derived from them. But these conditions are fulfilled by the primary contraries, which are not derived from anything else because they are primary, nor from each other because they are contraries.[34]

Aristotle, therefore, thinks that everything that comes to be by a natural process is either a contrary or a product of contraries. For example, colors are regarded as intermediaries of the contraries black and white. If the principle is either (about) black alone or white alone, other colors cannot be caused from them, however if the principle contains black *and* white all colors could follow from them as different products of contraries.[35]

Thus the necessary conditions for calling something a principle for [Aristotle] and Aristotle, just as other Greek thinkers, is continuous in this presupposition that fundamental principles must be contraries.

The origin of this belief appears to be rooted in certain other basic presuppositions. As Aristotle himself says:

> Our first presupposition must be that in nature nothing acts on, or is acted on by, any other thing at random, nor may anything come from anything else, unless we mean that it does so in virtue of a concomitant attribute.[36]

This passage again is highly relevant for any attempt to study genealogy of scientific knowledge. The earlier part of the passage reflects a belief in *conservation*, and the latter part in

[34] *Ibid.*
[35] Ch.5, 188b 22-27.
[36] *Ibid*, 188a32-35.

coextension of attributes. Here Aristotle is suggesting clearly that if causal or conservational account is not obtainable, then concomitance of attributes should. The belief in the principle of conservation is vital for studying variational phenomena such as motion. For studying invariant things, at least apparently invariant, of different *kinds*, knowledge of concomitance of attributes is essential. The passage shows the in built tension between the study of the essences of individuals on one hand and the study of the 'essences' of invariance on the other. The tension exists because the former leads to proliferation of individuals, while the latter leads to unification of essences. One is the search for causal principles based on the principles of conservation, and the other is the search for taxonomic principles based on principles of coextension. This again indicates, on the methodological front, the tension between the method of taxonomy and the method of inversion. While the former investigations come out with knowledge related to variational regularities in the world, the latter form of knowledge comes out with compatibility of different 'things' stated as coextensional or coexistential regularities of the world. Aristotle must have noticed the problem, but we think that due to his opposition to mathematical techniques and his obsession with deductive logic he could not achieve any reconciliation.

Using the vocabulary of Aristotle, our central thesis can be stated thus: *The principles of nature should either be contraries, or be complexes of concomitant (compatible) attributes, where the former kind are obtainable by the method of inversion and the latter kind by the method of taxonomy.*

After [Aristotle] remarkable developments took place in the school of Alexandria. This school further developed the linkage between Aristotle's tendency to study phenomena at a local, as against, global level, and Plato's tendency to *idealize* phenomena to geometrical objects. We have seen in [Aristotle] how idealization of the balance, lever etc., have enabled him to apply geometrical principles and thus achieve reduction of the unkown to the known. That a marriage between the Platonic and Aristotelian approaches took place as far back as [Aristotle] and Archimedes is significant because the popular understanding suggests that it took place as late as 16th century in the hands of Galileo. Though Galileo also achieved a similar synthesis in the case of his studies of the phenomena of motion, in the case of statics and hydrostatics it took place soon after Aristotle.

The developments that took place in the school of Alexandria are significant on many fronts. One of them is the emergence of the problem solving approach. [Aristotle]'s work, let us recollect, was titled *Problems of Mechanics*, and its main objective is to find explanations for specific mechanical phenomena already mentioned. This problem oriented (paradigmatic) science can be traced to Aristotle's *Physics*, where several problems were posed and explanations offered at local level. It was in that context, we have seen, a large number of direct and inverse proportions stated between quantities, and most of them were inductively arrived. In the school of Alexandria some of the problems raised by Aristotle find ingenious solutions by means of the new methodology of mathematical physics. In this school we find evidences for the birth of the experimental method. It was in this school that Euclid composed the *Elements*, which became a symbol of Greek rationality. A generation or so later, Archimedes had shown a general method applying mathematical analysis to physics, and became the founder of mathematical physics. He has many mathematical discoveries to his credit, such as the method of exhaustion, which is in essence same as that of the method of integration of modern calculus.

It is not merely to provide a list of all the great inventions and discoveries that took place in the school of Alexandria that these words of acclaim are included. Better accounts can be found in books on history of science. Our interest here is to show that all these developments took place only after [Aristotle], who as already elaborated, gave a clear direction to future investigations. Here we shall briefly describe how Archimedes's contributions also show the general features of scientific principles, which are inversely modeled. It is significant to note that in this initial period when science was in the making, all the inverse models were equilibrium models.

In his treatise *On the Equilibrium of Planes* Archimedes develops mainly two issues: the principle of lever and the center of gravity. To illustrate our point the first three postulates of the *principle of the lever* will be discussed briefly.

> 1. *Equal* weights at equal distances are in *equilibrium*, and equal weights at *unequal* distances are not in equilibrium but incline towards weight which is at the greater distance.
> 2. If, when weights at certain distances are in equilibrium, something be *added* to one of the weights, they are not in equilibrium, but incline toward that weight to which the addition was made.
> 3. Similarly, if anything be *taken away* from one of the weights, they are not in equilibrium but incline towards the weight from which nothing was taken.[37]

These three postulates represent a model of *equilibrium*, which is nothing but an ideal case of inversely ordered model. It is needless to say that equilibrium presupposes inverse relation. The first postulate states the condition of equilibrium, and disequilibrium, in other words, order and disorder. Since most phenomena that we perceive are presumed to be of the latter kind, the task is to put forth the conditions of the 'apparent' disorder. The second and third

[37]T.L. Heath 1897, *The Works of Archimedes,* p. 189.

postulates state how the equilibrium can be disturbed, either by adding or by taking away some weight, which are inverse operations. The effects are the same which ever way we do it, because the change is explained by relativizing one with the other and not in absolute terms. It is another distinguishing feature of science that its knowledge is based on principles of relativity, which can not be stated without presupposing a model of equilibrium, or a group of transformations inversely ordered.

Another instance where Archimedes applies the similar model of equilibrium, is in the case of floating bodies. We are aware of the famous story of Archimedes' solution of the problem of Heiro's golden crown. This problem is solved by understanding that different substances have different specific gravities (relative densities) by which each substance can be identified.

Two masses of the *same weight* [mass] one of gold another of silver do not pour out the same amount of water when immersed in a jar filled to the very brim. Gold displaces a smaller quantity than silver of the same weight. The difference is due to specific gravity, which is characteristic of each substance. This is another instance of a conception that developed as a result of relative, and not absolute, 'weightage' of different substances in the same medium. This is also a paradigm instance of experimentation where certain conditions like medium, weight etc., are held constant to understand 'specific' differences between substances. The 'real difference' of things, as characteristic features, can be understood in relation to others and not in absolute terms is an important heuristic principle of science. The objectivity claimed in science has always been relativized objectivity, never absolute.

A difference between Archimedes and [Aristotle] that needs to be pointed out is that the latter's analysis of the problem was in terms of the general laws of motion, while the former's analysis is based on static principles like equilibrium and the center of gravity. This Archimedes achieves by eliminating factors like speed and the time taken while studying the phenomena. That these factors throw no extra light on the problem and should therefore be set aside, is an important methodological principle necessary for mathematization.

A few more observations are in order before we close this chapter on genesis of scientific knowledge. It was known from common experience that with a lever, only a *small force* has to be applied on the *long arm* in order to support a heavy weight on the *short arm* or to move a *big load*. Thus the inverse correlation stated in this form is *inductively* obtained between the length of the arm and force applied. But the solution consists in relating one side with the other side in a symmetrical manner. This means that if one were to lift a small load attached to the long arm of lever by applying force on the short arm of the lever, the force required would be more than the load. Technically we say that the mechanical advantage would be less than one. Though no one would in actual life, unless for experimental reasons, like to lift loads using this other 'side' of the inductively obtained principle, it is highly relevant to suppose this *counter-inductive* side of the principle to achieve the 'balance' required for stating an equation.[38] Though it is known to us it is necessary to recollect that one of the needs for an experiment arises only when actual experience is insufficient.

It is clear from the above account that the models of explanations so far obtained are obtained from abstracting from our experience with instruments like balances. Models of explanations once abstracted from the original context can find applications in contexts other than the original, as mentioned already. This point is rather well understood and is often discussed as a characteristic feature of any abstractions like concepts. But unlike ordinary concepts, such as color concepts, where once understood the application of it becomes trivial, every new application of constructive concepts is a discovery. When we find yet another instant of red color, we don't shout with insight and excitement 'Eureka!', because the concept is directly linked to its instances. In the case of models, which we see are complex conceptual structures, this is not the case. *Application* of one model to another phenomenon is a case of *discovery*. It is discovery because the analogy between the model and the phenomenon will become clear only after the interpretation of the phenomenon in terms of the elements of the model is well understood. Which element of the phenomenon corresponds to which element of the models can be grasped only if the analysis of the phenomena can be obtained in similar terms as that of the model, which is essential for establishing an isomorphism between phenomenon and the model.

[38] We are using the expression 'counter-inductive' in the specific sense mentioned above, which is to see the consequence of the converse of inductively arrived inverse correlation. We are not clear whether this sense has any relationship with Feyerabend's use of the term. Cf. his 1978, *Against Method: An Out line of an Anarchistic Theory of Knowledge*.

Chapter 8

A Study of Galileo's *De Motu*

It is believed that the work, *De Motu*, which was not published by Galileo was composed during the time that Galileo taught at the University of Pisa between 1589 and 1592. The manuscript contains an essay version and a Dialogue version, as well as a series of brief notes on the subject.[1]

Due to the importance of the work for understanding the evolution of Galileo's thought we shall discuss most of the material in detail mainly to show that his reasoning is a classic example of inverse reason. Despite differences with Aristotle he demonstrates the possibility of arriving at the principles (causes) of nature that are 'identical' to statements of contraries. Galileo has been regarded as the central figure in shattering the conceptual scheme of Aristotle by proposing an alternative scheme. This study also demonstrates the point that this conceptual transformation would not have taken place without inverse reason. It is very important to say this here before we proceed further, that the set of problems they are attending to are similar, though their solutions are radically different.

8.1 The Cause of Motion

In the first chapter Galileo clarifies (defines) the terms 'heavier' and 'lighter' in a characteristically anti-Aristotelian way. We will be discussing in greater detail the notions of heavier and lighter in another section (8.5), to contrast the thinking pattern of Aristotle and Galileo. The vantage point from which this is done becomes clear in the first few chapters of *De Motu*, though greater understanding will be achieved as we go further into the details of other chapters. These terms are going to become the contraries of the principle (cause) of

[1] We are following Drabkin's (1960) translation of the manuscripts edited and published by Antonio Favaro in 1890. Cf. Drabkin's introduction, p. 3.

natural motion, and are therefore immediately relevant.

> [W]e sometime say that a large piece of wood is heavier than a small piece of lead, though lead, as such, is heavier than wood. And we say that a large piece of lead is heavier than a small piece of lead, though lead is not heavier than lead.

These words could lead to confusion if it is not clear as to what is being said and from *what point of view*. Galileo, therefore, thinks it is "necessary to settle this," and "avoid pitfalls of this kind". Can there be a way of saying that something is heavier or lighter absolutely? Galileo's answer is 'no'. Heaviness and lightness can however be defined relatively. It is important to note that he in fact defines three notions, and not just two, viz., "equally heavy", "heavier" and "lighter" in the following way. As already stated it is a necessary condition that any inverse order must contain at least three terms.

(1) Equally heavy: "[T]wo substances which, when they are *equal in size* [i.e., in volume], are also equal in weight."[2]

(2) Heavier: "[O]ne substance should be called heavier than a second substance, if a piece of the first, *equal in volume* to a piece of the second, found to weigh more than the second."[3]

(3) Lighter: "[O]ne substance is to be considered lighter than a second substance, if a portion of the first, equal in volume to a portion of the second, is found to weigh less than the second."[4]

The similarity of these statements with that of Archimedes, stated in the earlier chapter (§7.4 page 236), can be readily seen. The first one in both cases is regarding the state of equilibrium, and the other two are about the two possible variations of disequilibrium. The three statements actually constitute one single *principle*, by means of which all effects are to be explained with regard to differences in heaviness or lightness. This structure of a principle of science allows one to explain invariance as a result of equilibrium, and variance as a result of disequilibrium of opposites. We shall see that most crucial changes from Aristotle's to Galileo's concepts take place due to the sort of principles that are inversely structured.

In the above definitions, heaviness has been defined in terms of weight. These definitions therefore also make a conceptual distinction between weight and heaviness, which later becomes the distinction between weight and mass.

[2]p. 10. All page references are to the Galileo's text *De Motu*, unless otherwise specified.
[3]p. 14.
[4]p. 14.

The most significant point to note is the method of selectively controlling one of the parameters to understand the proportional relations between them. Here the volume of the substances under comparison are controlled in the sense that they are kept constant in all the cases. What is the methodological necessity of this? Since a change in volume effects a corresponding change in heaviness, unless volume is kept constant no relative [objective] comparison is possible. In common comparisons, since we do not bring in a *third* factor into consideration the resulting judgements can be confusing.

So far we see two distinct influences on Galileo, one, that of the Atomists' understanding of density, and two, of Archimedes' principle of equilibrium. In the second chapter he comments that Aristotle wrongly criticized the Atomists in *De Caelo*, Book IV. He approvingly says of the latter that if there is a single kind of matter in all bodies, and heavier and lighter bodies differ in the amount of matter in a given space, then (presuming that the earth is spherical), since spaces in a sphere become narrower as we approach the center and larger as we recede from the center, it is natural that earth (the element) would occupy the center and other three elements, water, air and fire would occupy successively farther from the center in that order. "It was therefore with prudence and justice," with "complete justice and with consummate wisdom" that nature determined respective places for the four elements in proportion as the matter of each of those elements was *rarer* or *denser* and not as Aristotle thought that different *kinds* of bodies will have different *tendencies*.

He thus arrives at a single principle of explanation for all kinds of bodies. In so far as considerations of parsimony do enter in the organization of scientific knowledge, Galileo is surely more scientific than Aristotle.

He 'recedes' further from Aristotle—the centre of learning—in his reworkings of Chapter 2, version II. The more categorical terms "heavier" and "lighter" are less preferred to relativized terms "heavier" and "less heavy" on the ground that nothing is devoid of weight. Later in Chapter 12, he says that even fire, which is *less heavy* than all "will move downward if air is removed under it, that is, if a void or some other medium lighter than fire is left under it." The other pair of categorical terms "downward" and "upward" have also been relativized to "nearer the center" and "farther from the center". Thus in Chapter 2, of version II he says:

> Up to now we have spoken of "the heavy and the less heavy" not of "the heavy and the light"; and of "nearer the center and farther from the center," not of "downward and upward." ... Yet, if at times, out of a desire to use ordinary language (for quibbling about words has no relevance to our purpose), we speak of "the heavy and the light," and of "downward and upward," these expressions should be understood as meaning "more and less heavy" and "nearer the center

and farther from the center.

Having considered the natural order of the *arrangement* of bodies, Galileo considers the question of what causes *natural* motion in Chapter 3. Both upward and downward motions (i.e., going farther from the center and coming nearer to the center) are caused, according to Galileo, by considerations of relative heaviness or lightness (lesser heaviness). Any change contrary to the arrangement of nature causes motion. A main consideration that goes into the argument is that the motion of bodies should not be studied independently of the *medium* in which it takes place. Should this mean that there is no motion if there is no medium? Surely not for Galileo, but for Aristotle 'yes'. We shall shortly see that his position is that though the heaviness of bodies is relative to the media, the actual heaviness can be measured only in vacuum. The information that a body is heavier is not sufficient to determine the character of its motion. The further information that at *which place*, in *which medium*, and whether the medium is heavier or less heavy than the body, is also necessary.

Later he goes on to prove three principles of hydrostatics: [1] that bodies of equal heaviness as the medium move neither upward nor downward - a state of equilibrium; [2] that bodies lighter than the medium do not sink in it, and cannot be submerged totally, but move upwards; [3] that bodies heavier than the medium get totally submerged and move downwards. These principles are the same in *structure* as that of the propositions 3, 4, and 7 stated by Archimedes in *On Floating Bodies*[5], except that Archimedes does not concern himself with the movement of bodies upwards or downwards, for his concerns are purely with statics and not kinematics or dynamics. In proposition 7, however, Archimedes speaks of the *descending* of the heavier body. The structure of the statements stating the principles shows beyond doubt that his discoveries are based on working in an idealized space created by inverse order. Galileo frequently repeats statements that are similar in structure to the principles of statics.

Galileo being interested in solving the problem of natural motion by using the model developed by Archimedes, makes it a special point to use the terms denoting movements of bodies in media. As noted earlier Archimedes systematically avoids any kinematic concerns unlike [Aristotle].

[5]Proposition 3: Of solids those which, size for size, are of *equal* weight with a fluid will, if let down into the fluid, be immersed so that they do not project above the surface but do not sink lower. ...

Proposition 4: A solid lighter than a fluid will, if immersed in it, not be completely submerged, but part of it will project above the surface. ...

Proposition 7: A solid heavier than a fluid will, if placed in it, *descend* to the bottom of the fluid, and the solid will, when weighed in the fluid, be lighter than its true weight by the weight of the fluid displaced. Quoted, without proofs, from T.L. Heath (1897) *op.cit.* pages 255, 256, and 258 respectively.

In this respect Galileo's return to kinematic questions keeping in mind the model of statics, which has achieved sufficient abstraction such that the mathematization of physical phenomena would be possible. Despite its originality we shall regard it as a *development* because we perceive it as the finding of new applications (discoveries) of the model already invented or constructed by Archimedes. The Alexandrian school had not merely achieved remarkable strides in mathematics, they had also used the experimental method. Whoever would build models must ultimately come 'down' to controlled experiments to realize a 'world' constructed in thought. The general claim that it was only Galileo et.al., around the sixteenth century, who mathematized natural science, can not be regarded as historically true, because Archimedes has already achieved the objective in principle. This is however not to suggest that Galileo's contributions are unreasonably overestimated. What is being said is that the 'seed' was already in the "air", and not in the "earth", and the greatness of Galileo lies in his ability to have picked it up and planted in a 'rich soil' such that it started developing. The 'rich soil' corresponds to the philosophical, mathematical, experimental, reasoning that nourished and supported it for further development.

Before going further on the subject, Galileo explains the analogy between the case of a balance and the case of bodies moving naturally, by reducing the latter to the former. Though he has been a student of and occupied a chair in mathematics, he has a desire to make matters clear by conveying the message through physical analogies. His acute concern for communication has very few parallels in intellectual history. His objective is "a richer comprehension of the matters under discussion, and a more precise understanding on the part of [his] readers" and he therefore restrains himself from using mathematical elucidation.

Coming to the analogy, he says in Chapter 6, whatever happens in a balance also happens in the case of bodies moving naturally.

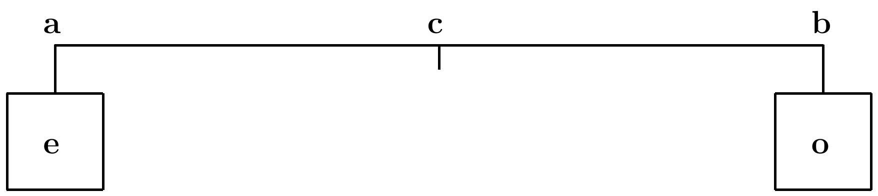


Figure 8.1: Structure of Balance

Let ab represent a balance, as shown in the figure 8.1, and c its center bisecting ab; and let e and o be weights suspended from points a and b.

> Now in the case of weight e there are three possibilities: it may either be at rest, or move upward, or move downward. Thus if weight e is heavier than weight o,

> then e will move downward. But if e is less heavy, it will, of course, move upward, and not because it does not have weight, but because the weight of o is greater. From this it is clear that, in the case of the balance, *motion upward as well as motion downward takes place because of weight*, but in a different way.

This is indeed a breakthrough, in the sense that it differs drastically from the Aristotelian order of things. For Aristotle, motion upward takes place because of two reasons: one by force, and otherwise naturally (i.e., without force) if the bodies are lighter or have no weight like fire. Bodies go downwards because of weight, or if the body is by nature light, then by force. For Galileo, having relativized the notion of heaviness and lightness, both take place due to the same cause, namely, heaviness. Thus the analogy of the balance, which is an idealized symbol of inverse order, systematically helps Galileo to break conceptually with Aristotle. However, as he qualifies in the end, there is a difference in the way in which weight causes motion in both cases. The difference lies in a careful distinction between *internal* and *external* weight.

> For motion upward will occur for e on account of weight of o, but motion downward on account of *its own weight*.

This remarkable distinction, we think, must have become crucial for the development of both the conceptions of *inertia* as an 'internal' cause, and *force* as an 'external' cause, without which classical mechanics is inconceivable. This distinction would not have been possible without the analogy of balance because the analogy *constrains* us to think of only one other inverse factor that is affecting the motion, and not any of the numerous other logically possible factors. Since the body that is moving upwards and the weight that is causing are joined together by the lever, and because it is joined, the most immediate cause must be just the other weight that is external, but joined, to the body. This example clearly shows how a *physical system* can be obtained by inverse reason. If the joint is cut, not only the heavy body, but also the lighter body would move downwards, due to their own weights. Therefore downward natural motion must be because of its own weight.

Galileo did not arrive at the conclusion all at once. It took time because he started with natural motion, and now he has to term it, so called, natural motion. He must have achieved this break while analyzing the consequences carefully from the analogy. The analysis in the initial version goes on as follows.

Continuing his considerations of balance he enunciates the general proposition that the heavier cannot be raised by the less heavy. This follows from the principles of balance. If water is less heavy than wood then wood cannot float on water. And, most importantly, wood goes up above the state of equilibrium because, water as a weight on the other side of the balance, by analogy, is *lifting* or *raising* the piece of wood.

> It is therefore clear that the motion of bodies moving naturally can be suitably reduced to the motion of weights in a balance. That is, the body moving naturally plays the role of one weight in the balance, and a *volume of the medium equal to the volume of the moving body* represents the other weight in the balance.

Thus the other weight is not of the entire water, but only that portion of it which is equivalent to the volume of the moving body. The motion therefore is caused by force, especially the upward motion.

We have seen above that Galileo presents principles that are isomorphic to those of statics. Another illustration is as follows:

> [2] [I]f a volume of the medium equal to the volume of the moving body is heavier than the moving body, and the moving body lighter, then the latter, being the lighter weight, will move up. [3] But if the moving body is heavier than the same volume of the medium, then, being the heavier weight, it will move down. [1] And if, finally, the said volume of the medium has a weight equal to that of the moving body, the latter will move neither up nor down, just as the weights in the balance, when they are equal to each other, neither fall nor rise.[6]

In the reworking of this part Galileo reaches the unambiguous conclusion that no upward motion is natural, i.e., it must be *forced*. Since all bodies have weight they all have an internal cause, which is nothing but the weight (relative) for downward motion.[7] In the memorandum Galileo adds:

> Downward motion is far more natural than upward. For upward motion depends entirely on the heaviness of the medium, which confers on the moving body an *accidental lightness*; but downward motion is caused by the *intrinsic heaviness* of the moving body. In the absence of a medium everything will move downward. Upward motion is caused by the extruding action of a heavy medium. Just as, in the case of a balance, the lighter weight is forcibly moved upward by the heavier, so the moving body is forcibly pushed upward by the heavier medium.[8]

That all upward motion is forced is a significant move away, if not against, Aristotle. In order that one arrives at this statement we need to have rejected that some bodies levitate and some gravitate *by nature*. In Aristotle's scheme of things, if there existed only one element, say fire, in the universe, it would have occupied that layer which is in proximity to the lunar sphere, while in Galileo's scheme of things it would reach the center of the universe, for fire

[6] The numbering is included to show the isomorphism with the earlier statements with the same number.
[7] p. 177.
[8] Notes 4 on p. 22.

is, in this hypothetical case, the heaviest. That every body (every element) has weight and that weight is relative are some of the initial steps Galileo takes away from Aristotle's thesis. Taking clues from the kind of motion that takes place in the case of balance he goes another step forward, yet another step away from Aristotle, by proposing that *all upward motion is forced*. In order to reject Aristotle's thesis that wood in water rises up naturally, and propose that in such a case the body (in this case wood) is being lifted up by another *weight* external to the body, the analogy with the balance is crucial. Here lies the genius of Galileo. This is no ordinary achievement, despite the simple logic.

The crucial contribution does not consist in saying that bodies with weight would naturally move downward, for this was also the thesis of Aristotle. It also does not consist in saying that lighter bodies go up because of force. Neither Galileo nor Aristotle would say this, because for Galileo there is nothing like a light body, but only less heavy, and for Aristotle not all upward motion is by force, it is by force only for heavier bodies. The contribution consists in proposing that in the case of *all bodies*, irrespective of their heaviness, if they move upwards the motion is forced, and the force is external to the body. Our concern here is not to see whether what Galileo says is true or not, but to understand how the conceptions are transforming.

Galileo continues his journey, being convinced that his path is right.

> And since the comparison of bodies in natural motion and weights on a balance is a very appropriate one, we shall demonstrate this parallelism throughout the whole ensuing discussion of natural motion. Surely this will contribute not a little to the understanding of the matter.[9]

8.2 The Cause of Change in Motion

Galileo has so far postulated the cause of 'natural' motion, which is heaviness or relative density for both upward and downward motion. If the cause of motion is heaviness then what would be the cause of change in motion? Since a change in cause should produce a corresponding change in effect, a difference in heaviness should produce a difference or change in motion.

Can there be *kinds* of change in motion, such as slowness and speed? Accordingly should we need to postulate two separate causes, one for slowness of motion, and one for swiftness of motion? For Aristotle slowness has one cause, namely, density of the medium and swiftness has another, namely, rarity of the medium. Galileo, on the other hand, argued

[9] p. 23.

for one cause for both slowness and speed, just as he argued for one cause for both upward and downward motions. This unification is a necessary move for what he would be finally driving at, which is one cause for motion as well as for change in motion. This ultimate unification is one of the revolutionary contributions of Galileo, which helped in the development of classical mechanics.

What Aristotle says is that a body would be faster in air than in water, because the former is more incorporeal (less dense) than the latter. (Note that Aristotle did describe in relative statements. But he restricts relative description, as stated in the above chapter, to the two media, water and air. His statements with respect to earth and fire are absolute. See further discussion in 8.5 page 264) He also says that density of the medium impedes the movement of a body.

> We see the same weight or body moving faster than another for two reasons, either because there is a difference in what it moves through, as between water, air, and earth, or because, other things being equal, the moving body differs from the other owing to excess of weight or of lightness.
>
> Now the medium causes a difference because it impedes the moving thing, most of all if it is moving in the opposite direction, but in a secondary degree even if it is at rest; and especially a medium that is not easily divided, i.e. a medium that is somewhat dense.[10]

From this it is clear that Aristotle believed in a twofold cause to the motion of the body, one external to the body in the form of resistance of the medium, and other internal in terms of the weight of the body. One of them (weight) to be accounted for the speed and the other (density of the medium) for the slowness of the moving body. Galileo differs from him in a very subtle but significant way.

Galileo says that both downward motion in the rarer media and upward motion in denser media would be swifter, and upward motion in the rarer media and downward motion in denser media would be slower. These descriptions, one can easily see, are transformations obtained by appropriate changes of the opposite terms.

From the above arguments it follows that density of the medium does not always decrease motion, because upward motion in denser media is swifter. Similarly rareness of the medium causes swifter motion only in the downward direction and not in the upward direction. Therefore the view of Aristotle that slowness of natural motion is due to the density of the medium is incorrect because certain things such as an inflated bladder, which when left in deep water (or any other denser medium), moves up swiftly. *In a place where*

[10] *Physics* 215a25-31.

downward motion takes place with difficulty, an upward motion necessarily takes place with ease: a canonical statement of inverse reasoning.[11]

> Therefore, dismissing his [Aristotle's] opinion, so that we may adduce the true cause of slowness and speed of motion, we must point out that *speed cannot be separated from motion.* For whoever asserts motion necessarily asserts speed; and slowness is nothing but lesser speed. Speed therefore proceeds from the same [cause] from which motion proceeds. And since motion proceeds from heaviness and lightness, speed or slowness must necessarily proceed from the same source. That is, from the greater heaviness of the moving body here results a greater speed of the motion, namely, downward motion, which comes about from the heaviness of that body; and from a lesser heaviness [of the body], a slowness of that *same motion*. On the other hand, from a greater lightness of the moving body will result a greater speed in that motion which comes about from the lightness of the body, namely, upward motion.[12]

This is the method of unifying the causes that Galileo consistently, and (there is evidence to show that he) consciously, adopts in solving problems of physics.

Compare the pattern of reasoning that leads him to infer that *lightness is nothing but less heavy and heaviness is a character of all bodies*, with what he says here in the case of motion and change of motion and the causes of motion and change of motion. Substituting 'lightness' with 'slowness' and 'less heavy' with 'lesser speed', 'heaviness' with 'speed' and 'bodies' with 'motion', the statement underlined above reads: *Slowness is nothing but lesser speed and speed is a character of all motion.* This is a typically Galilean method of solving the problem.

Though it falls short of finality, the remarkably Galilean turn, necessary for the emergence of classical mechanics, takes place here. This has been made possible by a specific pattern of thinking in terms of contraries. This pattern has an added advantage over, and is not accessible to, the taxonomic way of thinking. The advantage is already exemplified above in the previous section in finding the cause of natural motion. Here we have another instance. The main point that is emerging again and again, which will continue later too, is that the contraries cannot become two separate qualitatively or quantitatively distinct categories, but belong to one scale. This point is entirely missing in Aristotle, despite the fact that he shows awareness and inclination towards principles characterized by contraries as mentioned before. He did not make the crucial move, the Galilean move, because he could not develop his thinking on the basis of certain *logical inferences based on the converse relation*. We shall elaborate.

[11] p.24
[12] pp. 24-25

Aristotle realizes, as Galileo does, that media as well as weight affect the motion of bodies. Galileo would not deny that corporeal (dense) nature of the media would resist motion, but he would not accept it for all kinds of motion, because of the reasons mentioned above. It may appear as though Galileo is introducing a new taxonomy unknown to Aristotle. Was Aristotle wrong in taxonomizing kinds of motion? It appears not. Galileo does not disagree with Aristotle on this point. He, however, disagrees with him on the corresponding taxonomizing of the kinds of causes of motion. That is causes need not follow the same taxonomic pattern of effects, and this for the very important reason that *one cause is sufficient to explain (by generating) all varieties and effects of (natural) motion*. This is the crucial aspect of the kind of systematization that is achieved by a cause that is inversely structured. Suppose for every kind of effect there should exist a corresponding cause, then there should be as many causes as there are effects. The simplicity and systematicity of causal explanation, however, consists in *reducing* a large number of effects to a single unifying causal principle. Galileo achieves this simplicity by adopting a pattern of thinking guided by the inverse relation. Our thesis in this connection is that Aristotle has not achieved what Galileo achieved with regard to the problem of motion because Aristotle has not 'understood' the potential of the inverse relation.

If a medium is dense it would not continue to be so in relation to every media. Water may be more dense than air, but it is less dense or rarer than sea water or milk. This possibility suggests that something is dense or rarer in relation to, and only in relation to, another thing. Surely it does not appear like a major clarification of the matter. But however simple it may appear, Aristotle could not appreciate the point. Even if he was aware of the point (surely Aristotle the logician must have been aware), he has not clearly made use of the consequences of this realization while solving the problem of motion.

Secondly, one can easily see that the greater the density, the lesser the rarity and conversely. That the realization of this sort of inverse relationship is crucial for the development of scientific knowledge appears on the face of it a very silly point. But understanding the meaning is one thing, and realizing the deeper consequences, another. Semantically (i.e., intensionally) everybody would understand the mentioned inverse relationship between density and rarity. But one can go beyond this mere understanding when one realizes that extensionally it is sufficient to talk in terms of either density or rarity, because they are interdefinable. Talking in terms of both, extensionally speaking, does not add any more information than what is said with one alone.

Consider, for example, the extension of all things that are dense, and let that be an ordered class of things according to the degree of density. Let there be another such class of things except this time ordered according to the degree of rarity. Extensionally we would have obtained the same class of things except in inverse order. Not so for Aristotle. He believed that the *elementa extrema*, which are earth and fire, are absolutely heavy and absolutely light. Earth and fire are the moving bodies with opposite directions in their natural motion, and they move in the two *elementa media*, water and air. He also believed that basic elements will have their absolute value of weight in their own places (natural places), which are the corresponding spheres.[13] Since Aristotle believed that there exists a natural compartmentalization between things, he stuck to his taxonomic order,[14] Galileo on the other hand realized that there is no extensional difference, except in *the order of things which is inverse*. Therefore, which ever way one would speak there would not in principle be any difference. They are for him two ways of saying the same thing.

Though Aristotle uses each expression in terms of being more or less one quality, such as "more incorporeal", still he does not allow that the less of one would mean more of another, i.e., less of the corporeal would mean more of the incorporeal. This appears to us one of the major setbacks of Aristotle's intellectual achievement. Galileo, however, relativizes all the opposites with regard to motion systematically, and in a remarkable sense goes ahead of Aristotle. For example, let us look at the passage of Aristotle from *Physics* quoted on page 247.

Here Aristotle, like Galileo, correctly identifies the possible candidates of causes of motion, namely, first, the difference in media, i.e., whether the media are denser or rarer; second, excess of weight or lightness. However the two reasons become one in Galileo because there exists a relation of direct proportionality between density and weight or rarity and lightness for a unit volume of any body or medium, leading him to the definition of specific weight. It is important to note that the definition is not arbitrary, but is a systematic composition made by carefully following the proportionality relations between the quantities of the bodies. The consequences of speaking in terms of specific weight go a long way firstly, because the notion has rich information content, which is due to the proportional relations between weight, density and volume of bodies. Secondly, it is a property that can be applied to *both* the moving body and the medium, making it possible to use the structure (analogy)

[13] *De Caelo* Book IV.

[14] Inverse order is also possible across compartments (classes), but in such cases different mathematical operations should be called for. In such cases mathematical composition and division would be in terms of multiplication and division. I.e., in such cases geometrical proportionality would be applicable, while in those cases where inverse order is among the objects of the same extension, then the mathematical relationship obtainable and applicable would be arithmetic proportionality. This point is fundamental to the methodological thesis proposed.

of balance. As a result, both the body and the medium are analyzed by the same method. Thirdly, as a consequence of the above, the application of the principles of statics, and then, hydrostatics becomes possible.

Once the analogy with statics and hydrostatics is accomplished very useful propositions can be obtained due to the inverse structure's generative power. For example, if a body moves downwards in a medium with 'difficulty', i.e., by the application of force, as in the case of wood in water, the same body in the same medium would move 'easily' upward. And conversely, if a body moves downwards with 'ease', like stone left free in the air, the same body in the same medium would move up only if external force is applied. If downward motion is known to be swifter, then its upward motion will be slower. This is the structure of a large number of passages in different contexts in the text of *De Motu*, indicating his thinking pattern. This according to our understanding is one of the major differences between Aristotle's and Galileo's pattern of reasoning, and also that the latter's achievement consists precisely in applying this reasoning. The following passage throws more light on the generative potential of inverse reasoning.

> The body moves downward more swiftly in that medium in which it is heavier, than in another in which it is less heavy; and it moves upward more swiftly in that medium in which it is lighter, than in another in which it is less light. Hence it is clear that if we find in what media a given body is heavier, we shall have found media in which it will fall more swiftly. And if, furthermore, we can show how much heavier that same body is in this medium than in that, we shall have shown how much more swiftly it will move downward in this medium than in that. Conversely, in considering lightness, when we find a medium in which a given body will be lighter, we shall have found a medium in which it will rise more swiftly; and if we find how much lighter the given body is in this medium than in that, we shall also have found how much more swiftly the body will rise in this medium than in that.

The passage also shows why and how inverse structures can make *mathematization* possible. In order to calculate unknown quantity from known quantity we need in precisely what terms the quantity varies with the other. Galileo attends to the problem of finding precise quantitative relations between the relevant parameters so that it is possible to ascertain in what ratio the speed of a body varies in different media. But before we go into that an important point requires special mention, which is regarding the choice of heaviness and not lightness as the cause of motion.

We have seen above that whether one speaks in terms of lightness or in terms of heaviness, it makes no difference. However Galileo realizes that lighter means only less heavy or less dense, and hence he chooses to speak only in terms of greater and less heaviness. But why heaviness, why not lightness? One major reason for choosing heaviness as the cause of motion is because Galileo believed that all bodies, including fire, have weight.[15] However there are other epistemological reasons for this choice: that is, heaviness is given more *directly* to experience and experiment than lightness.

In principle, it is indeed possible to give a converse account by choosing the other possibility which would amount to speaking in terms of greater and lesser lightness. Operationally or logically, though not epistemologically, it makes no difference. The advantage of speaking in terms of heaviness is that determining heaviness is more *direct* than determining lightness, which is only a matter of practical consideration. It is easier for us to have standards of measurement that fall on the heavier side of the spectrum. Though investigations typically begin by 'fixing' the more familiar and feasible side of the spectrum, later attempts to make finer and finer instruments involves solving inverse problems, where in problems would be worked out from the other side of the spectrum, for achieving greater certainty. It is well known that indirect measurement gives finer results than direct measurement. It will not be possible for us to get into this very important and relevant epistemological problem in this essay, except for indicating that it suggests an interesting relationship between the 'factual' and the 'theoretical'.

It may however help us to grasp the indicated sense intuitively from another context which we understand relatively better. Given natural numbers—which may be considered *direct*—and *direct operators*—like addition and multiplication—to begin with, it is possible to construct symmetrical systems of numbers, like integers, rationals, reals, and complex numbers by inventing inverse elements and inverse operators. The objective of the mathematician in this case is to make all kinds of algebraic equations solvable. Here too there is no logical necessity to start this constructive activity only from natural numbers; one might as well start from a set of negative numbers, and the inverse operators subtraction and division and from them construct the so called direct numbers and operators. However for at least human beings about which we have a better knowledge, it is known to be easier to start with what we call directly given 'things', and then indirectly obtain the inverses. Since it is *sufficient* to choose either of them as fundamental, we can as well choose that which is familiar and feasible to our experience and experiment.

[15]See Ch. 12.

8.3 The Ratio of Speed

Already Galileo has laid a secure foundation for the construction of an alternative interpretative framework to Aristotle's, for the investigation of the problems of motion. Some of the essential aspects of the peculiarly Galilean pattern of thinking have been illustrated, and the role of the inverse relation in binding the structure of thinking highlighted. However many specific and deeply held beliefs of Aristotelian science need to be *demolished* before erecting the alternative structure. Galileo's attempt we believe was to construct at the *same location*. It has been held by many authors, such as Kuhn, that the two *paradigms* have two corresponding and independent 'worlds' of their own. We see the possibility of a contrary argument that they are alternatively constructed views of the same world and therefore to be constructed at the same location.

In the eighth and ninth chapters Galileo advances a large number of arguments, most of them in the form of what we today call thought (*gedanken*) experiments, against Aristotle's thesis that there is a direct proportionality between largeness (greater weight) of a body and its speed in natural motion. The structure of the arguments further strengthens our thesis that Galileo's reasoning is a classic case of inverse reasoning.

Aristotle's view as presented in *De Caelo*[16] is as follows:

> A given weight moves a given distance in a given time; a weight which is as great and more moves the same distance in a less time, the times being in inverse proportion to the weights. For instance, if one weight is twice another, it will take half as long over a given movement.

Similar statements asserting that larger and/or heavier bodies move quicker have been made in *De Caelo* 290a1-2; 277b4-5; 309b11-15; 394a13-15; and in *Physics* 216a13-16. The law as stated is also believed to be true of the weightless element fire. He says in *De Caelo*:[17]

> The greater the mass of fire or earth the quicker always is its movement towards its own place.

It is clear from these statements that ratios of the speeds of their motion downwards for earth, and upwards for fire, is proportional to the sizes of the bodies. Since Aristotle had no notion of *mass*, we interpret the term 'mass' as 'massive' or 'larger' or 'voluminous'. We have already seen how Aristotle's usage was ambiguous and confusing because of the lack of a clear distinction between mass (or heaviness in Galileo's sense) and weight.

[16] 273b30-274a2
[17] 277b4-5

Critical of Aristotle's view, Galileo says that his views are ridiculous and that they are ridiculous is "clearer than day light". But Galileo is wrong to say this, because the mistake of Aristotle is pretty involved. The process of moving from what appears to immediate perception to what holds true after meticulous and involved constructive reasoning, is not very easy. However Galileo may be right if one were to say that if one looks at the phenomenon from the framework that Galileo has adopted then we would get a "clearer than day light" picture of things. Since there is a difference in the interpretative framework of Galileo and Aristotle it cannot be said that the latter's mistake would be clear to anybody. After all centuries had to pass to realize Aristotle's 'mistakes'. Galileo, we think, uses often and more often than necessary, the phrase "who will ever believe that" or "who would ever say that", when its known rather clearly that the Peripatetics did indeed say and believed just that, and their views remained dominant for centuries. See for example, the passage in full:

> For who will ever believe that if, for example, two lead balls, one a hundred times as large as the other, are let fall from the sphere of the moon, and if the larger comes down to the earth in one hour, the smaller will require one hundred hours for its motion? ... Or, again, if a very large piece of wood and a small piece of the same wood, the large piece being a hundred times the size of the small one, begin to rise from the bottom of the sea at the same time, who would ever say that the large piece would rise to the surface of the water a hundred times more swiftly?

While Aristotelians would not hesitate to say for objects that are twice as large as the other, they certainly would hold on for a while before responding to questions of this kind, where the proportion of variation is too large.[18] Galileo uses this method of amplifying the variation in order to correspondingly amplify the effects that follow, so that certain observations that are difficult at small variations would become visible at large variations. This method of glorifying both the truth and falsity must have certainly worked well against the Aristotelians. We shall see more such instances on the way.

Could one believe contrary to what the Aristotelians said, without reasoning this way? It is difficult to think so, because unless one is motivated enough these reasons are difficult to come by. The Aristotelian mistake is not 'childish', the expression that Galileo uses more than once, and it appears like a rather natural mistake for anyone who believed in a hypothesis based on induction, and not based on rigorous reasoning, which is to reduce the unknown to the known. For Galileo, things appear clearer than daylight because he could achieve such a reduction, of the unknown case of natural motion to the known case of balance and hydrostatics. Since this reduction is not an easy task and is far from self evident, we

[18] Compare Simplicio's responses to these questions in *The Two New Sciences*.

think, Galileo's remarks are a bit too rash. Galileo, however, did not publish this work, and remarks of this sort are evidently deleted in his published works.

Let us return to the substantial arguments Galileo provides against and in favor of his thesis. Before we do that let us also note the symmetrical pattern of Galileo's reasoning in the above quoted passage. Here, as elsewhere, he works out both possibilities of 'natural' motions. Whatever he would assert for one would be asserted for the other by inverting the conditions, displaying the symmetry of the argument. In the following thought experiments too, this point recurs again and again.

Galileo's view, contrary to Aristotle's, is that bodies of the same kind i.e., made of the same substance, whatever be their size move at the same rate in the same same medium. To help understand this rather surprising conclusion Galileo asks us to conduct a series of thought experiments. It should be pointed out that while this statement of Galileo is sufficiently surprising, this remains a statement that plays the role of only a 'rung' of the ladder he climbed, for ultimately he arrived at a far more surprising statement, viz., that *all bodies irrespective of their kind, and irrespective of their size fall at the same rate in vacuum (or void).* We shall see how he, step after step, arrives at this conclusion. It did not take him less than four decades to reach from one rung to the next.

His arguments in the form of thought experiments have the following pattern. First, he considers the case of a body moving downwards and upwards in a changing medium, and second, the case of a combination of two bodies moving downwards and upwards, and finally, on the basis of the above two arguments he proves that Aristotle's thesis leads to contradiction.

First: Consider a medium like water on which one large and another small piece of wood are afloat. *Imagine* that the medium is gradually made successively lighter, so that finally the medium becomes lighter than the wood and both pieces slowly begin to sink. Now following the principles of hydrostatics "who could ever say that the large piece would sink first or more swiftly than the small piece?"

As already argued both the pieces being made out of the same material (wood) they would have the same heaviness (specific gravity), which is same for wood whatever be its size. since heaviness is the only determining factor of natural motion, as already argued, there would be no difference in their motion.

> For, though the large piece of wood is heavier than the small one, we must nevertheless consider the large piece in connection with the large amount of water that tends to be raised by it, and the small piece of wood in connection with the correspondingly small amount of water. And since the volumes of water to be raised

> by the large piece of wood is equal to that of the wood itself, and similarly with the small piece, those two quantities of water, which are raised by the respective pieces of wood, have same ratio to each other in their weights as do their volumes ... i.e., the same ratio as that of the volumes of the large and the small piece of wood. Therefore the ratio of the weight of the large piece of wood to the weight of the water that it tends to raise is equal to the ratio of weight of the small piece of wood to the weight of the water that *it* tends to raise.[19]

Consider now the case of a large piece of wax floating on water and suppose by some means, such as mixing some sand with the wax, it be made successively heavier than water so that it would begin to sink slowly. If we take say one-hundredth part of *that* wax, considering the principles of hydrostatics, who would ever believe that the piece would not sink at all or would sink hundred times more slowly than the whole piece of wax?

In the former experiment the medium was considered for change and in the latter the floating body. This aspect of experimental science, changing the parameters symmetrically, but successively, reveals an important truth about a law of nature. In this case it is illustrated that it is the difference of specific weight that matters, and not whether the difference is with the body or with the medium. *The source of difference does not matter, what matters is the difference*. Working out the argument by varying the conditions symmetrically now on the 'left side' and now on the 'right side' of the balance, and obtaining an invariant result remains a remarkable feature of Galileo's thinking pattern, also true of the structure of scientific thinking. If the situation can be reduced to the balance, then how and why should it matter which side is considered for variation. Having shown that it is the difference in specific weight that matters and not the source of difference, he goes to the next step of the argument.

Second: Consider there are two bodies of which one moves more swiftly than the other, then the

> combination of the two will move more slowly than that part which by itself moved more swiftly, but the combination will move more swiftly than that part which by itself moved more slowly.[20]

For example, take the combination of a piece of wax and an inflated bladder both moving upward from deep water.

> [W]ho can doubt that the slowness of the wax will be diminished by the speed of bladder, and, on the other hand, that the speed of the bladder will be retarded by the slowness of the wax, and that some motion will result intermediate between the slowness of the wax and the speed of the bladder?[21]

[19] pp. 27-28.
[20] p. 28.
[21] p. 29.

Figure 8.2: Thought Experiment

Similarly the combination of wood and bladder in air will fall more slowly than the wood alone, but more swiftly than the bladder alone. Similarly when two equal bodies moving equally come close and join together they would not double their speed, contrary to Aristotle, for the same reason. It follows from this that the same kind of body, whatever be its weight should move at the same speed. Galileo having also shown what happens when two bodies combine, goes on to prove that Aristotle contradicts himself. The proof is as follows:

> Suppose there are two bodies of the same material, the larger a, and the smaller b, and suppose, if it is possible, as asserted by our opponent, that a moves [in natural motion] more swiftly than b. We have, then, two bodies of which one moves more swiftly. Therefore, according to our assumption, the combination of the two bodies will move more slowly than that part which by itself moved more swiftly than the other. If, then, a and b are combined, the combination of a and b is larger than a is alone. Therefore, contrary to the assertion of our opponents, the larger body will move more slowly than the smaller. But this would be self-contradictory.[22]

Thus Galileo demonstrates that Aristotle's view is incorrect. The next step is to finally refute Aristotle's thesis that the ratio of the speeds of a body in different media is equal to the ratio of the rareness of the media. Galileo's correction consists in applying the principle discovered earlier that for 'natural' motion we need to consider not the densities and weights of the body and medium as such, but in relation to each other, which means the excess of weight of one medium over the weight of the moving body is to the excess of the other medium over the weight of the body.[23]

> Therefore, if we wish to know at once the [relative] speeds of a given body in two different media, we take an amount of each medium equal to the volume of the body, and *subtract* from the weights [of such amounts] of each medium the weight of the body. The numbers found as remainders will be to each other as the speeds of the motions.

This, then, is the method suggested by Galileo for calculating the speeds. This might appear like a minor difference, and just a correction. However, the consequences of this discovery

[22] p. 29.
[23] p. 34-35.

for the later developments of the subject are immense. It was with the help of this incorrect law that Aristotle argues quite convincingly against the Atomists' thesis of void. Since his arguments are based on this wrong presupposition, among others, if it were not refuted convincingly the possibility of motion in the void, which is indispensable for the emergence of classical mechanics, would not have been possible.

The crucial point upon which this correction is based needs a special mention. The crucial point is that the mathematical operation suggested by Galileo is *subtraction*, and not division. Because what needs to be known is the excess of weight. But one can say this in two ways, one way is to speak in terms of *how many times* a body is more or less than the other. Aristotle does this, while Galileo employs the other alternative, which is to speak in terms of *how much more or less*. We shall come back to this question later, after elaborating Galileo's arguments in favor of motion in the void. At this place, a comment may however be made that to know a proportionality relation is one thing, and to know what kind of (geometrical or arithmetic) proportionality to apply, is another. For proper application of a mathematical operation here Galileo was again guided by the the inverse structure of the balance. Though enough has already been said about this, it needs to be repeated in every context, and in this context specially, because it demonstrates a point of the thesis that inverse structure (such as that of a balance) can also suggest what mathematical operation to apply.

> One can see this ... in the weights of a scale. For if the weights are in balance, and an additional weight is added to one side, then that side moves down, not in consequence of its whole weight, but *only by reason of the weight by which it exceeds the weight on the other side*. That is the same as if we were to say that the weight on this side moves down with a force measured by the amount by which the weight on the other side is less than it. And for the same reason, the weight on the other side will move up with a force measured by the amount by which the weight on the first side is greater than it.[24]

These considerations suggest that Galileo was not a born antagonist of Aristotle's theory, but it was by systematic reason that Galileo turned against his theory. He began as a student of Aristotelian mechanics, started renovating in the same conceptual space, and introduced necessary changes that initially enter as correction factors. But when the process was gradually extended to domains nearer the 'boundary of the world', total inversion of the framework takes place, when the changes that occur are no longer corrections of the building, but involve radical restructuring.

One such radical change, highly crucial for the later developments of classical mechanics, takes place when the above line of argument was extended successively.

[24] p. 39.

Does every object has a weight of its own, an exact weight? He believed that it would have weight of some value. But an object may lose all its weight or develop negative weight depending on in which medium the body is present. Galileo reaches the conclusion that objects can have their exact weight only in a void. The path of this discovery is simple, but takes place by gradual elimination of the medium altogether.

> Now from what has been said it should be clear to everyone that we do not have for any object its own proper weight. For if two objects are weighed, let us say, in water, who can say that the weights which we then obtain are the true weights of these objects, when, if these same objects are weighed in air, the weights will prove to be different from those [found in water] and will have a different ratio to each other? And if these objects could again be weighed in still another medium, e.g., fire, the weights would once more be different, and would have a different ratio to each other. And in this way the weights will always vary, along with the differences of the media. But if the objects could be weighed in a void, then we surely would find their exact weights, when no weight of the medium would diminish the weight of the objects.[25]

Aristotle could not have arrived at this conclusion because of several impeding suppositions. One major contrary supposition of Aristotle was that the elements would have their proper weight in their own places, according to the natural arrangement, while for Galileo things would have improper weight in their natural places. Second, he believed that if void existed, motion would take place instantaneously, while for Galileo *pure and unimpeded motion takes place only in a void*. Third, the medium is supposed as an indispensable cause of motion, while for Galileo the role of the medium ceases to be an impeding factor of 'real' motion. All these conceptual transformations, however, took place as illustrated above by the systematic application of inverse reasoning, and is in not a result of a flash of insight or a conjecture or even a dream.

Considering the often realized fact that the laws of classical mechanics would apply more accurately in a space devoid of a number of material hindrances, the discovery, in the first place, that only in such a void objects would behave exactly and would appear in their purity will be significant. Based on the method of calculating speeds of bodies in different media Galileo proves that motion is possible in the void, and it does not take place instantaneously, but in time. This argument is for Galileo a 'spring board' that has thrown him into a space up above all that is 'earthy' so that most of his later thinking could take place in a space where material hindrances mattered little while investigating the exact relationship between crucial parameters of motion. His excessive involvement so far with the

[25] p. 40.

medium gradually vanishes, helping him to say finally that all objects irrespective of their size and shape would fall at the same velocity. This, being a remarkable and quite surprising discovery is crucial for understanding the conceptual transformation. We shall discuss it in the following section.

8.4 Motion and Weight in the Void

The modern science of mechanics is *unthinkable* without the idea of motion in a vacuum or void. Though the idea of the void is not new, it is kept away from discussion due to Aristotle's aversion towards it and the conjoint atomistic world view. As already mentioned, Galileo was intellectually influenced by both the mathematical method of Archimedes and the world view of the Atomists. He specially argued against Aristotle's position that void and motion in the void are impossible. Galileo's line of attack also demonstrates where Aristotle went wrong. We shall first brief Aristotle's argument.

Aristotle's thesis is that "void in so far as it is void admits no difference."[26] In media like air and water bodies move because they admit difference (bodies are offered resistance differentially), and since void admits of no difference motion is impossible: "not a single thing can be moved if there *is* void."[27]

> [H]ow can there be natural movement if there is no difference throughout the void or the infinite? For in so far as it is infinite, there will be no up or down or middle, and in so far as it is a void, up differs no whit from down; for as there is no difference in what is nothing, there is none in the void (for the void seems to be a non-existent and a privation of being), but natural locomotion seems to be differentiated, so that the things that exist by nature must be differentiated. Either, then, nothing has a natural locomotion, or else there is no void.[28]

Aristotle's assumption that differences in the speed of a body arise from differences in density (rarity) of the medium has already been proved false by Galileo. It is also proved that ratio of the speeds of the motion of the body is not equal to the ratio of the rareness or density of the media. Aristotle also held a view that no number can have the same relation to another number as a number has to zero. In this view lies the major problem.

> We see the same weight or body moving faster than another for two reasons, either because there is a difference in what it moves through, as between water, air, and earth, or because, other things being equal, the moving body differs from the other owing to excess of weight or of lightness.

[26] 214b34.
[27] 214b30.
[28] 215a5-13.

Now the medium causes a difference because it impedes the moving thing, most of all if it is moving in the opposite direction, but in a secondary degree even if it is at rest; and especially a medium that is not easily divided, i.e. a medium that is somewhat dense.

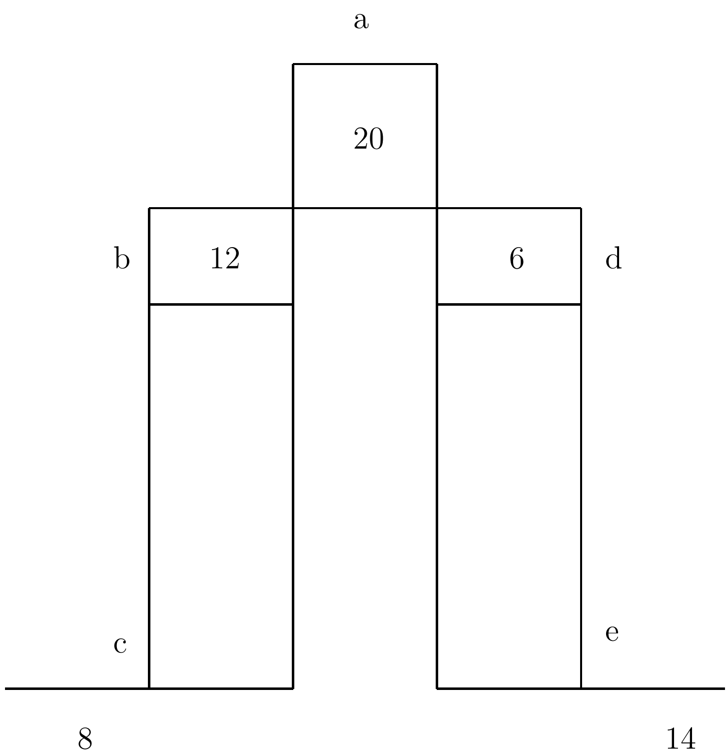
A, then, will move through B in time C, and through D, which is thinner, in time E (if the length of B is equal to D), in proportion to the density of the hindering body. For let B be water and D air; then by so much as air is thinner and more incorporeal than water, A will move through D faster than through B. Let the speed have the same ratio to the speed, then, that air has to water. Then if air is twice as thin, the body will traverse B in twice the time that it does D, and the time C will be twice the time E. And always, so much as the medium is more incorporeal and less resistant and more easily divided, the faster will be the movement.

Now there is no ratio in which the void is exceeded by body; as there is no ratio of 0 to a number. For if 4 exceeds 3 by 1, and 2 by more than 1, and 1 by still more than it exceeds 2, still there is no ratio by which it exceeds 0; for that which exceeds must be divisible into the excess + that which is exceeded, so that 4 will be what it exceeds 0 by + 0. For this reason, too, a line does not exceed a point—unless it is composed of points! Similarly the void can bear no ratio to the full, and therefore neither can movement through the one to movement through the other, but if a thing moves through the thickest medium such and such a distance in such a time, *it moves through the void with a speed beyond any ratio.* For let F be void, equal in magnitude to B and to D. Then if A is to traverse and move through it in a certain time, G, a time less than E, however, the void will bear this ratio to the full. But in a time equal to G, A will traverse the part H of D. And it will surely also traverse in that time any substance F which exceeds air in thickness in the ratio which the time E bears to the time G. For if the body F be as much thinner than D as E exceeds G, A, if it moves through F, will traverse it in a time inverse to the speed of the movement, i.e. in a time equal to G. If, then, there is *no* body in F, A will traverse F still more quickly. But we supposed that it traverses F in an equal time whether F be full or void. But this is impossible. It is plain, then, that if there is a time in which it will move through any part of the void, this impossible result will follow: it will be found to traverse a certain distance, whether this be full or void, in an equal time; for there will be some *body* which is in the same ratio to the other body as the time is to the time.

To sum the matter up, the cause of this result is obvious, viz. that between any two movements there is a ratio (for they occupy time, and there is a ratio between any two times, so long as both are finite), but there is no ratio of void to full.[29]

This long passage reflects the structure of Aristotle's thinking. It also reflects the state-of-the-art of mathematics at his period. Galileo's argument begins by *correcting Aristotle's mistakes in calculation.* His argument briefly is that one should be applying arithmetic proportions and not geometric proportions as Aristotle did, in this case. Aristotle's argument is valid

[29] 215a25–216a8.



GALILEO	ARISTOTLE
14 - 8	12 / 6
15 - 8	12 / 5
16 - 8	12 / 4
17 - 8	12 / 3
18 - 8	12 / 2
19 - 8	12 / 1
20 - 8	12 / 0

Figure 8.3: Galileo's and Aristotle's Calculations on Fall of Bodies

(deductively speaking). But that is not sufficient for natural science. The premises must be true. His premise that the ratio of the speeds were equal to the ratio of the rareness of the media, in the geometrical sense, is incorrect, because the speeds depend on arithmetic ratio. Therefore Aristotle's conclusion that no motion is possible in the void would not follow. Galileo proves Aristotle wrong in the following thought experiment (See figure 8.3.)

Suppose a body a whose weight is 20, and two different media, bc and de. Let the volume of a, b and d be equal, and their weights 20, 12, and 6 respectively. The ratio of the speed of body a in the medium bc and de will be equal to the excess of weight of a over the weight of the medium bc to the excess of weight of d to the weight of the medium de, which is $8 : 14$. If the speed of a in bc is 8, its speed in de will be 14. Aristotle would have calculated the ratio as $8 : 16$, because bc is doubly denser than de. Since Galileo calculates the arithmetical difference, the difference in speed is lesser than Aristotle's calculation. It therefore follows that the speed does not increase at the same rate even if there is a similar rate of decrease in the density of the medium. Realizing this apparently minor point of difference is more than vital for the development of the modern science of motion, where medium does not play an essential role in the motion of bodies, because the impeding effect of the medium is less than what Aristotle expected. To see how Galileo extrapolates these simple calculations to bring back the void into physics, let us calculate the ratios of speeds by decreasing the values of the the weight of *de* gradually, keeping the rest of the things constant.

If the medium *de* has the weight 4, for the unit volume, the speed of *a* in *de* according to Galileo would be 16, and the difference between the speeds of *a* in *bc* and *de* would be 8, which means only *twice* as fast as in *bc*. Aristotle's calculation for the same situation shows that it would be *thrice* (12/4) (See the table in the figure 8.3). When we decrease the weight of *de* further to 3, the difference in speed will be 9 for Galileo, but for Aristotle four times. When the weight of *de* is further decreased to 2, Aristotle would calculate a difference of six times that *a*'s speed in *bc* than *de*, and at a further decrease it becomes 12 times. And finally one more step and Aristotle would be in great trouble, because when the weight of *de* becomes 0, Aristotle gets at what is impossible to comprehend, 20/0. For a similar situation Galileo gets a convenient $8 : 20$.

On one hand Aristotle uses his calculations to abandon motion in void, which was presumed to be of zero weight. On the other hand Galileo goes to a radical conclusion that *the pure form of motion takes place only in the void.* It is thus very clear how eventually for Galileo the medium became an *impeding and accidental* factor, while for Aristotle it was an essential factor of motion. What was held to be necessary became accidental, and what was held to be impossible became not only possible, but became the purest possible. A systematic application of inverse reason eventually *inverted* our notion of what motion is—it is radical enough to call it revolutionary.

The above proof also brings home the point that it is one thing to know that two quantities are inversely proportional and quite another thing to know the *quality* of proportionality, i.e., whether arithmetic or geometric. Aristotle's charge that it is impossible for one number to have the same relation to another number as a number has to zero, has been proved by Galileo as untenable. In conclusion to this proof, Galileo says:

> Therefore, the body will move in a void in the same way as in a plenum. for in a plenum the speed of motion of a body depends on the difference between its weight of the medium through which it moves. And likewise in a void [the speed of] its motion will depend on the difference between its own weight and that of the medium. But since the latter is zero, the difference between the weight of the body. And since the latter is zero, the difference between the weight of the body and the weight of the void will be the whole weight of the body. And therefore the speed of its motion [in the void] will depend on its own total weight.[30]

[30] p. 45.

It becomes clear from this passage that Galileo considers the void as a medium with zero weight. As noted earlier, things weigh proper in the void because in any media other than the void they will always be lighter.

In *De Motu* Galileo thought that different bodies would fall in the void at different rates, though irrespective of their size or weight.

> For example, in the case of a body whose weight is 8, the excess over the weight of the void (which is 0) is 8; hence its speed will be 8. But if the weight of a body is 4, the excess over the [weight of the] void will, in the same way, be 4; and hence its speed will be 4. finally, using the same method of proof in the case of the void as we used in the case of the plenum, we can show that bodies of the same material but of different size move with the same speed in a void.[31]

This conclusion is not correct which Galileo realizes much later. In *Discorsi* (First Day) he reaches the correct conclusion that all bodies, irrespective of weight, density and size fall in the void at the same rate. To arrive at this conclusion Galileo has to correct another of his premises. In *De Motu* he thought that in natural fall bodies would fall at a constant speed, while later he corrects this to say that they undergo uniform acceleration. Both these corrections are very vital for the further development of the science of motion. We shall see below, how even these developments took place as a result of inverse reasoning reaching a 'finale' as far as natural fall of bodies is concerned.

8.5 Heavy and Light

A very good example to know the difference between Aristotle's and Galileo's thinking patterns is to study their notions of heavy and light. This case shows quite glaringly exactly where the differences between Galileo and Aristotle lie. One may wonder how important the notions of heavy and light are in a discussion which is on motion. In fact lack of clarity on these notions remained a major intellectual hurdle for the development of the subject of motion. Galileo realizes the importance of these pair of ideas and spends a lot of his energy to correct the views of Aristotle. However, we will see that gradually Galileo eliminates the term 'heaviness' and begins to talk in term of force, as he proceeds further. A terminological clarification: In this section the term 'heavy', should be understood as weight, and not as heaviness as defined by Galileo in the first section.

Aristotle defines the notions absolutely heavy and absolutely light as follows:

> In accordance with general conviction we may distinguish the absolutely heavy, as that which sinks to the bottom of all things, from the absolutely light, which

[31] pp 48-49.

> is that which rises to the surface of all things. *I use the term 'absolutely', in view of the generic character of 'light' and 'heavy', in order to confine the application to bodies which do not combine lightness and heaviness.* ... But the heaviness and lightness of bodies which combine these qualities is different from this, since while they rise to the surface of some bodies they sink to the bottom of others. Such as air and water.[32]

Thus for Aristotle the distinction is absolute. It is also clear that he defines them in terms of the direction of motion, which it should be noted is only in the vertical component. We have already discussed Galileo's early thinking on this matter and noted that heaviness becomes the *cause* of motion. (To begin with Galileo also concentrated on motion in the vertical component, but gradually he shifts his attention to the horizontal component. In this shift we will see below how inverse reason plays the central role in yet another revolutionary transformation.) In the above section we have shown how Galileo arrives at the belief that pure form of motion is possible only in void. His view on the weight of bodies is that bodies can be properly weighed only in the void, which contrasts well with that of Aristotle's, according to whom air and water are sometimes lighter and sometimes heavier, but are *absolutely* heavy only in their own proper place. Aristotle thought that all bodies are either made of the matter that is light or that is heavy or those that contain both kinds of matters, light and heavy. The one which contains the light kind of matter is fire, and the one that contains the heavy kind is earth. Air and water contain both kinds. Air has more of the light kind than water and water has has more of the heavy kind than air.[33] Therefore all the elements except fire have weight and all but earth lightness. Earth being absolutely heavy always moves downwards, and fire being absolutely light moves always upwards. Since heavy/light are defined in terms of motion, and motion determines the position of different bodies, everything has a fixed 'natural' position on earth.[34]

Aristotle, as is well known, develops his ideas on this subject by rejecting the Atomists' thesis that there existed only one kind of matter and heavy and light are to be understood relatively. Referring to this Aristotle says:

> Our predecessors have not dealt at all with the absolute use of the term, but only with the relative. I mean, they do not explain what the heavy is or what the light is, but only the relative heaviness and lightness of things possessing weight.[35]

We have already observed that Galileo is returning to the Atomists' manner of relativistic

[32] *De Caelo* Bk.IV, Ch.4,311a16–24. Also cf. 308a28–31, 311b16–18.
[33] *De Caelo* Bk.IV, Ch.5, 22-25.
[34] See quotation on 266
[35] 308a10–13.

thinking. Plato also held a relativistic position with respect to heaviness and lightness.[36] Aristotle is explicitly arguing against the Atomists' and Plato. His absolutist manner of thinking prevents him from seeing the possible, which he in fact goes on to rule of as as impossible. For example, he says:

> [S]ince a multitude of small atoms are heavier than a few large ones, it will follow that much air or fire is heavier than a little water or earth, which is impossible.[37]

Galileo's arguments demonstrate precisely this that it is indeed possible to bottle air that weighs more than Aristotle's weight. This shows clearly where Aristotle has gone wrong.

Aristotle certainly recognized the importance of contraries, for he thought that all changes are to be explained either in terms of changes to a contrary or in terms of something intermediate. However, he continued to oppose the Atomists' thesis by asserting that contraries are absolute, and he allowed relative usage of the terms to refer only to the intermediate stages.[38] He further believed that the changes are not due to any accidental factors:

> the thing altered is different from the thing increased, and precisely the same differences hold between that which produces alteration and that which produces increase. ... Now, that which produces upward and downward movement is that which produces weight and lightness, and that which is moved is that which is potentially heavy or light, and the movement of each body to its own place is motion towards its own form.[39]

These are the reasons given in support of his views, and against the Atomists' views.

Galileo argues against Aristotle's thesis by applying the principles of hydrostatics. Water in its own place and air in its own place cannot have any any weight because such a situation is nothing but a state of equilibrium. This is because neither air nor water moves downward or upward in its own region. Therefore, Galileo says that "they should not be called either heavy or light".[40] Bodies would have their proper weight only in the void and not in their own place. Aristotle applies his absolute idea of lightness in this argument knowing fully well that the Atomists' notion was relative. Therefore, Galileo says that Aristotle should have framed his argument that fire in its own place has weight, and a large amount of it in its own

[36] Aristotle cites Plato's *Timaeus*: "One use of the terms 'lighter' and 'heavier' is that which is set forth in writing in the *Timaeus* [63 C.], that the body which is composed of the greater number of identical parts is relatively heavy, while that which is composed of a smaller number is relatively light." 308b5–9.

[37] 310a11–13.

[38] Bk.IV, Ch.4.

[39] 310a26–29.

[40] p. 55

place would weigh more, and where fire has no weight (as in air) a large amount of it would have more lightness.

Aristotle's criterion of heaviness is that a heavy body should be seen falling down. But for Galileo a large amount of water can be heavier than a small amount of water without water showing any downward motion. Because downfall is determined by relative density and not weight. In this sense Galileo could find an independent cause of motion that escapes the circularity of Aristotle's definition of heavy and light. For Aristotle, that which is heavy goes downwards and that which goes downwards is heavy, and similarly for lightness. Aristotle's mistakes, therefore, arise from his inability to apply the worthier notion of density, which is *invariant* with respect to the size of things. The notion of density is of course impossible without relativistic thinking.

8.6 Discovering the Horizontal Component

The structure of balance and general principles of statics helped Galileo to reconcile the problems that arise when, like Aristotle, contraries are considered as absolute and as different genera. There are other cases of motion that also are analogous to the structure of balance such as the motion on inclined planes and the motion of a pendulum. Each of these cases upon analysis help in understanding a few more dimensions of motion that are otherwise not so easy to isolate from the complex fabric of phenomena. It is vital, for the emergence of the modern science of motion, to make possible the understanding of a certain special situation which can be characterized as a *neither-nor-state.* The state of equilibrium in a balance is one such state, where the weights move *neither upward nor downward.*

Just as bodies fall down 'naturally' they also roll down on any inclined surface. The case of the inclined plane is thus just another case of natural motion. But experimentally it is a very efficient and easy means of studying motion, specially in relation to measuring time and distance. This is because it takes more time to roll down on an inclined plane than in a vertical fall. The delay would make it easier to observe and measure, otherwise difficult, the relationship between velocity, time and distance. Galileo claims that his studies are the first on this problem; "The problem we are going to discuss has not been taken up by any philosophers, so far as I know." He states the problem as follows:

> The problem is why the same heavy body, moving downward in natural motion over various planes inclined to the plane of the horizon, moves more readily and swiftly on those planes that make angles nearer a right angle with the horizon; and, in addition, the problem calls for the ratio [of speeds] of the motions that

take place at the various inclinations.[41]

His approach toward solving this problem is based on the earlier understanding that the force necessary to lift a body upwards is equal to the force it tends to move downwards. This is another typical example of the method of solving the problem by reducing the unknown to the known.[42] The known aspect of knowledge consists in statics and geometry, and the unknown aspect is the problem at hand, viz. the motion of a body on an inclined plane. His mature mathematical mind is at work in solving this problem. He says what the solution of the problem constists in.

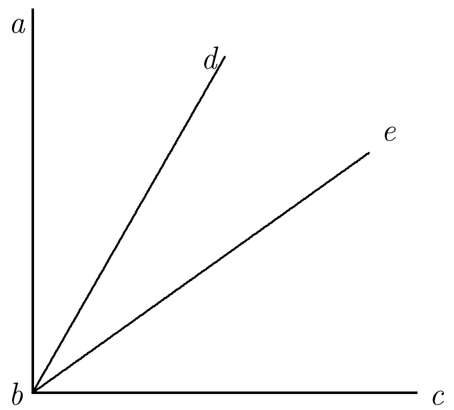


Figure 8.4: Inclined Planes

> If ... we can find with how much less force the heavy body can be drawn up on line *bd* than on line *ba*, we will then have found with how much greater force the same heavy body descends on line *ab* than on line *bd*. And, similarly, if we can find how much greater force is needed to draw the body upward on line *bd* than on *be*, we will then have found with how much greater force the body will descend on *bd* than on *be*. But we shall know how much less force is required to draw the body upward on *bd* than on *be* as soon as we find out how much greater will be the weight of that body on the [inclined] plane along *bd* than on the plane along *be*.

We shall present the procedure followed by Galileo in full detail.

> Consider a balance *cd*, with center *a*, having at point *c* a weight equal to another weight at point *d*. Now, if we suppose that line *ad* moves toward *b*, pivoting about the fixed point *a*, then the descent of the body, at the initial point *d*, will be as if on line *ef*. Therefore, the descent of the body on line *ef* will be a consequence of the weight of the body at point *d*. Again, when the body is at *s*, its descent at the initial point *s* will be as if on line *gh*; and hence the motion

[41] p. 63.
[42] p. 63.

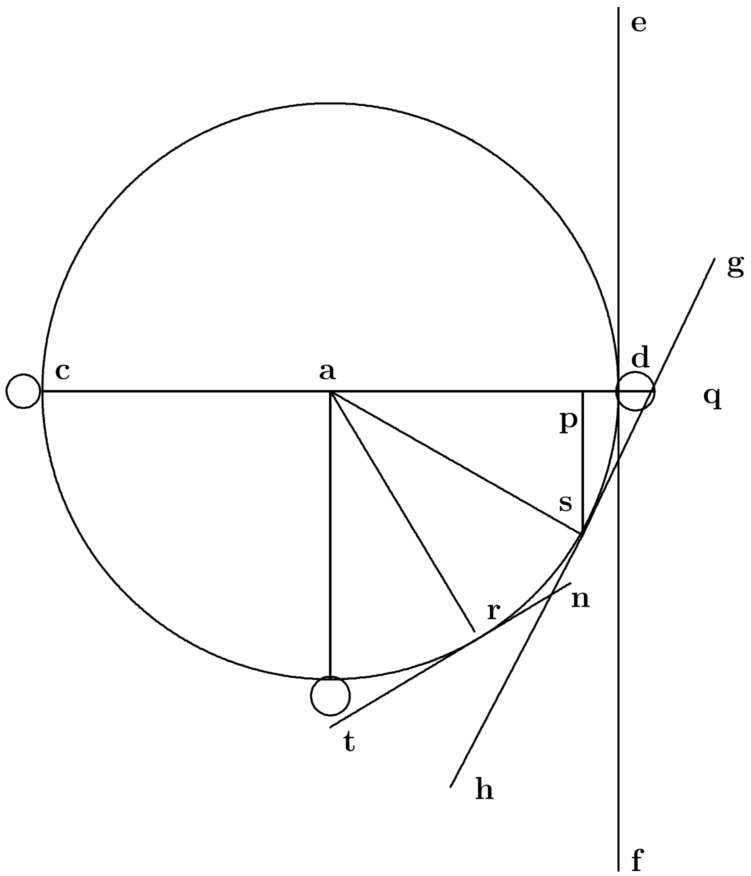


Figure 8.5: From Balance to Inclined Planes

of the body on gh will be a consequence of the weight that the body has at point s. And again, at the time when the body is at point r, its descent at the initial point r will be as if on line tn; hence the body will move on line tn in consequence of the weight that it has at point r.

If, then, we can show that the body is less heavy at point s than at point d, clearly its motion on line gh will be slower than on ef. And if, again, we can show that the body at r is still less heavy than at point s, clearly the motion on line nt will be slower than on gh. Now it is clear that the body exerts less force at point r than at point s, and less at s than at d. For the weight at point d just balances the weight at point c, since the distances ca and ad are equal. But the weight at point s does not balance that at c. For if a line is drawn from point s perpendicular to cd, the weight at s, as compared with the weight at c, is as if it were suspended from p. But a weight at p exerts less force than the [equal] weight at c, since the distance pa is less than distance ac. Similarly, a weight at r exerts less force than an [equal] weight at s: this will likewise become clear if we draw a perpendicular from r to ad, for this perpendicular will intersect ad between points a and p. It is obvious, then, that the body will descend on line ef with greater force than on line gh, and on gh with greater force than on nt.

But with *how much* greater force it moves on ef than on gh will be made

> clear as follows, viz., by extending line *ad* beyond the circle, to intersect line *gh* at point *q*. Now since the body descends on line *ef* more readily than on *gh* in the same ratio as the body is heavier at point *d* than at point *s*, and since it is heavier at *d* than at *s* in proportion as line *da* is longer than *ap*, it follows that the body will descend on line *ef* more readily than on *gh* in proportion as line *da* is longer than *pa*. Therefore the speed on *ef* will bear to the speed on *gh* the same ratio as line *da* to line *pa*. And as *da* is to *pa*, so is *qs* to *sp*, i.e., the length of the oblique descent to the length of the vertical drop. And it is clear that the same weight can be drawn up an inclined plane with less force than vertically, in proportion as the vertical ascent is smaller than the oblique. Consequently, the same heavy body will descend vertically with greater force than on an inclined plane in proportion as the length of the descent on the incline is greater than the vertical fall.

At each point on the descent the body is considered as if it is on an inclined plane. His proof shows that the body will become less and less heavier as it descends. This proof evidently makes use of the principles of lever: equal weights balance at equal distances; and unequal weights will balance at unequal distances. Which would mean that the weight of the body at *s* could balance *c* only at *q*, and cannot balance either at *p* or at *d*. Therefore it is clear that the inverse relation of unequal weights and the distances from the fulcrum of balance has been applied to get the desired result. We can determine or calculate the unknown only if the known and the unknown are related inversely.

He also assumes in this proof that the conditions are ideal ("incorporeal") and no accidental resistance would be caused by the roughness of the body or inclined plane or by the slope of the body.[43] Though Galileo would ultimately apply his knowledge to corporeal things, all his thinking takes place under ideal conditions, and hence his assertions are counterfactuals.

Having obtained a method of measuring the force required to overcome any given weight on an inclined plane, he extrapolates this to arrive at very important results. It follows from the results that any body on a plane parallel to the horizon will be moved by the smallest force, a force less than any given force.[44] Galileo proves this as usual by analogy with balance. Any weight rigidly suspended from the center of a balance would move and raise whatever little be the force exerted upon any side of the balance at equilibrium. The state of the horizontal plane is a neither-nor-state. He deduces the result as follows:

> [1] A body subject to no external resistance on a plane sloping no matter how little below the horizon will move down [the plane] in natural motion, without the application of any external force. ... [2] And the same body on a plane sloping

[43] p. 65.
[44] pp. 65–66.

> upward, no matter how little, above the horizon, does not move up [the plane] except by force. [3] And so the conclusion remains that *on the horizontal plane itself the motion of the body is neither natural nor forced.*[45]

This conclusion undoubtedly anticipates the *principle of inertia*. We have also noted earlier how similar descriptions were given for weights in a balance and floating bodies, which move neither up nor down. All these are different physical states reducible to a state of equilibrium or neither-nor-states. In all these cases we see that such a state was obtained by a composition of *two* equal but oppositely acting forces. Hence the equilibrium state is an effect of a *complex* cause.

Consequently, the taxonomic manner of thinking, prevalent in Aristotelian physics, finds no place in Galilean explanations. Galileo successively eliminates most of the dichotomies introduced by Aristotle and demonstrates by way of erecting an alternative model of scientific thinking according to which the contraries are not to be regarded as classifying criteria, but as covarying factors in a physical system. While Aristotle was looking for one cause for each kind of effect, Galileo is looking for one composite relation that would explain a set of possible effects. In this process, Galileo could not only eliminate ambiguities and inconsistencies in Aristotelian science, but achieved a greater degree of parsimony.

The significance of the neither-nor-states in science is undoubtedly great. Without visualizing such a state the principle of inertia could not be stated. The cementing factor in composing the forces to arrive at an equilibrium is the inverse relation between the forces.

Thus though initially Galileo started dealing with motion with the vertical component, his attention did shift towards the horizontal component. In this shift, as we have seen, the main guiding force is the model of balance. In *De Motu*, however, he did not resort to formulating various problems possible on the horizontal motion. The explanation for this could be due to the world order which continues to be geocentric. In order to consider the horizontal component seriously it is necessary that the world order be Copernican. He resolves the problems on this plane in a later work in *Dialogues Concerning the Two Chief World Systems,* which cannot be covered here.

So far Galileo has not considered the problem of so called forced motion. Ultimately Galileo gives up the distinction between the traditional dichotomy between forced and natural motion, though he continues to use the terms ‘forced’ and ‘natural’. The real breakthrough came much later in his life, when he correctly understood the proportionality relation between time and velocity. But the final results would not have been possible without the much

[45] p. 66, italics ours. The numbers are included to match with the three possible states that can be generated from a model of balance.

needed conceptual cleansing which took place in *De Motu*. We shall see in the next section how Galileo handled the problem, anticipating many concepts in the process. And most importantly inversion continues to play the main role in the process.

8.7 Projectile Motion

Galileo's analysis of the problem of projectile motion proceeds more or less in the above pattern. He has already shown that the medium plays only an accidental role in motion and that motion is shown possible even in the void. Therefore he believes that when a body is thrown up, the motive force from the hand of the projector is transmitted to none other than the projectile itself. Let us recall that Aristotle believed that the force is transmitted to the medium, and not to the body.(§7.3 page 227) Hence the problem is: "What is that motive force which is impressed by the projector upon the projectile?"[46]

In the case of natural motion a body goes up because it is lighter than the medium. Galileo takes a clue from the logic of hydrostatics, and says that since the body is moving up in a projectile motion, in this case too the body must be becoming lighter. He proposes that the motive force makes the body lighter. Galileo speaks here as if some invisible 'substance' enters into or comes out of the body, making it now lighter, and now heavier. He says it is like heat depriving the coldness of a metal when heated.[47] The analogy is with the corresponding inverses: heat is to lightness and coldness is to heaviness. The change that is taking place is one of transformation ("alterative motion") of some inherent quality, though temporarily.

> The body ... is moved upward by the projector so long as it is in his hand and is deprived of its weight; in the same way the iron is moved, in an alterative motion, towards heat, so long as the iron is in the fire and is deprived by it of its coldness. Motive force, that is to say lightness, is *preserved in the stone*, when the mover is no longer in contact; heat is preserved in the iron after the iron is removed from the fire. The impressed force gradually diminishes in the projectile when it is no longer in contact with the projector; the heat diminishes in the iron, when the fire is not present. The stone finally comes to rest; the iron similarly returns to its natural coldness. *Motion is more strongly impressed by the same given force in a body that is more resistant than in one that is less resistant, e.g., in the stone, more than in light pumice; and, similarly heat is more strongly impressed by the same fire upon very hard, cold iron, than upon weak addition less cold wood.*[48]

The motive force is said to be something that can be *preserved* in the projector which indeed is an anticipation of the notion of *energy*. This observation gets additional support from the

[46]p. 78.
[47]pp. 78–79.
[48]p. 79, italics ours.

very important correlation made in the above passage between the denser bodies like stone, and their ability to take more motive force from the projector.

This fact might appear contrary to what we experience, because we can move a feather more easily than a stone. Galileo has an answer:

> But the fact is that the lighter the body is, the more is it moved while it is in contact with the mover, but, on being released by the mover, it *retains for only a short time the impetus it has received.* This is clear if someone throws a feather, using as much force as if one had to throw a pound of lead. For he will more easily move the feather than the lead, but the impressed force will be retained in the lead for a longer time than in the feather, and he will throw the lead much farther. If it were the air that carried the projectile along, who would ever believe that the air could carry the lead more easily than the feather? We see therefore that *the lighter a thing is, the more easily is it moved; but the less does it retain the impetus it has received.*[49]

By paraphrasing the last sentence we get: the greater the mass of the body the greater will be its capacity to retain impetus (motive force). This relationship could have led Galileo to a great discovery had he pursued it further to measure this correlation. He could have discovered in the process either kinetic energy or momentum or both. It is easier for us to say these things now, retrospectively, but several other factors of both a mathematical an experimental kind are involved in the actual discovery of the notion of momentum by Descartes and of kinetic energy by Leibniz. The famous debate between Descartes and Leibniz over the issue of which of the quantities is the proper measurement for the quantity of motion is a classic illustration of the point that the ambiguity in the usage of terms is not an ordinary mistake that can be corrected if one is merely attentive enough.[50] This comment also applies to Galileo's ambiguous usage of the terms 'force', 'motive force' 'impetus' etc.

The imparted force *is* called lightness, and it "will render the body in motion light by *inhering* in it".[51] Talking in terms of lightness and heaviness may be confusing in every context. Galileo shows awareness of this problem. To avoid confusion he makes a distinction between "natural or intrinsic weight" and "preternatural" or "accidental" lightness[52]. Natural weight is that which is retained after the projectile returns back, and preternatural lightness is that which is temporarily inherent in the body when it is in projectile motion. He says in analogy with floating bodies that the projectile's "natural and intrinsic weight is lost in the same way as when it is placed in media heavier than itself."[53]

[49]p. 82, italics ours.
[50]Cf. Max Jammer 1967, 'Energy' in *The Encyclopedia of Philosophy* Edited by Paul Edwards. Volume-II, pp. 511–517.
[51]p. 80.
[52]p. 80–81.
[53]p. 81.

> Wood, too, becomes so light in water that it cannot be kept down except by force. And yet, neither the stone nor the wood loses its natural weight, but, on being taken from those heavier media, they both resume their proper weight. In the same way, a projectile, when freed from the projecting force, manifests, by descending, its true and intrinsic weight.[54]

This problem which Galileo is trying to clarify disappears completely when ultimately he sees the possibility of giving up the distinction between natural and forced motion. The analogy with hydrostatics helps him in the process.

This discussion on projectile motion is also an illustration of how Galileo is overcoming the anthropocentric views of commonsense (also of Aristotle) by *balanced* reasoning with opposites. Feathers driven away by the wind, and the 'appearance' of the movement of water waves, created when a stone is thrown in a still pond, will not confuse Galileo, as they did Aristotle who arrived at the false conclusion that the medium pushes the objects. As discussed in §6.10 page 195, one of the necessary conditions of objectivity is to externalize the standards of measurement. Galileo makes use of the possibility of obtaining a 'balance' by opposing quantities such as heavy/light, heat/cold, etc., so that an independent system is constructed. In all the instances discussed above, Galileo has been trying to isolate and externalize a system for subsequent analysis.

The initial idea of analyzing projectile motion in terms of loss and gain of weight continues to function when Galileo argues against Aristotle's views on the falling body.

8.8 Initial Study on Acceleration

In the Chapter 19 Galileo achieves some very important results that reflect the coherent nature of his thinking pattern. In this chapter he finally dissolves the distinction between 'natural' and 'forced'. He argues against Aristotle that the entire path of motion of projectile is *one continuous process* despite what we actually see. He also applies a symmetrical argument, in the sense that he first solves the problem for the fall of bodies and then says that the same applies to the body shooting up by simply reversing the description of the former case.[55] Almost every statement of the following argument is animated by the model of balance.

What are the necessary conditions for moving a body upwards?

> For a heavy body to be able to be moved upward by force, an impelling force

[54] *Ibid.*
[55] p. 88.

> greater than the resisting weight is required; otherwise the resisting weight could not be overcome, and, consequently, the body could not move upward.[56]

This is a simple truth that follows *a priori* from the model of a balance. The impelling force is applied when the object is projected upwards, and soon after that the force gets continuously diminished.

> [I]t will finally become so diminished that it will no longer overcome the weight of the body and will not impel the body beyond that point.[57]

This is the point when the body is *neither moving up nor moving down*, because the impressed force is so diminished that it equals that of the weight of the body. That is "the body will be neither heavy nor light." Did the body then achieve an equilibrium such that motion would stop at that instant? It will not, because "the impressed force characteristically continues to decrease." As a result "the weight of the body begins to be predominant, and consequently the body begins to fall". Here he explains why the motion is slower at the beginning of fall.

> Yet there still remains, at the beginning of this descent, a considerable force that impels the body upwards, which constitutes lightness, though this force is no longer greater than the heaviness of the body. ... Furthermore, since that external force continues to be weakened, the weight of the body, being offset by diminishing resistance, is increased, and the body moves faster and faster.

This is also the cause of the acceleration of motion. Galileo mentions that much before him Hipparchus gave a similar account of the process, which he came to know from Alexander's writings.[58] The description is so appealing that it is very difficult to find objections to this. However, Galileo attends to an objection allegedly made by Alexander that the above account attends to only forced motion. What about natural fall that does not follow an opposite forced motion? Galileo argues that this case requires no new explanation.

> [W]hen a stone, which had been thrown up, begins to move down from that extreme point at which equilibrium occurs between impelling force and resisting weight (i.e., from rest), it begins to fall. *This fall is the same as if the stone dropped from someone's hand.* ... For when the stone is at rest in someone's hand, we must not say that in that case the holder of the stone is impressing no force upon it. Indeed, since the stone presses downward with its own weight, it must be impelled upward by the hand with a force exactly equal, neither larger nor smaller. ... Therefore a force that impels upward is impressed on the stone by the hand or by whatever else controls the hand, and this force is exactly equal to the weight of the stone that tends downward.

[56] p. 89.
[57] *Ibid.*
[58] p. 90.

> ... For in this case, too, when it leaves its [instantaneous] state of rest, it leaves having an [upward] force [impressed on it] equal to its weight. Hence it is for the same cause that, just as in the latter type of [natural] motion [i.e., preceded by forced motion], so in the former [i.e., not preceded by forced motion], the body moves slowly at the beginning.[59]

This argument is not misguided by what one merely observes. This is an excellent example of applying a model to explain a particular instance. Though Galileo has no knowledge of how to measure the impressed force etc., the model in its pure form is more or less complete. The statement that *nothing would fall freely without a preceding impressed force*, undoubtedly anticipates the notion of potential energy. Thus what appears as a free fall is actually a result of a preceding event that imparted an impressed force (potential energy) in the body, and hence a free fall is in continuation with forced motion. As a result of this symmetrically structured model in his thought, Galileo could ultimately make the following statement.

> You can therefore see how well propositions that are true fit in with one another. And from this review anyone will easily be able to understand that *these are really not two contrary motions, but rather a certain motion composed of a forced and a natural motion.*[60]

Thus ultimately the synthesis of the contraries is achieved. He further says that the lightness imparted to the body changes into heaviness in the course of a projectile motion, and *this is a single continuous motion.*

> So far, then, are these motions from being contraries, that they are actually only one, continuous, and coterminous. Hence also the effects which flow from these causes cannot be rightly called contraries, since contrary effects depend on contrary causes. Hence the upward motion cannot rightly be called contrary to the ensuing downward motion—both of which motions proceed from motion [i.e., change] in the *mixture of lightness and heaviness.* And from this it can easily be deduced that [an interval of] rest does not intervene at the turning point [i.e., from upward to downward motion].[61]

We claim that this mixture of contraries is what we otherwise call the *covariance* of the parameters in a system or physical state. In the specific example of projectile motion given above, a modern physicist would have put the matter in terms of kinetic energy and potential energy which are inversely proportional, and the system constitutes a covariance of these quantities. This is a typical manner in which the inverses are composed together into a single system, and only such models provided successful explanations to the phenomena. This entire

[59] p. 91.
[60] p. 93. Italics ours.
[61] p. 94. Italics ours.

account given above is an illustration of how the *principle of included extremes*—the fundamental principle of inverse reason—animates the mind of Galileo as against Aristotle who was solely regulated by the *principle of excluded middle* and the principle of non-contradiction—the fundamental principles of deductive logic. Aristotle would classify a given motion as either natural or forced, while Galileo has reached a stage where he can describe without contradiction a state of motion that is neither natural nor forced.

Galileo could not bring the solution of the problem of falling bodies to a 'climax' in *De Motu*. The work is also not free of mistakes. Some of them he corrected later, and some remained. It took him almost another three decades of serious thinking to make his most remarkable discoveries, namely, the law of 'free' fall, the principle of relativity, and composition of horizontal and vertical components of motion describing the path of a projectile. In the next section we will complete the path to discovery of the law of free fall, which has been published in the *Dialogues Concerning Two New Sciences*.

8.9 The Discovery of the Law of Free Fall

In the above account from *De Motu* we have interpreted Galileo's thinking pattern as illustrative of inverse reason, guided by the principle of included extremes. This might appear an altogether post hoc study of Galileo's style of reasoning. However Galileo was not only aware that the method he was following involves the synthesis of contraries, but also proposed in a normative tone what the nature of the investigation should be and at what point reason should enter into the investigation. Salviati, the mouth piece of Galileo says on the first day:

> If contraction and expansion [*condensazione e rarefazzione*] consist in contrary motions, *one ought to find for each great expansion a correspondingly large contraction*. But our surprise is increased when, every day, we see enormous expansions taking place almost instantaneously. Think what a tremendous expansion occurs when a small quantity of gunpowder flares up into a vast volume of fire! Think too of the almost limitless expansion of the light which it produces! *Imagine the contraction which would take place if this fire and this light were to reunite*, which, indeed, is not impossible since only a little while ago they were located together in this small space. You will find, upon observation, a thousand such expansions for they are more obvious than contractions since dense matter is more palpable and accessible to our senses. We can see wood and see it go up in fire and light, but we do not see them recombine to form wood; we see fruits and flowers and a thousand other solid bodies dissolve largely into odors, but we do not observe these fragrant atoms coming together to form fragrant solids. *But where the senses fail us reason must step in*; for it will enable us to understand the

> motion involved in the condensation of extremely rarefied and tenuous substances just as clearly as that involved in the expansion and dissolution of solids.[62]

This illustrates his thinking style. If a body is losing weight then there must be that contrary phenomena where the body would gain weight. If a body is sinking in water, then imagine a media other than water where the body would begin to float. If a quantity is successively diminishing in a process, imagine of that quantity which at the same time is increasing. If there is a quantity progressively increasing in one direction, think of another quantity that would progressively increase in the opposite direction. If some thing continuous to remain in a state without variation, it must be in the state of equilibrium where the contraries are invariantly at work. If *by fact* we know one side of the process, we can construct *by reason* the other side of the process. The belief in complete reversibility and symmetry of the world order is the only guiding principle of scientific construction. The models thus created are the models where the contraries coexist or covary, strictly following the principle of included extremes.

Soon after the above passage the discussion is focused on the proof that all bodies fall at the same rate. This counterintuitive discovery is again a result of inverse reason. The first step of the argument is to prove Aristotle's view that "a heavier body does not move more rapidly than a lighter one provided both bodies are of the same material" wrong. This argument is already contained in *De Motu*, which we have presented in §8.3 starting on page 256. There the argument mainly consists in proving that Aristotle's view leads him to contradiction. But Simplicio, who argues for Aristotle in the *Dialogue*, expresses disbelief that "a bird-shot falls as swiftly as a cannon ball",[63] and Sagredo, the third interlocutor, requests Salviati to explain how "a ball of cork moves with the same speed as one of lead".[64] Salviati then describes the method of approaching the result.

> Having once established the falsity of the proposition that one and the same body moving through differently resisting media acquires speeds which are inversely proportional to the resistances of these media, and having also disproved the statement that in the same medium bodies of different weight acquire velocities proportional to their weights ... I then began to *combine these two facts and to consider what would happen if bodies of different weight were placed in media of different resistances; and I found that the differences in speed were greater in those media which were more resistant, that is, less yielding.* This difference was such that two bodies which differed scarcely at all in their speed through air would, in water, fall the one with a speed ten times as great as that of the other. Further,

[62] *Two New Sciences*, p. 60.
[63] *Ibid* p. 64.
[64] *Ibid* p. 68.

> there are bodies which will fall rapidly in air, whereas if placed in water not only will not sink but will remain at rest or will even rise to the top: for it is possible to find some kinds of wood, such as knots and roots, which remain at rest in water but fall rapidly in air.[65]
>
> ...
>
> [I]n a medium of quicksilver, gold not merely sinks to the bottom more rapidly than lead but it is the only substance that will descend at all; all other metals and stones rise to the surface and float. On the other hand the variation of speed in air between balls of gold, lead, copper, porphyry, and other heavy materials is so light that in a fall of 100 cubits a ball of gold would surely not outstrip one of copper by as much as four fingers. Having observed this I came to the conclusion that *in a medium totally devoid of resistance all bodies would fall with the same speed.*[66]

If the differences in speed are more in denser media, then according to inverse reason, the differences in speed will have to be lesser in rarer media like air. And if it is void, a medium with zero density, no difference should be observed. This is the reason that led him to the discovery that in a void, all bodies irrespective of their weight, size or shape, would fall at the same rate.

Since Galileo has no equipment with which to obtain a total vacuum to demonstrate his findings the truth of his claim, therefore he gives *plausibility* arguments.

> Since no medium except one entirely free from air and other bodies, be it ever so tenuous and yielding, can furnish our senses with the evidence we are looking for, and since such a medium is not available, we shall observe what happens in the rarest and least resistant media as compared with what happens in denser and more resistant media. Because if we find as a fact that the variation of speed among bodies of different specific gravities is less and less according as the medium becomes more and more yielding, and if finally in a medium of extreme tenuity, though not a perfect vacuum, we find that, in spite of great diversity of specific gravity [*peso*], the difference in speed is very small and almost inappreciable, then *we are justified in believing it highly probable that in a vacuum all bodies would fall with the same speed.*[67]

The kind of observations in support of his claim suggests that Galileo did not construct idealized systems without taking inputs from experience. This demonstrates his strength which lies in balancing empirical observations with mathematical reasoning. This further supports our earlier claim (§2.1 page 48) that he is neither a Platonist nor an Aristotelian. The data available to him comes from only keen observations, but loaded with

[65] *Ibid,* p. 68.
[66] *Ibid*, p. 72.
[67] *Ibid,* p. 72.

reason, and his experimental observations are not real experiments performed in closed or controlled conditions.

The result is undoubtedly counterintuitive. Therefore experimental proof is necessary.[68] As already mentioned, performing such experiments was practically impossible at that time. Galileo's ingenuity is again at work. If the height of fall is very great the lighter body may be left behind due to the retarding effect of the medium, and if the height is very small then the observation is very difficult. Under such a situation Galileo finds a method of amplifying the difference such that it would become easy for observation.

> It occurred to me therefore to repeat many times the fall through a small height in such a way that I might accumulate all those small intervals of time that elapse between the arrival of the heavy and light bodies respectively at their common terminus, so that this sum makes an interval of time which is not only observable, but easily observable. In order to employ the slowest speeds possible and thus reduce the change which the resisting medium produces upon the simple effect of gravity it occurred to me to allow the bodies to fall along a plane slightly inclined to the horizontal.

The logic is clear enough, but this time he displays it in designing an experimental setup. The time of fall can be delayed without increasing the height of fall by using inclined planes. We all know how, eventually, inclined planes became an excellent device for studying acceleration. The problem with inclined planes is that it offers notable resistance on the moving body.

> I also wished to rid myself of the resistance which might arise from contact of the moving body with the aforesaid inclined plane. Accordingly I took two balls, one of lead and one of cork, the former more than a hundred times heavier than the latter, and suspended them by means of two equal fine threads, each four or five cubits long. Pulling each ball aside from the perpendicular, I let them go at the same instant, and they, falling along the circumferences of circles having these equal strings for semi-diameters, passed beyond the perpendicular and returned along the same path. This free vibration ... repeated a hundred times showed clearly that the heavy body maintains so nearly the period of the light body that neither in a hundred swings nor even in a thousand will the former anticipate the latter by as much as a single moment [*minimo momento*], so perfectly do they keep step. We can also observe the effect of the medium which, by the resistance which it offers to motion, diminishes the vibration of the cork more than that of the lead, but without altering the frequency of either; even when the arc traversed by the cork did not exceed five or six degrees while that of the lead was fifty or sixty, the swings were performed in equal times.[69]

Again the utility of the pendulum in the study of the science of motion hardly requires any further comment. Many discoveries of great significance took place with the help of the simple

[68] *Ibid*, p. 83.
[69] *Ibid*, pp. 84–85.

but elegant tools such as the balance and the pendulum. We will end this chapter with one last quotation to illustrate how Galileo anticipates a conservation principle with the help of the analogy with pendulum, which is nothing but an extended structure rigidly attached to the fulcrum of an invisible balance with a constant swing of the weights.

> [I]t is very likely that a heavy body falling from a height will, on reaching the ground, have acquired just as much momentum as was necessary to carry it to that height; as may be clearly seen in the case of a rather heavy pendulum which, when pulled aside fifty or sixty degrees from the vertical, will acquire precisely that speed and force which are sufficient to carry it to an equal elevation save only that small portion which it loses through friction on the air.[70]

[70] p. 94.

Chapter 9

Inversion and Chemical Revolution

Just as Aristotelian physics was overthrown by the Galilean and Newtonian physics in the 17th century, so eighteenth century developments led to the overthrow of alchemy and Paracelsian beliefs (along with those of the Peripatetics) by the Stahlians (Stahl, Scheele, Cavendish, Priestley etc.) and the so-called Newtonians (Black, Boyle, Lavoisier etc.). In this chapter we will argue first that the chemical revolution consists in the overthrow of the Aristotelian, alchemical, and Paracelsian views by modern experimental chemistry headed by both the Stahlians and Newtonians and not, as often thought to be the case (e.g., by Kuhn).[1], in the overthrow of the phlogiston chemistry, by the Lavoiserian chemistry. Second we will argue that the revolution could not have taken place but for the application of inverse reasoning.

Thus we will first attend to the claim about where the actual focus of attention should be for understanding the revolution. Then we will argue that the inverse processes, which in the case of chemistry are the reversible processes, have been the major clue to the chemical nature of substances, and the discovery of chemical elements. These processes become part of a methodological theme which is better known as the joint method of proof, the method of analysis and synthesis held by both Stahl, who *invented* phlogiston and proposed the phlogiston theory, and Lavoisier, who *discovered* oxygen, and then proposed the oxygen theory of combustion.[2]

Some remarks about why the first thesis needs to be argued before the second. The major thrust of our argument in the present thesis as a whole, is to prove the necessary role of inversion, as a general synthetic methodological theme, in the genesis, development,

[1]Kuhn 1970, *The Structure of Scientific Revolutions,* Ch. VI, p. 188, and p. 120.

[2]The term 'theory' in this chapter is not used in any special sense. For example, by a theory of combustion or acidity etc., we meant it only as an hypothesis proposed to explain a phenomena, and not as a field or a domain of inquiry.

and structure of science. From this point of view, if the chemical revolution consisted in the opposition between the believers of phlogiston theorists and oxygen theorists, we find that both groups applied the joint method, and therefore subscribe to a common methodological theme. However they have been dubbed as alternative world views by Kuhn. In his book *Structure of Scientific Revolutions* he says that the change of vision as a result of the change of conceptual scheme is not only restricted to the examples from astronomy and electricity, but also from the history of chemistry.

> Lavoisier ... saw oxygen where Priestley had seen dephlogisticated air and where others had seen nothing at all. In learning to see oxygen, however, Lavoisier also had to change his view of many other more familiar substances. He had, for example, to see a compound or where Priestley and his contemporaries had seen an elementary earth, and there were other such changes besides. At the very least, as a result of discovering oxygen, Lavoisier saw nature differently.[3]

Therefore the question naturally arises: How can it be that the same methodological theme gave rise to different world views that are said to be incommensurable? If there is some problem in the application of a method, then it must be by rectifying or *systematic application of the method* that the change occurred. If this is not the case the thesis against scientific method becomes strong, for since there is no method, different conceptual schemes may develop without any methodological reasons. Since we contend that there exists a scientific method, we should show that Lavoisier's success, as well as the failure of the phlogiston theorists in isolating and identifying chemical elements, must be explainable in terms of methodological reasons.

Then in the course of the investigation we began to realize that there is a need to distinguish between conceptual schemes and theories. Kuhn evidently uses them synonymously. Kuhn also says that the "distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial." He further says, in the next line: "Its artificiality is an important clue to several of this essay's main theses."[4] We think that since Kuhn confuses conceptual schemes and theories, and as a result facts and theories, he provides the systematically *misleading* thesis that phlogiston chemistry and oxygen chemistry constitute rival paradigms. As a consequence of holding on to the distinction between conceptual schemes and theories, we visualize the possibility of shifting the object of focus from the rivalry between the supporters of phlogiston chemists and oxygen chemists to a much larger and most important issue of understanding the development of modern

[3]Kuhn 1970, *op.cit.* p. 118.
[4]*Ibid* p. 52.

experimental chemistry in its extended historical setting. Questions such as by whom, when, and how oxygen was discovered, are certainly relevant for both historians and philosophers of science. However in the present context, our concern is not to answer in definite terms the specific questions Kuhn raised such as: "Was it Priestley or Lavoisier, if either, who first discovered oxygen? When was oxygen discovered?", but rather to address the methodological question what made the discovery of chemical elements possible?

It is surely the case that the discovery of oxygen became a very important episode in the chemical revolution. However, as Kuhn himself says, "discoveries are not isolated events but extended episodes,"[5] and hence, we say that, the chemical revolution should be studied and the pattern of revolution understood from the over all picture of the *many* discoveries that took place in the hands of post-Paracelsians over an extended period starting from Boyle to Lavoisier.

If the problems are very specific, like when and who discovered something, it is always difficult to judge, specially when there are contenders. In the case of the story of the discovery of oxygen, for example, we learn that both Priestley and Lavoisier committed mistakes.[6] Kuhn expresses the difficulty of settling the problem as follows:

> [A]ny attempt to date the discovery must inevitably be arbitrary because discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is. Note, for example, that if oxygen were dephlogisticated air for us, we should insist without hesitation that Priestley had discovered it, though we would still not know quite when. But if both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery is a process and must take time. Only when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not be of a new sort, can discovering *that* and discovering *what* occur effortlessly, together, and in an instant.

If we look at the conditional nature of the statements Kuhn is making here, it becomes clear that the judgement that we make depends largely on certain decisions we take, and certain beliefs we hold. It may be in relation to substantial questions, like whether some gas is dephlogisticated air or oxygen, or in relation to deeply philosophical questions involving the nature of discovery, like *what*, and *when* shall we call something a discovery. A change in judgement on the conceptual issue would inevitably produce a corresponding change in the judgement on the substantial issue, and vice versa. This *conjugate* relationship between conceptual and substantial issues puts him/her in an eternal trap. If this is the last word regarding the problem, then Kuhn's conclusions regarding the impossibility of communication

[5] *Ibid* p. 52.

[6] J.B. Conant's 1950 pamphlet points out some.

between/across different world views appears correct. If this is not the last word, then we should be able to introduce certain factors that would 'break' the above mentioned conjugate relationship. This, according to our understanding, constitutes the challenge, at least one of the challenges, faced by the post-Kuhnian methodologist.

We think that the challenge can be met, because we see the possibility of a shared 'space' among the alternative conceptions, theories to be precise. By systematically working out from within the shared space it seems possible to judge either way. Having stated the problem this way, it must have become clear why we need to first attend to the problem of finding the common ground between the Stahlians and the so called Newtonians. This explains the motivation in defending the point that Stahlian chemistry could not have been an alternative to Lavoisierian.

9.1 The Problem of Identifying the Rival Paradigms

It is usual to distinguish between the Stahlians and the Newtonians and stretch the matter to such an extent where they start appearing like rivals. The entry of the Stahlians after Boyle and Black has been regarded by many historians as a retrogressive move. For example, Butterfield conjectures that "the emergence of chemistry as a science is remarkably late, that the chemistry of Boyle and Hooke may not have taken the shortest possible route to arrive at Lavoisier, and that the interposition of the phlogistic theory made the transition more difficult rather than more easy"[7] The chapter carries the message "Postponed Scientific Revolution in Chemistry", where he further says:

> The entire view [the theory of phlogiston] was based upon one of those fundamental conclusions of commonsense observation which (like Aristotle's view of motion) may set the whole of men's thinking on the wrong track and block scientific progress for thousands of years.[8]

Mary Boas also comments that there existed a "gulf" from Black to Lavoisier.[9] We think that this view is misleading in grasping the main problem of chemical revolution.

9.1.1 The Problem of Combustion

The major point of difference, as often pointed out, is with regard to the theory of combustion. Before we get into the metatheoretical issue, some introduction to the centrality of the problem of combustion is in order.

[7] Cf. Butterfield 196?, *The Origins Modern Science: 1300–1800*, p. 198.
[8] *Ibid*, p. 194.
[9] Boas 1959, *Robert Boyle and 17th-century Chemistry* p. 229.

> Why was the nature of combustion so important? ... First, because it is the most spectacular and fundamental of all chemical processes. Secondly, it is a process which concerns all four of the Aristotelian elements. A piece of wood burns; *air* is necessary; *fire* is produced; *water* is an important product of the burning; *earth* (ashes) is left. Thirdly, there is the literally 'vital' importance of combustion: for it is a slow and regulated combustion that maintains animal heat in the metabolic processes upon which animal life depends. The alchemists looked upon air as 'the food of fire'; it is equally 'the food of living organism,' both plant and animal.[10]

It is therefore natural for the investigators to have given most of their attention to this problem. Since phlogiston theory has been abandoned in favor of the oxygen theory of combustion, it is natural for historians to comment retrospectively that phlogiston theory retarded the progress of chemistry. But this should not be stretched too far, because though we now say that the hypothesis failed to give a satisfactory account of combustion, it nevertheless was a problem that investigators engaged in. The fact that phlogiston theory was expected to provide an explanation to a specific problem, shows that it belongs to a part of an investigating tradition which pondered over *local problems*, as against global ones. Just as Aristotelian physics properly identified and localized the problems of motion, alchemists and Paracelsians realized the problematic nature of combustion. We are not willing to accept the view held by relativists that even problems change from one paradigm to the other. Our case study on motion demonstrates the point that Galileo did attend to precisely those problems that were formulated by Aristotle. In this case too the problem remained the same, which is to explain the phenomena of combustion and calcination, and of course the general problem of explaining the transformation of chemical substances.

It is usually understood that the process of combustion was the issue over which the Stahlians (consisting of Priestley and a few of his supporters, to be precise) debated for about half a century with the Newtonians (consisting of Lavoisier and his supporters), after the latter proposed an alternative theory of combustion, replacing phlogiston by oxygen. The distinction between the Newtonians and the Stahlians cannot be made as sharply as one could distinguish the new experimental tradition from the old alchemical tradition.

Though it is usual to regard Lavoisier as a Newtonian, it should be borne in mind that *before* Lavoisier discovered oxygen, he was not against the Stahlians. He was rather regarded as one of the French Stahlians.[11]

[10] John Read 1961, *Through alchemy to Chemistry: A Procession of Ideas and Personalities*, p. 119.

[11] P.K. Basu 1992, in 'Similarities and dissimilarities between Joseph Priestley's and Antoine Lavoisier's Chemical Beliefs', cites C.E. Perrin 1988, who argued that Lavoisier was a chemist in the French Stahlian tradition. The difference between the German and French Stahlians is that the former adopted the view that there may be properties that cannot, in principle, be explained by the physicalist-reductionist program; the French followers of Stahl adopted the view that there may be properties that cannot be currently explained

Even Priestley was interpreted as an eighteenth-century mechanist by R.E. Schofield, a biographer of Priestley.[12] These points bring home the fact that the distinction between the Stahlians and the Newtonians is not straightforward, but remains problematic. How can it be that 'rivals' understood to belong to different world views (paradigms) cross their borders and change their identity? We will exploit this uncertainty to defend our thesis, that both Priestley and Lavoisier shared the same world view, *contra* Kuhn.

That characterizing the differences between the Stahlians and Newtonians in the context of chemical science is not an easy matter also becomes clear from the arguments of Basu, who shows in an essay aptly entitled 'Similarities and dissimilarities between Joseph Priestley's and Antoine Lavoisier's chemical beliefs', that there was considerable common ground between Priestley and Lavoisier. Thus Basu:

> They had similar ontological beliefs to the extent that both held Stahlian beliefs with respect to chemical composition and the distinctness of chemical properties. They held chemical reactivity as a distinguishing property to establish chemical distinctness. They also had a common commitment to gravimetric methods for determining chemical composition, although it would be fair to say that Lavoisier employed it more systematically and in analyzing more chemical reactions than did Priestley.[13]

One difference between them, as pointed out by Basu, is that

> Priestley did not accept the view that chemical distinctness between two compounds, which contain the same constituent principles, may depend *only* upon the different proportions in which the different constituent principles are present in each of these compounds.[14]

But, interestingly, on the point where Priestley differed from Lavoisier, he also differed from the other Stahlians, for on this point both Stahlians and Lavoisier agree.[15] Another difference, widely held and also reiterated by Basu, is that Priestley did not employ gravimetric analysis for all the chemical reactions, 'across the board', because he held that not all chemical substances are amenable to the gravitational criterion. On this point Priestley is definitely a non-Newtonian, unlike Lavoisier.

From this at least one point becomes clear, which is that Lavoisier remains undoubtedly a Newtonian, *but at the same time a Stahlian*. That is, he held Newtonian beliefs *over and above Stahlian beliefs*, showing that they are compatible belief systems. Therefore they

by such a program because the theory at its present stage of development is insufficient. (*Ibid.* p.452.)

[12] *Ibid.*

[13] p. 468.

[14] *Ibid.*

[15] *Ibid*, pp. 455, and also p. 468.

do not constitute alternate paradigms as suggested by Kuhn on the basis of phlogiston theory alone. Phlogiston theory may have blinkered the vision of Priestley, and possibly of a few others, but not of the large number of chemists (among them a large number of erstwhile Stahlians) who followed Lavoisier after his theory of oxidation and the discovery of a method of finding out the elemental nature of chemical substances were proposed.

9.1.2 Lavoisier and Priestley on the Joint Method

What is analysis and synthesis? The question appears to be trivial, because who does not know that analysis means decomposing a complex into its components, and likewise that synthesis means composing things into a complex whole. But then, as it appears clear, the answer would depend on what is complex and what is simple. That is if we know whether a substance is complex or simple, then we can tell whether the process is analysis or synthesis. However, to judge substances in these terms is not a trivial matter. The history of science provides ample evidence for this surprising fact. What was considered complex at a period of time, was later considered simple, and this inverse characterization has led to the genesis of alternate theories.

This inversion of terms makes our discussion of the joint method of proof more difficult and confusing. For example Joseph Stahl *analyzed* sulphur into vitriolic acid and phlogiston, and *synthesized* sulphur back from vitriolic acid and phlogiston rich material (charcoal).[16] Today we consider sulphur a simple element, and hence the question of analyzing it further would be impossible. However, the ability to conduct both the processes was considered by Stahl a major discovery, for he held the synthesis as a proof for the composition of the chemical, sulphur.[17] The underlying 'indubitable' belief, as stated by Juncker, a student of Stahl, is:

> A body is composed of the materials from which one forms it and into which it is resolved.[18]

The underlying belief in the method of analysis and synthesis remains an axiom of chemistry founded by Lavoisier. Lavoisier writes in the *Elements of Chemistry* Chapter III:

> Chemistry affords two general methods of determining the constituent principles of bodies, the method of analysis, and that of synthesis ... and in general it ought to be considered as a principle in chemical science, never to rest satisfied without both these species of proofs.

[16]N. Koertge 1980, p. 152.
[17]*Ibid.*
[18]Quoted by N. Koertge, *ibid*, p. 153.

In the context of a discussion regarding the composition of atmospheric air, Lavoisier further says that it is composed of two elastic fluids of different and opposite qualities. The proof for this is given as under:

> As a proof of this important truth, if we recombine these two elastic fluids which we have separately obtained in the above experiment, viz. the 42 cubical inches of mephitic air [nitrogen] with the 8 cubical inches of highly respirable air, we reproduce an air precisely similar to that of the atmosphere, and possessing nearly the same power of supporting combustion and respiration, and of contributing to the calcination of metals.[19]

Here we have Stahl and Lavoisier, both mentioning the usefulness of the joint method of proof by means of analysis and synthesis in finding the chemical nature of substances. This suggests, we think beyond doubt, that on the fundamental question of what constitutes chemical analysis they agree. This remains one of the major similarities between the Stahlians and Newtonians, among others that we shall encounter later. Lavoisier is clearly a Stahlian.

This method helped Stahl in the first place to systematically replace alchemy and Paracelsian science. And in the second place, the same method helped Lavoisier in inverting the usage of terms of Stahl by Lavoisier (analysis to synthesis, and simple to complex). This change ultimately resulted in the gradual elimination of the incorrect theory of phlogiston to explain combustion and calcination. One might raise the question: how can it be that the same method produced one correct theory and another incorrect theory? One of the reasons for this, we think, lies in the fact that phlogiston had been *invented* to explain combustion, and hence remains a supposition. By contrast oxygen, which replaced phlogiston's role entirely, was *discovered* in some sense (possibly unacceptable to Kuhn), and bottled by both the believers and non-believers of phlogiston.

The other more serious point is that the circular link between a property and the corresponding chemical principle could not be broken without bringing in non-chemical properties such as weight, into the scene of investigation. Without a conserved quantity like weight which added additional information toward the simpler and complex nature of the elements, the detection of the error would not have been possible. As indicated by the studies of Lavoisier, it was the increase in weight of the sulphur, phosphorus and metals in calcination, that gave the initial indication of the elemental nature of both metals like mercury, and of the 'eminently respirable air' (oxygen).

However Kuhn claimed that both the theories could explain the 'facts' equally well, arguing that the determining factor is to decide who belongs to what paradigm. It appeared

[19] *Ibid.*

to Kuhn that both conceptual schemes are coherent and meaningful, and hence the choice of the scientists at large has been the major reason or non-reason for the replacement of phlogiston theory. We shall try to show below, however, that phlogiston theory could not have given rise to a paradigm because it lacked certain features that are otherwise common features of paradigms. All attempts to defend the phlogiston theory have failed because of the lack of empirical evidence in which both believers and non-believers of phlogiston believed in.

9.1.3 The Genesis of the Phlogiston Hypothesis

When we look at the context of 'discovery' of phlogiston theory, it becomes clear that the observation of the disappearance of a portion of the burning substance was initially the problem, as well as the 'clue'. Becher (1635-82), who appears to have influenced Stahl in proposing the theory of phlogiston, supposed that all combustible matter *lost* the *terra pinguis*, or combustible principle, during the process of burning.[20] Stahl gave a new name, phlogiston, to this principle of combustion. What has been observed in the burning of wood and charcoal, has been applied, by analogy, to the calcination of metals, the analogy being the involvement of fire in both cases. Substances that burn easily were said to have more phlogiston than others.

Why should absence of something cause concern for the Stahlians or any scientist for that matter? If they did not believe in the conservation of matter, why would one think that the decrease or absence (or the increase) of something needs explanation? Most accounts of the chemical revolution give us the wrong impression that the success of Lavoisier was due to his firm belief in the conservation of matter.[21] Did the alchemists or the Paracelsians not believe in conservation? The notion of transmutation of substances suggests that they certainly believed in the conservation of matter. Thus this could not be regarded as a distinguishing feature of Lavoisier's assumptions.

The ancient Greek inscription associated with Ouroboros, or the tail-eating serpent, specially adopted by the Paracelsians, also suggests that conservation of matter is a universal theme across the chemical paradigms. The inscription says: "One is all, and by it all, and to it all, and if one does not contain all, all is nought."[22] Ancient metaphysicians, whether Atomists or non-Atomists, believed in conservation. If there was one common theme among

[20]Becher believed that all minerals are composed of three constituents, *terra pinguis*, *terra mercurialis*, and *terra lapida*, corresponding to the *tria prima*, the sulphur, mercury, and salt, of the Paracelsus. John Read 1961, *op.cit.* p. 120.

[21]See for example, J.B. Conant 1960, *op.cit.*

[22]Read 1961, *op.cit.* p. 25.

all the Greek physiologues, conservation certainly was that theme. If conservation was such a widely held belief among the philosophers, why has it been projected as the point of difference between Priestley and Lavoisier?

The actual difference, we think, is the experimental proof of the conservation of chemical matter in 'transmutations' (chemical reactions), provided by Lavoisier, and not the general belief in the conservation of matter. We think that it would be impossible to prove that either Priestley or any of the defenders of phlogiston theory did not believe in conservation. Priestley certainly argued against the conservation of weight as an index of a proof for the conservation of matter. It is important also to bear in mind that he did so after Lavoisier proposed his oxygen theory of combustion, and in defense of his phlogiston theory. But this does not suggest that he did not believe in the conservation of matter. The facts are very different, for he was questioning the property of weight as a fundamental property of all physical substances. Since physical substances like light, heat etc., appear not to have weight, why shouldn't we suppose that phlogiston be among those which lacked weight? This is the gist of Priestley's argument in defense of his theory.

It is therefore correct to think that while every chemist believed in the general idea of conservation, it was Lavoisier who specified and verified its truth. He also brought home the point that as far as chemical combinations and separations are concerned, the weight of the chemicals is a sufficient conserved quantitative parameter.[23] Since the presupposition of conservation was involved in the *invention* of the notion of phlogiston, this event is certainly not inexplicable.

If quantitative methods had been the order of the day, then possibly the Stahlians, would have realized the problems that burning may lead to *loss* in some cases, and *gain* in some other cases, of the quantity of substances. But the more important aspect of quantifying the loss and gain of matter is the necessity of conducting the experiment in *closed* conditions. Both Priestley's and Lavoisier's experiments were conducted in controlled conditions. The latter however took extraordinary care to close and isolate the apparatus from the surrounding environment.

Since we know from history that at the time of the genesis of the hypothesis both the quantitative method and the conduct of experiment in closed conditions were not prevailing

[23]Basu, however, thinks that Lavoisier did whatever he did without justification. He argues that since he could not prove the identity or proportionality relation between inertial and gravitational mass, his application of the principle of conservation of weight needs justification. We however think that under laboratory conditions of a chemical experimentation the knowledge of gravitational mass is sufficient. Looking at the theoretical expertize needed to prove the identity between gravitational and inertial mass, it is unfair to demand such a justification from a chemist. We should bear in mind that this was a problem Einstein, and other esteemed physicists were grappling with in the beginning of this century. Cf. Basu, *op.cit.* p. 460ff.

practices, we conclude that the phlogiston theory was invented under circumstances where the scientific methods, in the present sense of the term, were not applied. Therefore despite the belief in the joint method of proof, Stahl and his followers, could not but propose and believe in the (incorrect) theory of phlogiston.

It is also necessary to bear in mind that the involvement of the erstwhile element, air, is a necessary factor for an understanding of the phenomenon of burning as well as that of calcination. This has been another handicap of experimental chemistry that is just beginning to emerge. The first break-through in understanding the complex nature, i.e., non-elemental nature of air was only in the year 1754, when Black discovered an evidently distinctive species of air, namely fixed air. (The phlogiston theory was proposed by Stahl in the year 1697.) The later developments suggests that more adequate experimental methodology was required to achieve an understanding of the nature of air.

The discovery of fixed air involves, interestingly, both the quantitative method of weighing the reactants and products during the reaction, and the joint method of proof realized in the form of reversible reaction with lime, fixed air and chalk. The later discovery of oxygen would not have been possible without the prior discovery of fixed air, because fixed air has a number of contrasting properties with oxygen, making the individuation of oxygen easier. Since this point is supportive of the main thesis this will be elaborated below in a separate section.

Thus it is clear that in the *context of discovery* (or *invention* to be precise) of phlogiston, certain scientific methods were evidently not known and thus could not be applied. It is reasonable to think that it is very unlikely that a phlogiston type theory would have been proposed after the introduction of the quantitative method. The theory evidently appears incompatible with the method the new chemistry was beginning to adopt. Interestingly very bad and weak reasons were put into the fray by the defenders of phlogiston theory in the *context of application*. Since the methods that were needed for the discovery, and the methods necessary to establish were evidently the same, at least in this case, the thesis of generativism stands vindicated.

Since the idea of phlogiston was invented without the aid of scientific methodology, the problem stated earlier, that despite the similarity in the joint method of proof by both the Stahlians and the Newtonians they were led to different theories gets resolved. The joint method as stated by the Stahlians, is not sufficient. And for sufficiency the joint method should be linked to (a) the quantitative method of measuring the parameters involved, (b) an application of the principle of balance sheet, and (c) the need to conduct the chemical processes of analysis and synthesis in a *closed* environment. We will illustrate these points later.

These 'additional' components must not be interpreted as independent methods. They are factors, by the addition of which the former qualitatively stated method of Stahl becomes transformed into the one unitary quantitative and experimental method of Lavoisier. For Lavoisier all these considerations enter at once to become part and parcel of a single method favoring his discoveries.

The methods used, therefore, by Lavoisier are mostly in continuation with the foundations already laid by earlier investigators, and he is working over and above that foundation following a similar method. He of course discovered the need to employ certain other methods as well, such as the taxonomic method and the method of quantitative analysis.

9.1.4 The Stahlians and the Newtonians on the Notion of Element

We have seen that the Stahlians and the Newtonians were grappling with the same problem of finding the principles of chemical behavior. This as we see it was the central problem which led to the chemical revolution, and not merely the problem of phlogistic theory.

The problem was first realized and defined by R. Boyle (1627-91), an exponent of Francis Bacon's inductive system of philosophy. In his celebrated book *The Skeptical Chymist* written in the year 1661, Boyle came out with a set of arguments against the medically minded *iatro-chemists* (Paracelsians) and the gold-seeking alchemists. He questioned the three elements of Paracelsus, the four of Aristotle and the five of the Peripatetics. He proposed that chemistry must be built, if possible, on the identification and the knowledge of those chemical substances, which cannot be separated into different components, by chemical means.[24] A more or less similar pragmatic definition of chemical element is favored by Lavoisier.

> All that can be said upon the number of elements is in my opinion, confined to discussions entirely of a metaphysical nature. The subject only furnishes us with indefinite problems, which may be solved in a thousand different ways, not one of which, in all probability, is consistent with nature. I shall therefore only add upon this subject, that if, by the term *elements*, or *principles of bodies*, to express our idea of the last point which [chemical] analysis is capable of reaching,

[24] Boyle was skeptical of the search for elements. His notion of elements, as well as his skepticism are reflected in the following passage: "And, to prevent mistakes, I must advertize you, that I now mean by elements, as those chymists that speak plainest do by their principles, certain primitive, and simple, or perfecting unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately resolved: now whether there be any one such body to be constantly met with in all, and each, of those that are said to be elemented bodies, is the thing now in question." *The Skeptical Chymist* p. 187.

> we must admit, as elements, all the substances into which we are capable, by any means, to reduce bodies by decomposition. Not that we are entitled to affirm, that these substances we consider as simple may not compound of two, or even of a greater number of principles; but, since these principles cannot be separated, or rather since we have not hitherto discovered the means of separating them, they act with regard to us as simple substances, and we ought never to suppose them compounded until experiment and observation has proved them to be so.[25]

Just as Black and Boyle had done earlier, Lavoisier too insisted on the need to depend on experiment and observation and discouraged the use of *a priori* assumptions in chemical investigations.

We have seen that Stahl's beliefs about Principles of bodies are also based on chemical analysis and synthesis. Stahl adopted some of the criteria proposed by Boyle, such as that the constituents upon combination yield complex bodies, and upon separation yield the 'Principles of bodies'. Stahl defined chemistry as the art of analyzing or resolving mixt, compound, or aggregate bodies into their Principles, and of synthesizing or composing them back from the Principles[26]. This is quite in line with the Newtonians.

Thus, on such a central idea of what should be regarded as an element, the Stahlians and the Newtonians, from Boyle to Lavoisier, had agreement.

9.1.5 Positive Contributions of the Stahlians

The Stahlians and the believers of phlogiston theory continued contributing to the experimental isolation, identification and characterization of many chemicals, though they were not correct in their *identification* of elements. The results obtained from the laboratories of the Stahlians were used without inhibition by non-Stahlians notably Lavoisier, in their chemical investigations.

Henry Cavendish (1731-1810) a distinguished chemist in the Stahlian tradition, made important quantitative studies in chemistry, despite being faithful to phlogiston theory. He investigated the properties of fixed air and inflammable air which he thought was phlogiston; collected gases over water and mercury, an indispensable technique for isolating gases; determined the volumetric composition of water, and also the composition of atmosphere; etc.,

Priestley, despite being a very ardent supporter of the phlogiston theory till his death, contributed enormously and the list of his contributions is huge: He discovered alkaline air (ammonia), acid air (hydrochloric acid), nitrous air (nitric oxide), diminished nitrous

[25] *Elements of Chemistry* 1789.
[26] N. Koertge, 1980, p.153

air (nitrous oxide), nitrous vapors (nitrogen dioxide), dephlogisticated air (oxygen), phlogisticated air (nitrogen), vitriolic acid air (sulphur dioxide) etc. He recognized that green plants restore the goodness of air vitiated by the burning of candles and respiration of animals, and that in the process oxygen is released; found out that water is formed even if the gas is dry; along with Warlitre noticed that water is formed in the explosion of inflammable air with common air or dephlogisticated air.

Scheele (1742-86), who first discovered oxygen (fire air), but published only in 1777, also discovered chlorine; obtained pure hydrochloric acid and silicon fluoride; discovered several organic compounds like tartaric acid, oxalic acid, lactic acid, uric acid; citric acid glycerol, aldehyde etc. He held that when phlogiston combines with oxygen (fire air) it becomes heat.[27]

Thus the believers of phlogiston theory made important contributions, most of them were integrated into the new chemistry founded by Lavoisier, suggesting that discoveries may be compatible despite theoretical differences.

9.1.6 Conceptual Scheme and Theory

While Priestley linked the newly discovered 'eminently respirable gas' to his theory of phlogiston and named it 'dephlogisticated air', Lavoisier named it 'oxygen', linking it with his theory of acidity. Both theories have been proved wrong, while the discovery of a species of gas stands. Isn't it therefore clear enough that it is one thing to discover a gas, and quite another thing to embed the discovery in a theory? The fact that a new species of gas has been isolated and identified by certain individuating descriptions that are common to the believers of different theories, provides sufficient indication to the effect that there exists some invariant aspect. We think that this invariant aspect is the conceptual scheme of a given field of inquiry.

The descriptions used for individuating the gas discovered by Priestley and Lavoisier are similar, such as: that which not only supports combustion but also burns with radiance; that which is purer than the air that we live in, being eminently respirable; that which did not dissolve in water like fixed air; that which did not precipitate lime water like fixed air; that which could be used again for the calcination of metals; did not suffocate animals, rather enabled animals to live longer unlike in the fixed air; that which diminished more than the common air when added to nitrous air. Further Priestley's test for the goodness of air was also employed by Lavoisier.

[27] The listing of the contributions is borrowed from Partington (1960) *A Short History of Chemistry*.

These descriptions, we think, constitute the conceptual scheme, in distinction to the corresponding theories mentioned above. Whether these descriptions are independent of theory or not depends largely on what conception of theory one holds while passing the judgement. However what can be asserted with certainty is the following: whatever notion of theory one holds, the above descriptions constitute the *invariant* observations. Therefore if one calls these descriptions non-theoretical aspects of scientific knowledge, then they both shared them. If these descriptions are construed as theoretical then this portion of theory was shared by both. Any difference traceable should be over and above this invariant base. While these descriptions used are based on observed features of the gases, the theories are based on the *supposed* involvement of the gases, without prior empirical verification.

9.1.7 The Phlogiston Theory is no World-view

The fundamental problem of chemistry at the time of its genesis is, to our mind, to identify the principles of chemical behavior. Combustion or calcination do not exhaust all possible chemical changes. Therefore an incorrect theory of combustion did not halt the study of other chemical changes. We have already seen how the believers in phlogiston theory, such as Scheele, Priestley, Cavendish etc., did make important contributions in isolating a number of distinct chemical substances, including gases. What they could not do was to give a proof that a given isolated substance is an element. Scheele, Priestley and Cavendish had all isolated and bottled gases which were later known to be proper elements. Therefore we cannot say that a theory of combustion is *the* determining factor of chemical revolution. Lavoisier also believed in an incorrect theory of acidity, which was later abandoned. No historian or philosopher would argue that his incorrect theory of acidity halted the progress of chemistry, because Lavoisier did solve a fundamental problem of chemistry by providing a method of proof for the identity of chemical elements. Lavoisier having postulated that oxygen is the principle of acidity did not erect a new world view contrary to the presently accepted chemistry with the modern theory of acidity. Theories of combustion or acidity do not determine a world-view. An altogether different notion of chemical elements could have constituted a world-view. Alchemy in this sense is a world-view, because its practitioners admitted only a few essential 'elements' (principles). Therefore holding different viewpoints on relatively 'small' theories would not amount to holding different world-views. In the above chapter we claimed that Galileo's method of synthesizing contraries into a single system opposes Aristotle's views. The opposition is so radical that one cannot but call them different world-views. This we hold despite our claim that Aristotle and Galileo were attending to the same problems. Even in the case of chemistry the central problem that alchemists and the new experimentalists have is to explain chemical transformations—a problem formulated by the Physiologues as far back as Thales. Thus the rivalry between Priestley and Lavoisier is not fundamental enough to designate it as a rivalry between different world-views.

Not only did phlogiston theory with its various versions, live a short life, the two rival theories of combustion coexisted for a very brief brief of three decades. Stahl published his views on phlogiston in 1697, influenced by the views of Becher (1669). Lavoisier's views on combustion were published in 1783. The coexistence of the two theories in the community of scientists ended with the death of Priestley, which was in the year 1804. Therefore the actual period of conflict was only about two decades long.

This is quite unlike the 'physical' revolution, where Galileo had to fight even the State, the Church, as well as the common beliefs of the society at large. Aristotle's views were deeply entrenched in every aspect of society. This degree of entrenchment is vital to call anything a world-view. Phlogiston theory does not have these essential features of a typical world view. In the context of chemistry, however, alchemical and the Aristotelian views were just as well entrenched in the belief systems of society before the chemical revolution.

The beliefs that are due to a world view seep deep into society and are not just restricted to a few scientists. We are not familiar with any historical account which indicates that phlogiston theory amended the belief system of people at large, i.e., outside the close coterie of the working scientists. On the contrary the belief in alchemy or the Aristotelian theory of four elements has been part and parcel of the world view shared by not just the community of 'scientists', but people at large.

How can we call such a transformation involving a small number of adherents of the theory by an alternate theory a *revolution.* 'World view' or 'paradigm' are much too grand to characterize local explanatory theories such as the theory of phlogiston.

9.1.8 The Real Issue of the Chemical Revolution

The differences between the Stahlians and the Newtonians certainly exist, however, as we shall see, considering the arguments that led to the chemical revolution, they remain non-issues. The major points of difference that have been thought to be significant in understanding the chemical revolution are that the Newtonians were Atomists, that they believed in mechanistic and reductionistic explanations of chemical facts, and furthermore believed in quantitative, specially gravimetric, analyses in chemistry. The belief in conservation has already been shown to be a universal feature and therefore cannot be even considered the point of difference, with the qualification that the Newtonians proved a conservation 'theorem' in the case of chemical changes. We shall consider the other putative points of difference and discuss briefly how they can be shown to be non-issues.

A belief in Atomism is not a necessary condition for the identification of elements. This becomes clear because those that rejected Atomism such as Priestley also discovered and contributed to the *isolation*, if not the *identification* of elements as shown above. Atomic theory as a scientific and empirical thesis came much later, after the consolidation of the chemical revolution. The discoveries in chemistry have been found compatible with the philosophical idea of Atomism. However a belief in the idea is not an essential condition for making discoveries. Thus, a belief in a single, two or any number of elements is compatible with the general metaphysical theories of Atomism. While the chemical revolution could disprove the ancient belief that water, air etc., are elements, and there are more elements than initially thought, it did not prove the existence of atoms. That elements correspond to atoms continues to be a metaphysical belief at the time of the chemical revolution, and the correlation was proved only after the emergence of atomic *theory*, in the sense of a field of inquiry, in the present century. The discovery of elements is therefore independent of the belief in Atomism. However the development of the scientific atomic theory can be shown to be necessarily dependent on the discovery of elements and the knowledge of how and in what proportion they combine. Therefore, although Lavoisier believed in Atomism and Priestley did not, the major reason for Priestley's failure or Lavoisier's success is not due to holding or not holding this belief.

It is true that Boyle, Black, Lavoisier etc., were Newtonians, and hence believed in a mechanistic philosophy of nature. But this philosophy did not explicitly help them in any manner to achieve any reduction of chemical combinations in terms of mechanical principles. The subject of chemical kinetics is in fact a recent development. The philosophical problem of whether any such reduction has ever been effected is a subject of live debate. All that Lavoisier could achieve was a more satisfactory classification, in relation to the ones that are available at that time. He also identified, isolated, and characterized many chemical substances, besides *identifying* many substances as chemically *simple elements* such as oxygen, hydrogen, carbon, nitrogen, and various other metals. In the course of these findings, Lavoisier employed no reasons that can be identified as mechanistic in nature. Quantitative reasoning should not be confused with mechanistic reasoning, and similarly qualitative reasoning should not mean non-mechanistic. We have evidence to the effect that Lavoisier used quantitative reasoning, and indeed it was crucial, but he did not explain the reactions in terms of the mechanistic notion of chemical energy, heat energy etc. Therefore, this difference between Priestley and Lavoisier again should not be viewed as an important factor in analyzing the chemical revolution. It is however a useful notion to understand the intellectual qualities and beliefs of these scientists.

It can be argued that mechanistic philosophy, just as phlogiston theory, retarded the emergence of modern chemistry, because mechanists like Newton and Descartes did not think that there exists a distinct species of *change* other than the physical. One of the first principles of school chemistry today is to understand the fundamental distinction between chemical and physical change.

It may be argued that the major difference between the Stahlians and the Newtonians is based on gravimetric analysis of chemical reactions. Though this, on the face of it, appears like a serious difference, the Stahlians would find no need to oppose gravimetric analysis once the issue of the theory of combustion is set aside. This is because for the supporters of the phlogiston theory gravimetric analysis posed a serious threat. In order to save the phlogiston theory after the alternative has been suggested, they took a stand that physical features like weight cannot determine chemical simplicity.

Priestley's explicit arguments against applying gravimetric analysis to all the chemical substances are present in his work 'On the Phlogistic Theory' dated 1799. Oxygen theory of combustion and calcination was published by Lavoisier in the year 1783. Basu suggests that "it is possible that Priestley's views were close to those of the mechanists during the pre-1790 period." And it is possible that during the post-1790 period (the works written during that period have been analyzed by Basu in his paper) Priestley became a Stahlian. If these observations are correct, then Priestley opposed gravimetric analysis for all chemical reactions, only after the announcement of the oxygen theory of combustion.

The Stahlians were not otherwise opposed to inferring from physical properties of chemical substances like volume and weight, in their identification. We think, therefore, that their opposition to gravimetric analysis in chemistry enters later, just as the oxygen theory of combustion entered into Lavoisier's chemistry only after the discovery of oxygen. That this is so can also become clear from the absence of any resistance to J. Black's discovery of fixed air (carbon dioxide), the first species of a gas to be distinguished from atmosphere, where he uses gravimetric analysis. It is well known that it was Black who introduced the use of balance in the study of chemical reactions. Black adopted Lavoisier's oxygen theory in preference to phlogiston theory, and taught the views of Lavoisier to his students.[28] The identifying

[28] Partington, *op.cit.* p. 94.

properties of fixed air, such as its solubility in water, its reaction with lime, its being noxious to animals, its nature of extinguishing fire, etc., are the results obtained from his experiments, and they have been applied to identify fixed air by Priestley also. Some of them Priestley must have discovered independently. Though Priestley did not employ any gravimetric analysis, he used quantitative reasons (which we shall cite below) to favor his discoveries. Opposition to the quantitative method of chemical investigation, specially gravimetric, did not become an issue till Lavoisier proposed an alternative theory of combustion.

Physical properties such as weight and volume were the major indicators for the absence and the presence of chemical bodies for all the investigators including the Stahlians. Priestley after 'discovering' "dephlogisticated air" went on to *measure* one of its alleged properties, purity of air, which shows beyond doubt that he was very much in favor of quantitative reasoning in chemistry.

> Being now fully satisfied of the superior goodness of this kind of air, I proceeded to measure that degree of purity, with as much accuracy as I could, by the test of nitrous air, and I began with putting one measure of nitrous air to two measures of this air, as if I had been examining common air; and now I observed that the dimunition was evidently greater than common air would have suffered by the same treatment. A second measure of nitrous air reduced it to two thirds of its original quantity, and a third measure to one half. Suspecting that the dimunition could not proceed much farther, I then added only half a measure of nitrous air, by which it was diminished still more; but not much, and another half measure made it more than half of its original quantity; so that, in this case, two measures of this air took more than two measures of nitrous air, and yet remained less than half of what it was. Five measures brought it pretty exactly to its original dimensions.
>
> At the same time, air from the *red precipitate* was diminished in the same proportion as that from *mercurius calcinatus*, five measures of nitrous air being received by two measures of this without any increase of dimensions. Now as common air takes about one half of its bulk of nitrous air, before it begins to receive any addition to its dimensions from more nitrous air, and this air took more than four half-measures before it ceased to be diminished by more nitrous air, and even five half-measures made no addition to its original dimensions, I conclude that it was between four and five times as good as common air. It will be seen that I have since procured air better than this, even between five and six times as good as the best common air that I have ever met with.[29]

Undoubtedly, therefore, Priestley belongs to the quantitative experimentalist tradition.

From these observations we conclude that the similarities outweigh the dissimilarities between the so called rivals, therefore the chemical revolution should not be understood in

[29] Quoted in J.B. Conant's Pamphlet, *op.cit.*

terms of the Stahlians and the non-Stahlians, but in terms of the alchemical/Paracelsian versus scientific chemistry, in the sense explicated by Boyle. Both Stahlians and non-Stahlians are experimentalists; both groups believed in local theories of explanation as against global theories of explanation, i.e., both are non-metaphysical; they used similar devices and the same substances (sometimes even from the same bottle), the same identification criteria. Finally both believed in the joint method of analysis and synthesis.

While Kuhn's observations regarding the revolution in physics are more or less in order with the exception of certain qualifications already expressed in the previous chapter, his observations regarding the chemical revolution, commit a serious mistake of identifying rival paradigms within a more or less *coherent* paradigm of the emerging chemical revolution. We have shown that phlogiston theorists had more similarities with the newly emerging methodological principles of chemistry. Since phlogiston theory has been proposed to account for combustion and calcination of substances, it should not be considered the central determining factor for the identification of rival paradigms.

There exists no reason why Lavoisier should be against the Stahlians prior to the discovery of oxygen in 1783, and his theory of combustion involving oxygen which was postulated in the year 1783. Except for a brief period of resistance by the supporters of phlogiston theory, later chemists forgot that 'wrong' idea altogether. We think that the residue left within the Stahlian chemistry after deleting phlogiston theory, can be accommodated without fundamental conceptual problems within the newly formulated conceptual scheme of Lavoisier. This shows the possibility that with the elimination of this issue no major differences exist between Stahlians and Lavoiserians, and the similarities outweigh the differences. Therefore the two views cannot be characterized as rival paradigms.

9.2 Role of Reversibility in Chemical Revolution

It is generally regarded that in the the major figure responsible for the chemical revolution which took place from 1775-1789 is Lavoisier. However we have seen that Priestley and Lavoisier shared certain beliefs, and we have argued that they do not belong to different traditions/world views, but belong to the rising tradition of experimental chemistry. The question 'naturally' arises: Why did Lavoisier succeed? Though the question is natural to us, who have imbibed the influential views of Kuhn that Priestley and Lavoisier worked in different 'worlds', the question is not necessary. After all not every scientist succeeds in arriving at great ideas even if they all work in the same 'world'. It is not also necessary that every scientist belonging to a 'world' succeed. Why did only Galileo and not Archimedes succeeded in cracking the problem of motion? Why did Newton and not Galileo succeeded in solving the problem of gravitation, though they both worked in the same 'world'? We think that Priestley is just one of those who did not succeed, because he did not use the logic of inversion, despite being a great experimentalist. So in this section we will concentrate on the factors that favored Lavoisier's research methodology, as against Priestley's.

Whether in the context of the chemical revolution or otherwise, scientists have always upheld the arguments which are for (a) a taxonomic order, (b) the conservation of measurable quantities and (c) reversibility of processes. Lavoisier believed, practiced and preached all these three values that are part of scientific rationality. Priestley however, despite being a better experimentalist than Lavoisier, did not take these fundamental values of science seriously. Lavoisier succeeded in establishing conservation of mass during chemical reactions, and reversibility as a proof of the elemental nature of certain chemical substances.

This brings out another significant point of our thesis: It is not sufficient for a scientist to have more empirical data by inductive means. There should be a 'schema' in which the data fit well. Lavoisier being a good systematizer, concentrated more on arriving at a *form* rather than generating more *content*. He very freely used the content generated by the Stahlians. In what follows we shall discuss the most central reasons that ultimately became the methodological standards of chemistry.

9.2.1 The Taxonomic Ideal

The programs of Lavoisier and Priestley were markedly different, a fact which plays a decisive role in the choices made by them. The difference lies in the former's desire to taxonomize and standardize nomenclature. An investigator who attempts to follow the principles of taxonomy adheres to the condition of obtaining mutually exclusive classes, and in that attempt attains the intended clarity needed for scientific investigation. The differences between Priestley and Lavoisier with regard to the general taxonomy of chemical substances is very crucial in making a meta-theoretic judgement, as well as explaining the chemical revolution.

Lavoisier, influenced by the ideals of Condillac started his program by addressing the task of producing a system of chemical nomenclature by reforming the vocabulary of chemical terms. He wanted to build a vocabulary that would indicate the nature of the substance, composition and relationship. The reason why he embarked on the task of building a systematic nomenclature becomes obvious when one notices the sort of names chemical substances had before Lavoisier. Substances went by "fanciful and confusing names like pompholix, colcothar and powder of algaroth; butter of arsenic, flowers of zinc, and martial ethops ... "[30]

Lavoisier's program of naming chemical substances was thus connected to a knowledge of (a) taxonomy of chemical substances, (b) whether a substance is a complex or simple and if complex, its composition. However all these requirements were not immediately available to him from other investigators, so that he could collect, organize and finish his task. His task in fact widened

> While I proposed to myself nothing more than to improve the chemical language, my work transformed itself by degrees, without my being able to prevent it, into a treatise on the Elements of Chemistry.

This initial motivation is crucial, though he did not succeed in arriving at a taxonomy of chemical substances. Though conservation of weight had played an important role in the war against phlogiston, the concern for eliminating indeterminate reference and ambiguity of the term is also highly relevant for explaining the chemical revolution. If the discoveries made do not find a place in a taxonomic system, they would be eventually (if not immediately) be eliminated. After the construction of the Periodic Table of chemical elements, the elements that were identified by Lavoisier had passed the test of taxonomic order, and phlogiston could not find a place in that table. Just as symmetry considerations play decisive role in the construction of scientific concepts, taxonomic systematization plays a decisive role in the acceptance of *natural kinds.* Thus in the ultimate analysis the epistemological role of taxonomy, as a method of discovery as well as a method of justifying knowledge, cannot be replaced by inverse order alone. Since taxonomy is not the immediate concern we will not discuss this further.

9.2.2 The Use of Balance

When some substances disappear, and the cause of disappearance is not known, it becomes a matter of concern for an investigator, or for that matter anybody. The first component of air (atmosphere) to be discovered was fixed air (carbon dioxide), and the story of its discovery by Black clearly demonstrates the point that decrease (absence) in the weight of a substance prompted him to identify the air leaving the substance. This also illustrates the point that discoveries can be made only in a charged problem oriented mood of an investigator.

[30]Cf. Toulmin and Goodfield 1962, *The Architecture of Matter* p. 217.

Joseph Black (1728-1799) in the year 1755, discovered that chalk loses 44% in weight when burnt to produce lime. He explained the loss of weight to the escape of an invisible gas, which he named as fixed air (carbon dioxide). In the process of slaking, lime combines with water to form slaked lime, but in the presence of fixed air it releases the water, recombines with the fixed air and is reconverted into chalk.[31] Chalk turning to lime and lime back to chalk with release and uptake of fixed air is a reversible reaction. In these experiments Black *recovered* the original weight of chalk, shown by his quantitative analysis—the first case where conservation of mass was established in a chemical reaction. The credit of discovering a method of identifying and differentiating a gas different from the atmosphere goes to Black. The first property that has given clues to analyzing chemical properties is the change of colorless lime into white precipitate in the presence of air (fixed air). This white precipitate is the recovered chalk.

Ever since this discovery, the lime test was used to determine the fixed air of atmosphere which makes lime turn milky white. Later Black discovered another identifying property of fixed air that it is deadly to all animals that breathe it. He also discovered certain other sources where fixed air is formed, such as fermentation, breathed-out air, vapour released when charcoal is burned. This discovery can be termed the first breakthrough in modern chemistry, though it did not immediately discredit the then prevalent view that air is an element. Since his conclusions are definite and experimentally demonstrated and non-speculative in nature, they became the basic tools of analyzing other gases of the atmosphere. The properties of fixed air discovered by Black became the 'reference frame' for discovering other chemicals, specially other species of gases.

The amount of carbon dioxide found in the atmosphere is very low compared with other gases like argon, nitrogen, oxygen and hydrogen. Ironically the first gas to be identified and separated as a distinct chemical species was carbon dioxide. Another irony is that though solid and liquid substances are more immediate to experience and more properties of them are delineable, the initial breakthrough in chemistry in identifying and separating elements took place first with gaseous substance.

In a situation where the products in a chemical reaction were known to be of lesser weight than the reactants and an effervescence was also observed indicating the release of an invisible gas, the conclusion that the released gas must account for the loss of weight of the reactants appears to be inevitable. Therefore the most crucial observation in the discovery of fixed air as a component of air is the observation of loss of weight in the formation of

[31] Lowry *Historical Introduction to Chemistry* pp. 50-51.

lime. This observation and the conclusion cannot be coupled together unless the quantitative analysis confirming conservation is available.

If one would investigate the matter by considering the chemical properties of substances alone, without looking at the corresponding changes in physical properties, such as loss or gain of weight/volume, it is impossible to break the circle of properties. That to every visible property there exists a corresponding chemical principle (substance) was a commonly shared belief of all the schools of experimental chemistry at the time. However, such a one-to-one correspondence between a property and the underlying principle is not sufficient to establish the elemental nature of the world. That properties do not stay isolated independent of other properties, and connections reflected in terms of correlations are necessary for the success of chemical investigation, appears to be one of the beliefs of Black. This is reflected in his search for interrelations between chemical phenomena and physical properties. Though physical properties like weight and volume may not inform one about the distinctively chemical properties such as specificity in reactivity, they are sufficient to indicate in definite terms the presence or the absence of chemical substances. Which chemical substance is present or absent, however, cannot be determined by purely physical means. One fundamental assumption however of Newtonian chemists is that all chemical substances have weight.

Priestley believed in a sharp distinction between physical and chemical properties. But did Priestley believe in this dichotomy prior to Lavoisier's announcement of the oxygen theory of combustion? If not, then our earlier claim that even Priestley worked in the same world as that of Lavoisier comes out even more strong, because Priestley's arguments could then be rated as being motivated against Lavoisier. Priestley denied that weight could be considered for understanding chemical simplicity, just on the ground that weight is a non-chemical, physical property. Therefore it *appears* that he excluded one from explaining the other. But, we know from the reactions involving the hypothetical 'phlogiston', which was believed to have negative weight, that a change in weight of chemical substances could take place while undergoing a chemical reaction. Therefore it would be incorrect to say that chemical and physical properties do not affect each other. On this ground we can say that Priestley could not continue keeping the physical and the chemical independent of each other. Independently of bringing in the increase or decrease of weight of a metal upon calcination, how could Priestley account for the 'simplicity' of calx, or the 'complexity' of a metal? Priestley did argue for the simplicity of calx by bringing in the notion of a physical property such as weight.[32] Another difference is that Lavoisier identifies two compounds

[32] Cf. J.B. Conant *op.cit.*

with the same elemental constitution, but in different proportions, as two different chemical substances, while Priestley would regard them as essentially the same. The latter believed this because for him gravimetric data do not reflect chemical simplicity.[33]

Another case to show that Priestley indeed would consider physical factors in his arguments is the volume of substances, which again is not a chemical, and therefore a physical property. A change in volume has been considered in general by all chemists, including Priestley, as an indication of whether a substance entered or left the process. Change of the color of substances is also a physical property of substances, that again became a crucial part of Priestley's test for the purity of air.[34]

9.2.3 Conservation and Reversibility

It is often stated that the application of the principle of conservation of weight helped Lavoisier and all those chemists who followed him. This is indeed a correct observation. However we wish to draw the attention to a more fundamental issue: What makes it possible to apply the principle of conservation? It is the persistent claim of the present essay that inversion, in this case in the form of reversibility, creates a closure, a space, within which the conservation of a quantity can be shown to be possible. In this sense we consider that inversion is a prerequisite for realizing conservation.

In the specific case of the chemical reactions a space for applying the principle of conservation has been created by the reversible reactions. The 'enclosure' created by the reversible reactions is like the hypothetical closed universe where nothing is created nor destroyed. Since it is already assumed, following the general pattern of atomistic thinking, that chemical reactions consist in transformations involving combinations and separations of the basic elements from one form into another, conservation in its *general* scheme of things is already assumed. However a large number of phenomena cannot be closed in a small localized space such that conservation can be realized. A *general and global* metaphysical belief in conservation is different from a *specific and local* realization of conservation. While reversible reactions cannot be physically realized in a large number of cases, there are fortunately some instances where it is possible. It is interesting to see that the chemical revolution hinges on some of these simple reversible reactions.

Thus reversible reactions seemingly played a crucial role in the chemical revolution. Why do reversible processes help in understanding the matter better than unidirectional processes? One of the advantages of reversible reactions, as mentioned above, is the possibility

[33]Cf. Basu 1992, *op.cit.*
[34]Cf. J.B. Conant, *op.cit.*

of closure. Whatever factor is enclosed within the closure would alone be considered for investigation, thus helping the investigator to concentrate on the changes taking place in a small 'universe'. Though the question of ultimate simplicity (i.e., elemental nature) may remain unsettled the question can be answered relative to the process in clear terms because of reversibility. Unidirectional processes are open, because one can never say in certain terms what factors actually cause them. Therefore to begin with non-reversible reactions could not give clues to the chemical elements in definite terms. However after isolating and distinguishing the taxonomic properties of certain substances, continued dependency on reversible reactions in each and every case is not necessary, because the established knowledge can be applied in the unknown cases. Evidence for our claim are the crucial discoveries made by both Black and Lavoisier. In reversible reactions the causes and effects can be inverted in the sense that the cause of a forward reaction becomes the effect of the backward reaction, and vice versa. Therefore, even if finer resolution of causes may not be possible, the factors responsible for the process can be identified determinately. Those factors that are necessary but remain constant or common in both directions of the reaction however remain undisclosed. The role of such factors can be investigated by experimenting under controlled conditions.

Initial experiments by analysis suggests to the experimenter that a substance contains, say A and B, as constituents. But it does not say that A and B are the only constituents. This confirmation can be obtained by reversing the reaction if possible under controlled conditions, and quantitative analysis. This method of proof (§9.1.2 page 288) has become a regular method in chemistry.

Lavoisier's method of analysis and synthesis has been demonstrated by separating 7 to 8 cubic inches of air from 50 cubic inches of air, by boiling mercury in air under controlled conditions.[35] This portion being eminently respirable and combustible was identified as oxygen, while the remaining portion, 42 to 43 cubic inches, being found unfit for both respiration and combustion was identified as mephitic air (nitrogen). Upon recombining them he reproduced an air similar to that of atmosphere, all the properties being restored. This is the proof by synthesis.[36]

Another instance where a similar method of proof was applied is in proving that water is a compound of oxygen and hydrogen. Henry Cavendish (1731-1810) obtained hy-

[35] Cf. J.B. Conant, p. 50.

[36] Today we do not use the term 'synthesis' or 'combination' for the mixture of gases such as oxygen and nitrogen, because we distinguish between a mixture and a chemical compound. It was only after two decades of careful experimentation applying the principle of balance sheet, that scientists acquired a method of showing the difference between compound and mixture. Compounds are obtained by combination (chemical) of elements in a definite proportion, while mixtures are obtained by 'mixing' of elements in any proportion without involving any chemical change. (J.B. Conant, *op.cit.* p. 52.)

drogen by the action of acids on metals such as zinc, iron and tin.[37] Lavoisier repeated his experiments and found that water was formed when hydrogen was burned in air. Earlier experiments on burning various substances showed him that it involves the combination of oxygen with the substance. Therefore, he concluded that when it burns in air, hydrogen forms water by combining with oxygen. He predicted that hydrogen can be obtained back from water if the oxygen could be removed. He at once proceeded to conduct an experiment in which he passed hot steam over a hot gun barrel (made of iron) and obtained hydrogen and calx (iron oxide). Thus while synthesis (recombining) proved the elemental nature of oxygen in the earlier experiments with oxygen and mercury, analysis of a compound into its constituents proved the complex nature of the compound, in this case water. Therefore both converse methods, analysis and synthesis, can be employed as methods of proof depending on the objective of whether one is investigating the simplicity or complexity of any chemical substance.

The chemical revolution gains importance because it altered our earlier view that earth, water, and air are elements. It was only the Atomists of the past who proposed that all of the four basic elements are composed of indestructible atoms, and the differences in a physical property such as the density of these atoms would explain the transformations. The significant achievement, therefore, of the chemical revolution is that, apart from the changes that take place on the physical front, there is at least one another kind of change, called chemical. Variation in density, therefore, is not a sufficient explanation, as held by the ancient Atomists, for the problem of transformations and variety of the substances. It is in this period (the 18th century) that a number of metals, that were extracted in pure form even in the ancient times, were identified as elements; water was analyzed into its constituents, oxygen and hydrogen; air was analyzed into carbon dioxide, oxygen, nitrogen, hydrogen; diamond, graphite, charcoal are all discovered to be different forms of carbon; etc. Above all, the methods of chemical investigation became well established.

The case of the chemical revolution demonstrates that experimental and empirical evidence alone are insufficient to bring out scientific development. Also the significance of a discovery cannot be judged by local applicability alone, but rather by how a specific discovery finds connections with the established canons of knowledge not only within the domain of inquiry but also outside. In the present case the connection with Atomism and the growing Newtonian mechanistic world-view certainly played a crucial role for the acceptance of Lavoisier's chemistry.

[37]Lowry 1936, *Historical Introduction to Chemistry* p. 66.

Conclusion

We had set three objectives—one general and two specific—to this work as stated in the synopsis (page iii) The general objective was to clearly indicate the possibility of an epistemology based on an analytico-synthetic methodological framework. In order to meet that objective we have made a preliminary attempt, which we believe demonstrates that a logic of the construction of meaningful and applicable scientific ideas is indeed possible. We have proposed an alternative view of generativism which is epistemologically significant. We have envisaged the possibility of articulating a logic of constructing scientific ideas, that would generate systems having epistemologically desirable values such as equilibrium and symmetry. In this connection we have argued that generativism and the semantic view of scientific knowledge would mutually reinforce each other to form a strong epistemological framework. This framework would be analytico-synthetic, incorporating the envisaged constructive logic based on inversion and the analysis based on deductive logic. Since the instruments of deductive analysis are well established, we have concentrated mainly on the problem of giving a methodological character to synthetic logic. Some preliminary results in this direction are clearly visible, but, needless to say, more work needs to be done to claim anything called success. This sense of incompleteness is, needless to say, overpowering. But if we are pressed to say what have we achieved in the work, our clear reply would be: we could identify a problem as well as an idea so that it can be pursued with single mindedness for some years to come. In what follows we shall present (1) the reasons for our optimism, and (2) the nature of the work that needs to be done for further strengthening the proposed thesis.

The thesis claims that there exists the possibility of articulating an ampliative logic, which is neither inductive nor deductive in nature. It is visualized that such a logic can be based on the *principle of included extremes*. The principle would act as a guide in the construction of conceptual structures which are meaningful and applicable. Since the construction begins from known 'facts', it is impossible that the obtained concepts lack any application, and therefore the visualized logic is epistemologically relevant. Most importantly, since the potential of inversion in constructing possible states-of-affairs is great, it is specially relevant in accessing the observationally inaccessible and 'theoretically' postulated aspects of scientific knowledge. This approach if pursued further might lead to a better understanding of the relation between 'facts' and 'theories', as well as the role and the status of 'theoretical' entities in science.

We have shown that meaning, symmetry, equilibrium etc., have epistemological significance, because postulated theories that have these properties are seldom rejected by later empirical methods of testing. Since truth is not the only epistemological value scientists care for, epistemologists should reconsider the traditional view that the appraisal of a scientific theory should be carried out only on the basis of whether it is verifiable or falsifiable. If our argument and approach are correct the position would be epistemologically unique, because this would be the strongest possible position of generativism ever proposed. Though Nickles argued that the strongest possible justification of scientific knowledge is available from generative methods of justification, he expressed the opinion, following Peirce, that an ampliative logic cannot be valid. As argued in Chapter 4, generativists should argue for broadening the notion of validity to include inferences that preserve values other than truth. And this view applies also to induction, which, we have argued, is based on the *principle of excluded extremes*. Induction is valid because: first, it should be viewed primarily as an inference involving concepts and therefore as an ampliative method of abstraction; and second, it is impossible to generate gibberish by inductive means and therefore it is meaning preserving.

It is also impossible that by inverse reason one arrives at meaningless constructs, and hence inverse reason is also meaning preserving. Apart from preserving meaning, its strong point is its constructive potential in generating scientific knowledge of a highly abstract kind. Apart from the considerations of meaning, other desirable values of structures such as equilibrium and symmetry are impossible without inversion. There is therefore a necessary connection between these features of science and inversion.

It is too early to demand or even to claim anything more than showing the possibility and plausibility of the proposed alternative epistemological position. We have not formulated all the rules of inverse logic, nor have we proved the validity of the inference. More work needs to be carried out in this connection. However, here we wish to share the optimism that such a rigorous formulation is indeed possible. In fact, we suspect that most of the rules of inverse inference are already well known, and well entrenched in scientific practice; all that we need to do is to excavate them from the body of scientific knowledge in which they are embedded. Here are some examples.

Modern science is inconceivable without algebra, an essential tool of mathematical science. Modern algebra is formal, rigorous and content neutral, in the sense that the operations and the structures built by employing its methods require specific interpretation in the context of application in order to make sense of the form. One of the fundamental principles of inverse reason would be based on the 'principle of balance': when we add or subtract anything to both sides of the balance, the equilibrium of the system remains unaffected. We have an analogous principle in algebra which allows us to solve algebraic equations of various degrees of complexity. By adding, subtracting, multiplying or dividing an equation on both the LHS and the RHS of an equation we solve and simplify equations, and without this method there is nothing like algebra. This transformation of an equation is as fundamental to algebra as the rule of detachment is to deductive logic. Which is the principle of deductive logic from which this principle of balance can be deduced? It is usually proved as a theorem from other axioms, commonly called the *law of cancellation* in formal systems of arithmetic. Terms in an equation can be moved from LHS to RHS or vice versa by using this law for solving algebraic equations. This principle we claim is the heart of inverse reason, and since it is self evident should be considered as one of the principles of constructive reason, rather than as a theorem derivable from other axioms that are more remote and less self-evident than this. The formal proofs are also not very direct, and hence far from being satisfactory.[2] There are many other principles, such as

$$(\forall x)(\exists y)(x + y = 0),$$

which are fundamental to inverse reason.[3] This is again another principle of equilibrium systems. All the terms in an equation add up to a 'zero', precisely because additive inverses cancel out to yield a 'null'. A similar principle also exists for multiplicative inverses with 'unity' as the identity. Whether it is the principle of inertia of dynamics, or mathematical principles of the kind just mentioned, they must be *analogous* to a principle of balance. We invariably notice that such principles become the foundations upon which the respective inquiries are built. Therefore there is no reason why we should hesitate to generalize this as a clear foundation for a constructive reason based on inversion and indeed an epistemologically relevant one.

There are other branches of mathematics, such as vector algebra, in which we see an in built principle of inverse reason. Vector algebra is an elegant method that simplifies the

[2]Cf. Suppes 1957, *Introduction to Logic* pp. 134–35, for a formal as well as an informal proof of the law for addition. A similar law for multiplication is proved on pp. 148–149.

[3]This is introduced as axiom (8) of the fifteen in the Suppes's text, *op.cit.* p. 129.

method of 'discovering' invariant measures of magnitudes. Most quantities can be measured in more than one manner, and with reference to different 'frames'. We therefore get different numbers when we use different ways of measurement. By employing the method of vector algebra we can obtain an invariant value ('number') of the magnitude. In order to obtain an invariant value of a vector magnitude **a**, **a** will be analyzed into three[4] components a_x, a_y, a_z corresponding to the three values in a Cartesian coordinate system. Important point to note is that there exists a special relation between the three numbers a_x, a_y and a_z, which may better be stated in the words of Feynman:

> In order for it to be a vector, not only must there be three numbers, but these must be associated with a coordinate system in such a way that if we turn the coordinate system, the three numbers "revolve" on each other, get "mixed up" in each other, by the precise laws [of vector algebra].[5]

Thus the methods employed in vector algebra are vital in obtaining a definite invariant description of measurable dimensions. Since the connection between invariance, symmetry, and inversion, is more or less necessary, we are hopeful of proving eventually the stated interconnections.

With regard to Calculus the point is much more interesting due to the discovery potential of the method. Integrability of differential equations, that are usually obtained as a result of experimental work, is undoubtedly what a scientist always desires to achieve. The question that should interest us is: Why do they aspire for integrability? An obvious answer would be, because if such a method be available the problem solving in that field of inquiry would become more or less analytic (automatic). Very soon such a field of inquiry would reach a phase of research, which Kuhn would call normal science. That the two inverse methodological transformations, differentiation and integration, are vital to mathematical physics hardly requires detailed argument. However, what needs to be explored is to excavate from the body of calculus the rules that could enrich a logic of discovery based on inverse reason. This excavation is necessary because in the present form the synthetic rules of mathematics are not explicitly stated. On the contrary it is claimed by many that mathematics is a paradigm case of analytic reason.

Very little needs to be said about the usefulness of group theory in understanding the properties of various kinds of symmetries possible in nature, and its relevance to the present proposal, because here our argument would be straight forward. Still less needs to be said about the necessity of inversion in group theory. Group theory has already become a 'tool'

[4]The vector however could be analyzed in any number of dimensions.

[5]Feynman Lectures, *op.cit.* Chapter 11, p. 6.

for discovering theoretical entities especially after Dirac's famous discovery of the positron. It will not be incorrect to say that without the involvement of group theory there could be nothing like a field called particle physics. Again what needs to be explored in this context is to search for an inferential pattern that must have enabled and would enable such discoveries. Why is group theory essential in these fields of inquiry? What is the logically crucial inference that becomes a part of the group theoretic method? It is high time philosophers of science considered these methodological questions seriously. That a beginning has already been made in this direction can be clearly seen from van Fraassen's work on symmetry.

Thus we see that in the general 'body' of mathematics there are many principles of a constructive nature that had better be separated out to see the in-built synthetic nature of the discipline. We intend to take up this problem for further investigation to excavate possible principles of inverse reason from the rules usually employed in mathematics.

Any thesis on inversion would be incomplete without the mention of Piaget, who has already made use of the notion of groups in the development of his 'genetic' theory of intellectual development. Inversion is central to his thought, where equilibrium and reversibility are the prime factors determining the structures developed at various stages of development starting from childhood to adulthood and even to geniushood. We share Piaget's optimism regarding the potential of group theory.

> Groups are today the foundation of algebra. The range and fruitfulness of the notion are extraordinary. We run into it in practically every area of mathematics and logic. It is already being used in an important way in physics, and very likely the day will come when it acquires a central role in biology as well. Clearly, then, we should try to understand the reasons for the immense success of the group concept.[6]

Piaget also specified that the nature of abstraction involved in group construction is qualitatively distinct from induction. Thus he says:

> The primary reason for the success of the group concept is the peculiar—mathematical or logical—form of abstraction by which it is obtained; an account of its formation goes far to explain the group concept's wide range of applicability. When a property is arrived at by abstraction in the ordinary sense of the word, "drawn out" from things which have the property, it does, of course, tell us something about these things, but the more general the property, the thinner and less useful it usually is. Now the group concept or property is obtained, not by this sort of abstraction, but by a mode of thought characteristic of modern mathematics and logic—"reflective abstraction"—which does not derive properties from *things* but from our way of *acting on things,* the operations we perform

[6]Piaget 1969, *Structuralism,* pp. 18–19.

> on them; perhaps, rather, from the various fundamental ways of *coordinating* such acts or operations—"uniting," "ordering," "placing in one-one correspondence," and so on.[7]

A significant aspect of Piaget's contribution is the role of actions/transformations/operations etc.,. He has already demonstrated their potential in generating quantitative concepts, as against the qualitative, in a number of quasi-empirical studies as a part of genetic epistemology. This is another source of optimism about the applicability of the notion of inversion in epistemology. As was made clear in the main text, we are envisaging precisely this possibility of a logic that has this character of "reflective abstraction" (or Weyl's "creative abstraction"), as based on the principle of included extremes. This to the best of our understanding is the novel aspect of our proposal, i.e., to base the constructive logic on a principle that is independent from that of deductive logic. This is in fact the major point of difference between us and the other generative epistemologists, including Piaget. Though Piaget made use of the relation inversion or reversibility in giving shape to his structuralism, he thought that group theory can also be based on the principles of rationality. He thought that the internal coherence of group theory emerges from the principles of rationality. He says that inversion (reversibility) and associativity are the "restrictive conditions". As a result of these conditions "group structure makes for a certain coherence" governed by an internal logic of a self-regulating system. So far there is no problem. But he continues by saying:

> This self-regulation is really the continual application of three of the basic principles of rationalism: the principle of non-contradiction, which is incarnate in the reversibility of transformations; the principle of identity, which is guaranteed by the permanence of the identity element; and the principle, less frequently cited but just as fundamental, according which the end result is independent of the route taken.[8]

What is this "principle of non-contradiction" that Piaget has in mind? If the two other principles with which he associated is any indication, then this principle should not be the usual principle of non-contradiction. Another source of problem is that he has not distinguished the two operations negation and inversion in the manner we have in this thesis. He says that:

> the characteristic of structures belonging to the algebraic family is that "reversibility" takes the form of "inversion" or "negation" . . . .[9]

If these are any indications of the 'confusion' prevailing regarding the two different species of opposition, then we have stated a point of great significance by distinguishing clearly the

[7] *Ibid,* p. 19.
[8] *Ibid,* p. 20.
[9] *Ibid,* p. 24.

two species of opposites: one of them, negation, as a species responsible for the division of opposites by following the principle of non-contradiction, and the other, inversion, responsible for combining opposites by following the principle of included extremes.

But on the whole Piaget's insightful observations on the development of intelligence based on equilibrium and reversibility have been a constant source of encouragement.

	IDEAS that is	
DESIGNATIONS or TERMS or CONCEPTS		STATEMENTS or PROPOSITIONS or THEORIES
	may be formulated in	
WORDS		ASSERTIONS
	which may be	
MEANINGFUL		TRUE
	and their	
MEANING		TRUTH
	may be reduced, by way of	
DEFINITIONS		DERIVATIONS
	to that of	
UNDEFINED CONCEPTS		PRIMITIVE PROPOSITIONS
	the attempt to establish (rather than reduce) by these means their	
MEANING		TRUTH
	leads to an infinite regress	

On the Analytico-synthetic Epistemology: We have already harped enough on the two complementary aspects of reason that logic and epistemology should embody. Our main proposal in this regard is to *logically* distinguish the two modes of thought. By analysis we meant the assertive or postulational (or statemental) deductive mode of thought, and by synthesis the nonassertive or conceptual (or non-statemental) abstractive mode of thought. Further, analysis is non-ampliative, while synthesis is ampliative. The distinction is only logical, and therefore not to be understood as independent thought *processes* whenever and wherever they take place, i.e., not as spatio-temporally independent. The table which is reproduced from Popper's *Conjectures and Refutations*[10] represents the the two complementary aspects of reason.

In the course of the development of scientific knowledge, we have seen frequent shifts from the synthetic phase to an analytic phase. Before Euclid's axiomatization of geometry

[10] p. 19.

the phase of mathematics was mostly synthetic, but after him we observe a long period of the analytic phase of Euclidian geometry. During the seventeenth and eighteenth centuries when algebraic methods were beginning to influence mathematics in a major way, analytical rigor seemed immaterial. But soon in the nineteenth century mathematics returned to the classical ideal of the deductive-postulational (axiomatic) method. Are these shifts necessary for the growth of scientific knowledge? The evidence that they are necessary is strong in the case of mathematics. Courant and Robbins emphasize the complementary roles of analytic and synthetic phases, with a warning regarding the prevailing over-emphasis on the deductive-postulational character of mathematics.

> True, the element of constructive invention, of directing and motivating intuition, is apt to elude a simple philosophical formulation; but it remains the core of any mathematical achievement, even in the most abstract fields. If the crystallized deductive form is the goal, intuition and construction are at least the driving forces. A serious threat to the very life of science is implied in the assertion that mathematics is nothing but a system of conclusions drawn from definitions and postulates that must be consistent but otherwise may be created by the free will of the mathematician. If this description were accurate, mathematics could not attract any intelligent person. It would be a game with definitions, rules and syllogisms, without motive or goal. The notion that the intellect can create meaningful postulational systems at its whim is a responsibility to the organic whole, only guided by intrinsic necessity, can the free mind achieve results of scientific value.[11]

The view that mathematics, or even that of the whole of scientific knowledge, is analytic naturally promotes, in fact did promote, the impression that knowledge is unchanging. The recent developments in the history of philosophy clearly changed the view that natural science is not immune to changes and falsifications, for good. However we think that this change of view seems to have affected only our views about natural science. We strongly think that we need to change our views about mathematics, and even about logic. It is still widely held that mathematics and logic are embodiments of analytic reason. The history of ideas has enough evidence to prove this prevailing traditional understanding incorrect. No water tight distinction between the formal sciences and the natural sciences can be maintained. The analytic and synthetic modes of thought cut across this traditional distinction in the sense that just as there are clear synthetic modes in mathematics and logic, there are clear analytic modes in natural science. What is modern about modern logic without incorporating the principles constructed by the constructive discipline of algebra? The same can be said of modern mathematics. Therefore we think that there is a greater need to rethink our

[11] Courant and Robins, *What is Mathematics?* p. xvii.

traditional dichotomies.

E.T. Bell has expressed rather evocatively the point that mathematics is not immune to change. He says that

> mathematics is not the static and lumpish graven image of chargeless perfection that some adoring worshipers have proclaimed it to be.[12]

And he further says that

> it is idle to pretend that what was good enough for our father in mathematics is good enough for us, or to insist that what satisfies our generation must satisfy the next.[13]

This comment we think should be extended to logic too. There is no reason why we should continue to think that valid reason is that which preserves only truth. The history of ideas provides strong evidence to the fact that in the course of the development of scientific knowledge, truth has become more and more *localized*. It is more or less a 'logical truth' that meaning has greater scope than truth, for the 'set' of possibilities is greater than the 'set' of actualities. Truth whenever and where ever it is realized must be after all a subset of the large set of constructible possible worlds. Therefore, there is more sense in looking for a method of constructing possible worlds than restricting our search for a 'limited' truth. If this is really the situation then how can it be that a logic of constructing meaning is epistemologically irrelevant? There is sufficient reason, therefore, to direct our energy towards articulating a logic that is valid by virtue of its capacity to preserve meaning. Truth anyway is only local. This epistemological inversion from truth to meaning is vital to the version of analytico-synthetic generativist epistemology that we are advancing.

Possible Future work: Our objective here is not to give an exhaustive list of things that we intend to take up in future. What we intend to list here are some of the most urgent things that we need to take up which would further strengthen the analytico-synthetic epistemology. Here we shall mention only those that have not been mentioned above.

- The role played by analogies, metaphors, and models in the context of discovery is well known. After all balance and pendulum are models. Is there any internal structure to all the models, and analogies that science has been employing all along? How central is inversion in constructing those analogies?

[12] *Development of Mathematics op.cit.* p. 171.
[13] *Ibid,* p. 172.

- We have made the observation that inverse systematization and taxonomic systematization show mutually opposite tendencies. We have seen how central inversion is in realizing invariant patterns of transformations. But scientists also employ another method of systematizing proliferating variety, which we think is achieved by the method of taxonomy. We could not present the generative potential of taxonomy, though in the course of the research much time and thought was spent on this issue. By showing the interrelationships between these two methodological themes, the two faces of science—the discovery of more and more natural kinds and the invention of highly general universalizing dimensions—can be clearly brought out.

- There are a number of episodes, as suggested in Chapter-6, in the development of science that are amenable to a reconstruction based on inverse reason. This is not to suggest that the case studies presented in the thesis are in a complete form that require no further enrichment. We have considered in the present work only the case of the *genesis* of an idea. The role of inverse reason in the *development* of scientific knowledge also requires to be demonstrated, specially in the case of natural sciences, since there is at least one clear case of the development of number theory in mathematics. In this connection the relation of correspondence between two successive theories in a given domain of inquiry and inversion requires to be investigated. If this can be achieved then the similarity in the patterns of development of mathematics and natural science can become more than a mere analogy.

- Inversion appears to be also prevalent in various social theories. Special reference should be given to economic theories. Almost from the word 'go', economics is fashioned on the idea of balance. Whether it is the capitalist models based on profit and loss, or socialist models based on surplus value, whether it is the econometric input-output analysis, … it is the model of equilibrium that animates entire economic thought. Fruitful reconstruction of economic thought is therefore possible.

Appendix A

Groups

Groups are abstract mathematical structures characterized by an operation satisfying certain conditions. A structure $< G\circ >$ is a group iff

1. $\circ$ is an operation on set G and
2. $\circ$ is associative, and
3. G has a unique identity element, and
4. each element of G has unique inverse element.

An identity element is defined with respect to a given operation. If $\circ$ is an operation on a set G, then e is the identity element with respect to $\circ$ iff

$$(\forall x)[(x \in G)((x \circ e = x) \wedge (e \circ x = x))]$$

For example for the operation addition, $+$, 0 is the identity element, and for the operation multiplication, $\times$, 1 is the identity element. Inverse elements are defined as follows: Let x and y be any elements of a set G with operation $\circ$ and identity element e. The y is inverse of x iff

$$(x \circ y = e) \wedge (y \circ x = e)$$

For example $(-2 + 2 = 0)$ where 0 is the identity element for the operation $+$. $(2 \times 1/2 = 1)$ where 1 is the identity element for the operation multiplication. Given these definitions we can show that the set of integers $\mathcal{Z}$ with the operation $+$ will form a structure $< \mathcal{Z}, + >$ which is a group, because

1. for every $x, y \in \mathcal{Z}((x + y) \in \mathcal{Z})$, which means that the set $\mathcal{Z}$ is closed under the operation $+$;

2. for every $x, y, z \in \mathcal{Z}(x + (y + z)) = ((x + y) + z)$, i.e. $+$ is associative;

3. there exists an element $0 \in \mathcal{Z}$, such that for any $x \in \mathcal{Z}(x + 0) = (0 + x) = 0$, i.e., 0 is the identity element with respect to $+$; and

4. for any $x \in \mathcal{Z}$ there exists an element $-x \in \mathcal{Z}$ such that $x + (-x) = (-x) + x = 0$, i.e., $-x$ is inverse of x with respect to $+$.

The set of integers $\mathcal{Z}$ under the operation multiplication $\times$, does not form a group because for any element $x \in \mathcal{Z}$, x, may not have $1/x \in \mathcal{Z}$.

Appendix B

The Model of Equilibrium in Population Genetics

There are a large number of areas in biology where mathematics, specially statistics, is used in order to make sense of the data in terms of certain correlations. While this is surely a way of finding an application of mathematics, there exists another more interesting way of applying mathematics, which is by building models of explanations helping to understand the dynamics of certain systems. It is well known that Mendel used statistics to understand the varying patterns of inheritance of certain selected characters from one generation to another, ultimately leading to the formulation of what we today call the Mendel's laws. One of the conceptions developed by him is the conception of *dominant* and *recessive* genes ('factors' to be true to Mendel's description), the two influencing 'forces' of a character. While the conception proved to be very useful in understanding the genetics of all diploid organisms (organisms with a pair of genes for every character), it generated a problem for the evolutionists who tried to apply genetics to the process of evolution.

The mechanism of heredity and variation, is basic to the study of evolution and both evolutionists and geneticists in the beginning the century realized this.[1] For the geneticists the mechanisms of inheritance in both individual and population, constitute the problems of study. But the evolutionists are mostly concerned about the latter problem, because evolution has to do with the changes in populations, rather than in the individuals.

Population genetics deals with the *dynamics* of the genes in a population. Although genes exist within individuals the fate of genes as well as individuals is actually linked to the over all profile of the genetic constitution of the population.[2]

[1] Sturtevant 1965, *A History of Genetics* p. 107.

[2] 'Population' is generally defined by geneticists as a group of sexually interbreeding or potentially inter-

The genetic characterization of a population is generally carried out by measuring the gene frequency. Gene frequency refers to the proportions of the different alleles of a gene in a given population.[3] It is estimated by counting the total number of organisms with various genotypes in a given population.

Since there is a similarity between the genetic relationship between a parent and its offspring on the one hand and a generation and its subsequent generation on the other, just as an offspring's genetic characters are determined by its parent's genes, the gene frequency of the subsequent generation will be determined by the gene frequency of the older generation. Thus in populations, one could say, gene frequencies, rather than genes are inherited.[4]

Early studies on population genetics started by studying the application of Mendel's laws on populations of interbreeding individuals. The problem for population genetics can be stated as follows: A small proportion of recessive features always exist in populations without getting eliminated completely by the dominant features. For example, in the human population, brown eyes are dominant over the blue, curly hair is dominant over straight hair, etc. The proportion of frequency of these dominant to recessive features remains constant, contrary to the common belief that the proportion of dominant features should increase over time. Why doesn't the dominant supplant the recessive?

The sort of problem that would interest an evolutionist, however, is different, though both their problems get resolved ultimately by the same explanatory model. At the time of Darwin, the hereditary material was believed to be a part of the blood, the child receives a solution or an alloy of hereditary substance of the parents. One consequence of the blood theory is that as a result of sexual reproduction any genetic variability tends to level off over a period of time. This created a problem for Darwin, who admitted it, because for evolution to occur variation was believed to be a necessary condition.[5] To be consistent with the theory of evolution Darwin proposed that the hereditary mechanisms should at least be able to *conserve* the already available variations, if not increase the profile of variations. For speciation to take place both conservation of variation and occurrence of new variations are found necessary. Therefore, understanding the dynamics of genes in a population became a major problem of investigation by the evolutionists and geneticists alike in the beginning of the present century.

The early works of Yule in 1902, Castle in 1903, and Pearson in 1904 indicated that

breeding individuals.

[3]A pair of genes that are alternative to each other in heredity and are located at the same locus on the homologous chromosomes are called alleles.

[4]Cf. Strickberger 1990, *Genetics*. p. 670.

[5]T. Dobzhansky 1955, *Evolution, Genetics and Man* p. 117.

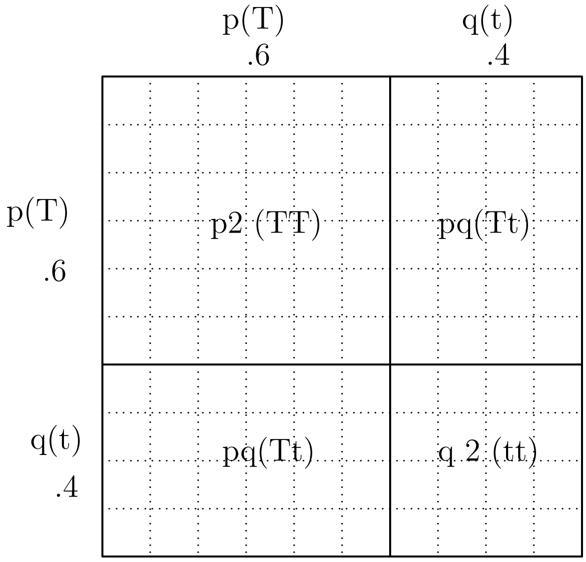


Figure B.1: Genotypic frequencies generated under conditions of random mating for two alleles, T and t

the proportion of the dominant and recessive genes in a population tends to stabilize, i.e., sum of frequency of occurrence of dominant genes p (TT) and the frequency of occurrence of recessive genes q (tt), remains a conserved or constant: $p + q = 1$.[6] Later, in 1908, Hardy and Weinberg, independently formulated the *law of genetic equilibrium* in a generalized mathematical form by expanding the binomial $(p + q)^2$:

$$p^2(TT) + 2pq(Tt) + q^2(tt).$$

The significance of the law lies in postulating a state of 'inertia' (equilibrium). According to the law, gene frequencies and genotype ratios in large biparental populations reach an equilibrium in one generation and remain constant there after *unless disturbed* by new mutations, selection, migration, genetic drift etc. The statement clearly is analogous to the Newton's first law. The disturbing factors mentioned are essential for speciation (evolution). The imbalance introduced by the disturbing factors will be balanced eventually. This model of explanation is characteristic of dynamic systems in most parts of natural science. The analogy with dynamic systems comes more clearly in the following description of the law by a famous geneticist, Dobzhansky:

> The Hardy-Weinberg theorem describes the *statics* [our mentioning] of a Mendelian population. If all gene frequencies in all populations remained con-

[6] Sturtevant 1961, *op.cit.* pp. 107-108.

> stant, evolution would not take place. Evolution may be defined in a most general way as a *change in gene frequencies*.[7]

The disturbing factors mentioned above are regarded as the dynamic forces.[8]

The Hardy-Weinberg law, however, is strictly valid only if several conditions are fulfilled: (1) The population must be large, (2) there must be no mutation, (3) no selective mating (i.e., there should be free interbreeding), (4) no selection, and (5) no migration. These conditions, though, look like limitations regarding the applicability of the law, however, they are indirectly stipulating the conditions to be met for evolution to take place. Briefly put: upsetting the genetic equilibrium is necessary condition of evolution.

Further developments in the field depended, therefore, on the mathematical (algebraic) analysis of the effects of deviations from the above mentioned conditions. Haldane in 1924, Fisher in 1928, Wright in 1932 etc., have made studies that are largely mathematical based on laboratory experiments, and not on naturally occurring wild populations, with the exception of the work of Dobzhansky in 1937, whose work is on the laboratory populations[9]

According to Paul Thompson the case of population genetics "has been atypical within biology in its use of mathematical descriptions"[10] He argues that though biological systems are complex, they can be 'tamed' only by the use of mathematical descriptions of the dynamics of the systems. While it is easy to appreciate this, the methodological questions remain to be answered. First, what is in the method of mathematics that 'tames' complexity? Secondly, what situations can be rendered to mathematical analysis. It is the claim of our thesis that wherever opposite 'forces', dominant and recessive genes in the above case, can be obtained, it allows the possibility of defining a state of equilibrium, a neither-nor-state. Since this definition by theoretical construction refers to an *idealized state of affairs*, it can be employed to characterize *indirectly* the *actual state of affairs*, by contrast or comparison. The ideal state becomes the *invariant model*, and the actual state becomes the *variant case*. In science we find that this pattern of explanation is a recurring feature in almost all the areas of scientific knowledge. In the above case we have seen how the variant case of evolution is explained, though partially, by the use of an invariant model of equilibrium. In all such cases the application of mathematics has been non-trivial, in the sense that without its use there exists no hope of understanding the pattern in the complex and variable phenomena.

[7] Dobzhansky *op.cit.* pp. 118–119. Italics original.

[8] *Ibid,* p 119.

[9] Cf. Sturtevant 1961, *op.cit.* pp. 108–111.

[10] Paul Thompson 1992, "Mathematics in the Biological Sciences" in *International Studies in the Philosophy of Science* p. 242.

Bibliography

[1] Peter Achinstein. 1968, *Concepts of Science.* The John Hopkins Press, Maryland.

[2] Prajit K. Basu. 1992, Similarities and dissimilarities between joseph priestley's and antoine lavoisier's chemical beliefs. *Studies in History and Philosophy of Science*, 23:445–469.

[3] Henry Beilin. 1971, The development of physical concepts. In Theodore Mischel, editor, *Cognitive Development and Epistemology.* Academic Press, New York.

[4] E.T. Bell. 1945, *Development of Mathematics.* McGraw-Hill, New York, second edition.

[5] E.T. Bell. 1961, *Men of Mathematics.* Simon and Schuster, New York.

[6] Nuel Jr. Belnap. 1963, *An Analysis of Questions: Preliminary Report.* Santa Monika, California.

[7] E. Beth. 1961, Semantics of physical theories. In H. Freudenthal, editor, *The Concept of the Role of Model in Mathematics and Natural and Social Sciences*, pages 48–51. D. Reidel Publishing Company, Dordrecht.

[8] Max Black, editor. 1954, *Problems of Analysis.* Cornell University Press, New York.

[9] Richard Blackwell. 1969, *Discovery in the Physical Sciences.* University of Notre Dame Press, Notre Dame.

[10] Mary Boas. 1959, *Robert Boyle and Seventeenth-century Chemistry.* Cambridge University Press, Cambridge.

[11] Salomon Bochner. 1966, *The Role of Mathematics in the Rise of Science.* Princeton University Press, Princeton.

[12] Robert Boyle. 1661, *The Skeptical Chymist.* E.P.Dutton & Co. Inc., New York. Reprinted by Dutton in 1949.

[13] Mario Bunge. 1967, *Scientific Research I: The Search for System.* Springer-Verlag, New York.

[14] Arthur Burks. 1946, Peirce's theory of abduction. *Philosophy of Science*, XIII:301–306.

[15] M.F. Burnyeat. 1977, Examples in epistemology: Socrates, theaetetus and g.e. moore. *Philosophy*, 52.

[16] H. Butterfield. 1962, *The Origins of Modern Science: 1300–1800.* G. Bell and Sons Ltd., London.

[17] Robert E. Butts (ed.). 1978, *New Perspectives on Galileo*, volume 14 of *University of Western Ontario Series in Philosophy of Science.* Reidel Publishers.

[18] Rudolf Carnap. 1966, *Philosophical Foundations of Physics.* Basic Books, New York.

[19] James D. Carney and Richard K. Scheer. 1980, *Fundamentals of Logic.* Macmillan Publishing Co. Inc., New York, second edition.

[20] Ernst Cassirer. 1923, *Substance and Function and Einstein's Theory of Relativity.* Open Court, Chicago.

[21] Peter Caws. 1965, *The Philosophy of Science: A Systematic Account.* D. van Nostrand Company Inc.

[22] A.F. Chalmers. 1976, *What is this thing called Science?: An Assessment of the Nature and status of science and its methods.* The Open University Press, Milton Keynes.

[23] Marshall Clagett, editor. 1959, *Critical Problems in the History of Science.* The University of Wisconsin Press, Madison.

[24] J. Alberto Coffa. 1991, *The Semantic Tradition from Kant to Carnap.* Cambridge University Press, Cambridge.

[25] Morris R. Cohen and Ernst Nagel. 1968, *An Introduction to Logic and Scientific Method.* Allied Publishers, N. Delhi.

[26] M.R. Cohen and I.E. Drabkin, editors. 1958, *A Source Book in Greek Science.* Harward University Press, Cambridge, Massachusetts.

[27] J.B. Conant. 1960, *Overthrow of the Phlogiston Theory: The Chemical Revolution of 1775-1789.* Harward University Press, Cambridge.

[28] J.R. Conant. 1951, *Science and Common Sense.* Yale University Press, New Haven, Connecticut.

[29] F. M. Cornford. 1935, *Plato's Theory of Knowledge: The Theaetetus and the Sophist of Plato.* Routledge and Kegan Paul, London. Translated with a running commentary.

[30] Courant and Robins. 1960, *What is Mathematics?* Oxford University Press, London.

[31] Donald Davidson. 1974, On the very idea of conceptual scheme. *Proceedings and Addresses of the American Philosophical Association*, (67):5–20.

[32] Richard Dedekind. 1901, *Essays on the Theory of Numbers.* Dover Publications, New York. Translated by W.W. Berman 1963.

[33] E.J. Dijksterhuis. 1959. In Marshall Clagett, editor, *Critical Problems in the History of Science.* The University of Wisconsin Press, Madison.

[34] E.J. Dijksterhuis. 1961, *The Mechanization of the World Picture.* Oxford University Press, London.

[35] Theodosius Dobzhansky. 1955, *Evolution, Genetics and Man.* John Wiley & Sons, Inc., New York.

[36] Stillman Drake. 1957, *Discoveries and Opinions of Galileo.* Doubleday and Company Inc., New York. Translated with Introduction.

[37] Rene Dugas. 1955, *A History of Mechanics.* Editions Du Griffon, Neuchatel-Switzerland. Translated by Maddox, J.R.

[38] Pierre Duhem. 1954, *Aim and Structure of Phisical Theory.* Princeton University Press, Princeton.

[39] C.H. Jr. Edwards. 1979, *The Historical Development of the Calculus.* Springer-Verlag, New York.

[40] David Elkind. 1969, Conservation and concept formation. In David Elkind and John H. Flavell, editors, *Studies in Cognitive Development: Essays in Honour of Jean Piaget.* Oxford University Press, New York.

[41] Gareth Evans and John McDowell, editors. 1976, *Truth and Meaning: Essays in Semantics.* Clarendon Press, Oxford.

[42] K.T. Fann. 1970, *Peirce's Theory of Abduction*. Martinus Nijhoff, Hague.

[43] Paul Feyeraband. 1977, Changing patterns of reconstruction. *British Journal for the Philosophy of Science*, 28:351–382.

[44] Paul Feyeraband. 1978, *Against Method: An Out line of an Anarchistic Theory of Knowledge*. Verso Edition, London.

[45] Feyerabend. 1981, *Problems of Empiricism: Philosophical Papers*. Oxford University Press, Cambridge.

[46] R. P. Feynman. 1965, *The Character of Physical Law*. MIT Press, Cambridge.

[47] R.P. Feynman, Robert B. Leighton, and Mathew Sands. 1963, *The Feynman Lectures on Physics*. Addison Wesley, Reading. In 3 volumes.

[48] H. Freudenthal. 1961, *The Concept of the Role of Model in Mathematics and Natural and Social Sciences*. D. Reidel Publishing Company, Dordrecht.

[49] George Gale. 1979, *Theory of Science: An introduction to the History*. McGraw-Hill Book Company, New York.

[50] Galilei Galileo. 159?, *De Motu*. The University of Wisconsin Press, Madison. Unpublished work, written by Galileo around 1590s, Translated by Drabkin, I.E. 1960.

[51] Galilei Galileo. 1632, *Dialogues Concerning the Two Chief World Systems*. University of California Press, Los Angeles. Translated by Stillman Drake 1962.

[52] Galilei Galileo. 1636, *Dialogues Concerning Two New Sciences*. Dover Publications, Inc., New York. Translated by Henry Crew and Alfenso De Salvio, 1914.

[53] Neal W. Gilbert. 1960, *Renaissance Concepts of Method*. Columbia University Press, New York.

[54] Neal W. Gilbert. 1963, Galileo and the school of padua. *Journal of History of Philosophy*, 1:223–31.

[55] Marjorie Grene. 1963, *A Portrait of Aristotle*. Faber and Faber, London.

[56] Mirko Drazen Grmek, Robert S Cohen, and Guido (Eds.) Cimini. 1981, *On Scientific Discovery: Erice Lectures 1977*. Reidel, Dordrecht.

[57] N. Gulley. 1962, *Plato's Theory of Knowledge*. Methuen & Company Ltd., London.

[58] Ian Hacking, editor. 1981, *Scientific Revolutions.* Oxford University Press, Oxford.

[59] Ian Hacking. 1983, *Representing and Intervening.* Cambridge University Press, Cambridge.

[60] Ian Hacking. 1983, *Representing and Intervening.* Cambridge University Press, Cambridge.

[61] Jacques Hadamard. 1945, *An Essay on the Psychology of Invention in the Mathematical Field.* Dover Publications, New York.

[62] Norwood Russell Hanson. 1958, The logic of discovery. *Journal of Philosophy*, 55:1073–1089.

[63] Norwood Russell Hanson. 1958, *Patterns Of Scientific Discovery.* Cambridge University Press, London.

[64] Norwood Russell Hanson. 1965, Number theory and physical analogy. In *Boston Studies in the Philosophy of Science Vol. 2: In Honour of Philip Frank.* Humanities Press, New York.

[65] Sophie Haroutunian. 1983, *Equilibrium in the Balance: A Study of Psychological Explanation.* Springer-Verlag, New York.

[66] Rom Harrè. 1970, *The Principles of Scientific Thinking.* Macmillan, New York.

[67] T. L. Heath. 1956, *The Thirteen Books of Euclid's Elements.* Dover Publishers, New York. Edited and Translated by Heath.

[68] T.L. Heath. 1897, *The Works of Archimedes.* Dover Publications, Inc., New York. Translated.

[69] Carl G. Hempel. 1952, *Fundamentals of Concept Formation in Empirical Science.* Chicago University Press, Chicago.

[70] Carl G. Hempel. 1966, *Philosophy of Natural Science.* Prentice-Hall, Inc., New Jersey.

[71] Carl G. Hempel. 1970, On the 'standard concption' of scientific theories. In M. Radner and S. Winokur, editors, *Minnesota Studies in Philosophy of Science*, pages 142–163. Minnesota University Press, Minneapolis.

[72] M. Hesse. 1966, *Models and Analogies in Science.* Norte Dame Press, Norte Dame.

[73] M. Hesse. 1974, *Structure of Scientific Inference.* Macmillan, London.

[74] Cleveland P. et.al. Hickman. 1979, *Integrated Principles of Zoology.* The C.V. Mosby Company, St. Louis.

[75] Gerald Holton. 1978, *The Scientific Imagination: Case Studies.* Cambridge University Press, Cambridge.

[76] Gerald Holton and D.H.D. Roller. 1958, *Foundations of Modern Physical Science.* Addison-Wesley, Cambridge.

[77] Terence Irwin. 1988, *Aristotle's First Principles.* Clarendon Press, Oxford.

[78] Max Jammer. 1967, Energy. In Paul Edwards, editor, *The Encyclopedia of Philosophy*, pages 511–517. The Macmillan Company and Free Press, New York.

[79] Nicholas Jardine. 1976, Galileo's road to truth and the demonstrative regress. *Studies in History and Philosophy of Science.*

[80] W. Stanley Jevons. 1958, *The Principles of Science: A Treatise on Logic and Scientific Method.* Dover Publications, New York.

[81] Benjamin Jowett. 1952, *The Dialogues of Plato.* Encyclopedia Britannica, Inc, Chicago. Translation.

[82] Bernard Kaplan. 1971, Genetic psychology, genetic epistemology, and theory of knowledge. In Theodor Mischel, editor, *Cognitive Development and Epistemology*, pages 31–61. Academic Press, New York.

[83] Felix Kaufmann. 1978, *The Infinite in Mathematics.* D. Reidel Publishing Company, Dordrecht. Edited by Brian McGuinness.

[84] K. Kelly. 1987, The logic of discovery. *Philosophy of Science*, 54:435–452.

[85] William Kneale and Martha Kneale. 1962, *The Development of Logic.* Clarendon Press, Oxford.

[86] Noretta Koertge. 1980, Analysis as a mathod of discovery during the scientific revolution. In T. Nickles, editor, *Scientific Discovery, Logic, and Rationality*, pages pp. 139–159. Reidel Publishing Inc., Dordrecht.

[87] Stephan Korner. 1959, *Conceptual Thinking: A Logical Inquiry.* Dover Publications Inc., New York.

[88] W. Krajewski. 1977, *Correspondence Principle and Growth of Science.* Reidel Publishing Company, Dordrecht.

[89] Saul Kripke. 1971, Identity and necessity. In Ted Honderich and Myles Burnyeat, editors, *Philosophy as it is*, page 567ff. New York University, New York. The collection published 1979.

[90] Saul Kripke. 1972, Naming and necessity. In Donald Davidson and Gilbert Harman, editors, *Semantics of Natural Language.* Reidel Publishing Inc., Dordrecht.

[91] Thomas Kuhn. 1977, *Essential Tension.* California University Press, California.

[92] Thomas S. Kuhn. 1970, *The Structure of Scientific Revolutions.* University of Chicago Press, Chicago, second edition.

[93] Thomas S. Kuhn. 1977, On theory-change as structure change: Comments on the sneed formalism. In Robert E. Butts and Jaakko Hintikka, editors, *Historical and Philosophical Dimensions of Logic, Methodology and Philosophy of Science*, volume 12 of *The University of Western Ontario Series in Philosophy of Science*, pages 289–309. D. Reidel Publishing Company, Dordrecht.

[94] Henry E. Jr. Kyburg. 1984, *Theory and Measurement.* Cambridge University Press, Cambridge.

[95] Imre Lakatos. 1976, *Proofs and Refutations: The Logic of Mathematical Discovery.* Cambridge University Press, Cambridge.

[96] Imre Lakatos. 1978, *Mathematics, Science and Epistemology*, volume 2. Cambridge University Press, Cambridge.

[97] Imre Lakatos and Alan Musgrave, editors. 1970, *Criticism and Growth of Knowledge.* Cambridge University Press, Cambridge.

[98] D. Lamb and Easton S.M. 1984, *Multiple Discovery: The Pattern of Scientific Progress.* Avebury Publishing Company, England.

[99] Pat Langley, Herbert Simon, Gary L. Bradshaw, and Jan M Zytkow. 1987, *Scientific Discovery: Computational Explorations of the Creative Proceses.* The MIT Press, Cambridge.

[100] Larry Laudan. 1977, *Progress and Its Problems.* Routledge and Kegan Paul, London.

[101] Larry Laudan. 1980, Why was the logic of discovery abandoned? In Thomas Nickles, editor, *Scientific Discovery, Logic, and Rationality*, pages 181–191. D. Reidel Publishing Company, Dordrecht. Reprinted with changes in Laudan 1981.

[102] Larry Laudan. 1981, *Science and Hypothesis.* Routledge and Kegan Paul, Dordrecht.

[103] Antoine-Laurent Lavoisier. 1790, *Elements of Chemistry.* Dover Publications, Inc., New York. Translated by Robert Kerr, Dover Edition 1965.

[104] T.M. Lowry. 1936, *Historical Introduction to Chemistry.* Macmillan, London.

[105] Michael E. Malone. 1993, Kuhn reconstructed: Incommensurability without relativism. *Studies in History and Philosophy of Science*, 24(1):69–93.

[106] Peggy Marchi. 1980, The method of analysis in mathematics. In T. Nickles, editor, *Scientific Discovery, Logic, and Rationality*, pages pp. 159–179. Reidel Publishing Inc., Dordrecht.

[107] S.F. Mason. 1956, *Main Currents of Scientific Thought: A History of the Sciences.* Abelard Schuman, New York.

[108] Robert McLaughlin. 1982, Invention and induction: Laudan, simon, and the logic of discovery. *Philosophy of Science*, 49:198–211.

[109] G. Nagarjuna. 1993, Greek thematic pairs and the origins of the method of analysis and synthesis. *Journal of Foundational Research*, I:99–125.

[110] Ernst Nagel. 1961, *The Structure of Science: Problems in the Logic of Explanation.* Routledge and Kegan Paul, London.

[111] Thomas Nickles. 1984, Positive science and discoverability. *PSA 1984*, 1:13–27.

[112] Thomas Nickles. 1985, Beyond divorce: Current status of the discovery debate. *Philosophy of Science*, 52:177–206.

[113] Thomas Nickles. 1990, Discovery logics. *Philosophica*, pages 901–926.

[114] Thomas Nickles (ed.). 1980, *Scientific Discovery, Logic, and Rationality.* D. Reidel Publishing Company, Dordrecht.

[115] J.R. Partington. 1960, *A Short History of Chemistry.* Macmilan & Co Ltd., London.

[116] David Pearce. 1981, Is there any theoretical justification for a nonstatement view of theories? *Synthese*, 46:1–39.

[117] Charles Sanders Peirce. 1960, *Collected Papers of Charles Sanders Peirce.* Belknap Press, Harward.

[118] Charles Saunders Peirce. 1985. In Carolyn Eisele, editor, *Historical Perspectives on Peirce's Logic of Science: A History of Science Part-I.* Mouton Publishers, Berlin.

[119] Marcello Pera. 1981, Inductive method and scientific discovery. In Grmek et.al., editor, *On Scientific Discovery: Erice Lectures 1977.* Reidel, Dordrecht.

[120] C.E. Perrin. 1988, Research traditions, lavoisier, and the chemical revolution. *Osiris*, 4:53–81. This work, cited in Basu 1992, could not be consulted due to non-availability.

[121] Jean Piaget. 1950, *The Psychology of Intelligence.* Routledge and Kegan Paul, London.

[122] Jean Piaget. 1968, *Structuralism.* Routledge and Kegan Paul, London. Translated and edited by Chaninah Maschler.

[123] Jean Piaget. 1970, *Genetic Epistemology.* Columbia University Press, New York. Translated by E. Duckworth.

[124] Karl Popper. 1959, *The Logic of Scientific Discovery.* Hutchinson, London.

[125] Karl Popper. 1963, *Conjectures and Refutations: The Growth of Scientific Knowledge.* Routledge and Kegan Paul, London.

[126] Karl Popper. 1972, *Objective Knowledge.* Clarendon Press, Oxford.

[127] Hilary Putnam. Analytic and synthetic. In H. Feigl and G. Maxwell, editors, *Minnesota Studies in Philosophy of Science, Vol III*, pages 358–397. University of Minnesota Press, Minneapolis.

[128] Hilary Putnam. 1962, What theories are not? In E. Nagel, P. Suppes, and A. Tarski, editors, *Logic, Methodology and Philosophy of Science.* Stanford University Press, Stanford.

[129] Hilary Putnam. 1979, *Mathematics, Matter and Method.* Cambridge University Press, Cambridge.

[130] Quine. 1977, Natural kinds. In Stephen P. Schwartz, editor, *Naming Necessity and Natural Kinds*, pages 155–175. Cornel University Press, Ithaca.

[131] W.V.O. Quine. 1953, *From A Logical Point Of View.* Harward University Press, Massachusetts.

[132] F.P. Ramsey. 1931, *The Foundations of Mathematics and Other Logical Essays.* Kegan Paul, London.

[133] J.H. Jr. Randall. 1960, *Aristotle.* Columbia University Press, New York.

[134] J.H. Jr. Randall. 1964, *Career of Philosophy: From the Middle Ages to the Enlightenment.* Columbia University Press, New York.

[135] John Read. 1961, *Through Alchemy to Chemistry: A Procession of Ideas & Personalities.* G. Bell and Sons, Ltd., London.

[136] Hans Reichenbach. 1938, *Experience and Prediction.* University of Chicago Press, Chicago.

[137] Alexander Rosenberg. 1985, *The Structure of Biological Science.* Cambridge University Press, Cambridge.

[138] W.D. (ed.) Ross. 1908–52, *The Works of Aristotle.* Oxford University Press, London.

[139] Gerasomos Xenophon Santas. 1979, *Socrates: Philosophy in Plato's Early Dialogues.* Routledge and Kegan Paul, London.

[140] R.E. Schofield. 1964, Joseph priestley, the theory of oxidation and the nature of matter. *Journal of the History of Ideas*, 25:285–294. This work, cited in Basu 1992, could not be consulted due to non-availability.

[141] Dudley Shapere. 1964, The structure of scientific revolutions. *Philosophical Review*, 73:383–394.

[142] Dudley Shapere. 1977, The structure of scientific theories. pages 518–599. University of Illinois Press, Illinois, second edition. Also reprinted in Shapere 1984.

[143] Dudley Shapere. 1984, *Reason and the Search for Knowledge: Investigations in the Philosophy of Science*, volume 78 of *Boston Studies in the Philosophy of Science.* D. Reidel Publishing Company, Dordrecht, Holland.

[144] Herbert Simon. 1977, *Models of Discovery and Other Topics in the Methods of Science*. Reidel, Dordrecht, Holland.

[145] Aron Sloman. 1978, *The Computer Revolution in Philosophy: Philosophy and Models and Mind*. Harvester Press, Brighton.

[146] J.D. Sneed. 1971, *The Logical Structure of Mathematical Physics*. D. Reidel Publishing Company, Dordrecht.

[147] Wolfgang Stegmueller. 1973, Structure and dynamics of scientific theories: Some reflections on j.d. sneed and t.s. kuhn. *Erkentnis*, 9:75–100.

[148] Wolfgang Stegmueller. 1976, *The Structure and Dynamics of Theories*. Springer Verlag.

[149] Wolfgang Stegmueller. 1979, *The Structuralist View of Theories: A Possible Analogue of Bourbaki Structures*. Springer Verlag.

[150] Grice & Strawson. 1956, In defense of a dogma. *Philosophical Review*, 65:141–158.

[151] M. W. Strickberger. 1990, *Genetics*. New York: Macmillan Publishing Company, third edition.

[152] A. H. Sturtevant. 1965, *A History of Genetics*. New York: Harper & Row, New York.

[153] Frederick Suppe, editor. 1977, *The Structure of Scientific Theories*. University of Illinois Press, Illinois, second edition. Edited with a critical introduction and an afterword by Suppe.

[154] Patrick Suppes. 1957, *Introduction to Logic*. Litton Educational Publishing, New York.

[155] Patrick Suppes. 1965, What is a scientific theory. In F. Suppe, editor, *Philosophy Reprint Collection*, volume XIV. Illinois.

[156] Patrick Suppes. 1969, A comparison of the meaning and uses of models in mathematics and the empirical sciences. In *Studies in the Methodology and Foundations of Science*. D. Reidel Publishing Company, Dordrecht.

[157] A. Tarski. 1944, The semantic conception of truth and the foundations of semantics. *Philosophy and Phenomenological Research*, 4.

[158] A. Tarski. 1961, *Introduction to Logic and to the Methodology of Deductive Sciences*. Oxford University Press, New York.

[159] Paul Thagard. 1988, *Computational Philosophy of Science.* MIT, Cambridge.

[160] Frank Thilly. 1951, *History of Philosophy.* Central Book Depot, Allahabad. Revised by Ledges Wood.

[161] Paul Thompson. 1992, Mathematics in the biological sciences. *International Studies in the Philosophy of Science*, 6(3):241–248.

[162] Stephen Toulmin. 1953, *The Philosophy of Science.* Hutchinson University Library, London.

[163] Stephen Toulmin. 1971, The concept of stages in physical development. New York: Academic Press.

[164] Stephen Toulmin. 1972, *Human Understanding.* Clarendon Press, Oxford.

[165] Stephen Toulmin. 1972, Rationality and scientific discovery. *PSA 1972.*

[166] Stephen Toulmin and June Goodfield. 1962, *The Architecture of Matter.* New York: Harper & Row.

[167] Bas C. van Fraassen. 1970, On the extension of beth's semantics of physical theories. *Philosophy of Science*, 37:325–339.

[168] Bas C. van Fraassen. 1980, *The Scientific Image.* Clarendon Press, Oxford.

[169] Bas C. van Fraassen. 1989, *Laws and Symmetry.* Oxford University Press, Oxford.

[170] Barbara Von Eckardt. 1993, *What is Cognitive Science?* The MIT Press, Cambridge, Massachusetts.

[171] Marx W. Wartofsky. 1968, *Conceptual Foundations of Scientific Thought: An Introduction to the Philosophy Of Science.* Macmillan Company, London.

[172] Herman Weyl. 1949, *Philosophy of Mathematics and Natural Science.* Princeton: Princeton University Press.

[173] Herman Weyl. 1952, *Symmetry.* Princeton University Press, Princeton.

[174] Lancelot Law Whyte. 1960, *Essay on Atomism: From Democritus to 1960.* Wesleyan University Press.

[175] Eugene P. Wigner. 1964, Symmetry and conservation laws. *Physics Today*, 17(3):34–40.

[176] Eugene P. Wigner. 1967, *Symmetries and Reflections.* Indiana University Press, Indiana.

[177] Bernard Williams. 1978, *Descartes: The Project of Pure Enquiry.* Pelican Books.

[178] Ludwig Wittgenstein. 1953, *Philosophical Investigations.* New York: The Macmillan Company, New York.

[179] Ludwig Wittgenstein. 1958, *The Blue and Brown Books.* Basil Blackwell, Oxford.

Index